The Minimum Legal Drinking Age and Crime Victimization

Aaron Chalfin[1], Benjamin Hansen*[2] and Rachel Ryley[3]

[1]Department of Criminology, University of Pennsylvania
[2]Department of Economics, University of Oregon; NBER; IZA
[3]Department of Business Economics and Public Policy, University of Pennsylvania

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Abstract

For nearly every crime there is a victim. However, the vast majority of 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 "birthday celebration effect." The effects are particularly large for sexual assaults, especially those that occur in non-residential 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 nearly every crime committed there is both an offender and a victim. Yet, due to both data constraints and disciplinary norms, economics research on crime dating back to Becker (1968) has focused primarily 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 not blamed 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. As such, providing information can be a critical means through which social planners can empower victims while also tailoring public safety interventions to be maximally disruptive to potential offenders (Cozens et al., 2005; MacDonald, 2015; Branas et al., 2016), in so doing, reducing the overall 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.

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 (Papachristos et al., 2015). Second, for many crimes, especially those that have low clearance rates, abating crime through deterrence-based strategies is costly. This is especially true of family violence which typically occurs indoors and, as such, may be less sensitive to traditional law enforcement inputs than other interventions such as mandatory reporting laws (Rodriguez et al., 2001; Iyengar, 2009), the provision of social services (Davis et al., 2008; Iyengar and Sabik, 2009) or divorce laws (Stevenson and Wolfers, 2006; Brassiolo, 2016). 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 (Aizer and Doyle Jr, 2015; Dobbie et al., 2018), this policy option may be worth further exploration.

Third, victims often have relatively little information about their probability of being victimized as well as the effectiveness of private investments in crime control. Indeed, given that research has yet to offer much evidence on the causal effect of actions or policies on victimization, we might expect that individuals will have difficulty accurately forecasting this on their own. Therefore, it stands to reason that victims might not optimally invest in precaution in wide variety of situations.[1] 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. Because of this, 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 (Gutierrez and Kirk, 2017). While a large research literature in criminology identifies some demographic and situational

[1]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.

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 that plausibly has an outsize influence on victimization: legal access to alcohol. A large body of research has found evidence of significant social costs associated with legal and low-cost access to alcohol (Grossman and Markowitz, 1999; Markowitz, 2000; Carpenter, 2005, 2007; Carpenter and Dobkin, 2009, 2011; Cook and Durrance, 2013; Heaton, 2012; Kilmer et al., 2013; Carpenter and Dobkin, 2015; Anderson et al., 2017; Carpenter and Dobkin, 2017).[2] These papers utilize both age-based discontinuities in access to alcohol and geographic variation in state or local policy. The consensus reached by this literature suggests that legal access to or lower prices of alcohol are associated with increased traffic fatalities, suicides, violent behavior and injuries, including injuries among male victims which were intentionally inflicted (Carpenter and Dobkin, 2017). 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 more broadly, 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 and more credible 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

[2] A notable exception is a paper by Lindo et al. (2016) who find no evidence of an effect of legal access to alcohol on motor vehicle accidents in Australia.

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. By focusing on legal access to alcohol, we study a bundle of related interventions including the amount of alcohol that individuals consume as well as the venue in which drinking occurs. Our empirical analysis considers the mechanisms underlying the effects we observe and points to evidence that the venue of consumption is an important driver of the effects of legal access to alcohol.

In order to identify a causal effect of legal access to alcohol on victimization, we utilize the fact that legal access 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 large 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.

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. In particular, our findings suggest that victimization rises to the greatest extent among venues outside of one's home, suggesting that it is not merely the volume of alcohol consumed but also where it is consumed that drives victimization risk. 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.[3] 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.[4]

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 concludes.

2 Background

2.1 Private Actions and Victimization

There are 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,

[3] Furthermore, while Carpenter and Dobkin (2017) study emergency department visits and hospitalizations, those data limit them to studying intentional injuries caused by others. This measure is a composite of sexual assault and assault, and is likely underpowered to detect increases driven by sexual assaults alone, given the relative frequency of each type of crime. In our data, assaults are roughly 15 more common than sexual assaults for female victims. This difference might be larger in medical utilization data. This difference might be larger in medical utilization data, as Sugar et al. (2004) find while body injury is common in sexual assault, admission to emergency or surgical services happened only in 5 percent of the cases they studied.

[4] It is important to note that we are unable to identify the subset of victimizations that are cleared by an arrest. It is entirely possible that, in keeping with the literature on offending, there is no significant change in these victimizations after the 21st birthday. This is especially likely if there is an increase in victimizations that involve alcohol at the MLDA. See Spohn and Tellis (2012) for more information.

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 to 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 that 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 (Chalfin and McCrary, 2018). 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.[5] This raises the possibility that there may yet be low hanging fruit to pick with respect to addressing crime through private victim action.

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. Within economics, prior research has found that emergency department visits for injuries inflicted by others increase at age 21 (Carpenter and Dobkin, 2017) and that sexual assault victimization rises during college football game days (Lindo et al., 2018), an effect which is credibly due to an increase in alcohol consumption.

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, 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.

[5] 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) and Ehrlich (1981) who conceive of the "derived demand" for crime as the willingness of market participants to invest in private precautions.

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., 2019). 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. Currently, essentially every state implements a minimum legal drinking age of 21.[6] 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. First and most directly, research uses information from National Health Interview Survey to show that drinking increases at both the extensive and intensive margins when individuals turn 21 (Carpenter and Dobkin, 2009). Second, prior research shows that traffic fatalities (Carpenter and Dobkin, 2009, 2011; Francesconi and James, 2019) and drunk driving arrests (Carpenter and Dobkin, 2015; Hansen and Waddell, 2018) increase with legal access to alcohol due to the MLDA. Third, related 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. Finally, we note that the MLDA may also shift

[6] There are a few very limited exceptions. For instance, some states, such as Wisconsin, permit alcohol use with one's parents at restaurants.

the venue of alcohol consumption. While underage individuals can obtain alcohol from bars and restaurants either using fake identification or because waitstaff do not ask for proof of legal age, research suggests that individuals who consume alcohol prior to reaching the age of legal majority generally obtain alcohol at convenience stores, supermarkets or at house parties (Gosselt et al., 2007; Fabian et al., 2008).

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 microdata 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 our request for data: Baltimore, MD, Miami, FL, Orlando, FL, Philadelphia, PA, Boston, MA, Columbus, OH, Portland, OR, Phoenix, AZ and Newark NJ. We used data from every city which supplied us with data and did not exclude data from the analysis for any reason.

In each city, the data contain information on the type of crime, the date of victimization and the victim's exact date of birth and gender. To protect victim anonymity, we do not have victim identifiers. 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 generate a national estimate of the effect of legal exposure to alcohol on crime.

We supplement our administrative data with 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 U.S. Bureau of Justice Statistics (BJS) since 1977. The NCVS is the principal national dataset on victimization in the United States (Gutierrez and Kirk, 2017) and allows us to ensure that the reporting of crimes to law enforcement does not change discontinuously at age 21, a critical falsification check for our analysis. In the NCVS, respondents are asked to indicate whether they have been the victim of a crime during the past six months. Critically respondents 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, a story 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 could lead us to conclude that victimization increases at age 21 even though this effect might merely be an artifact of differential crime reporting. We consider whether crime reporting changes

[9] In a deviation from the index crime designation, this category 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 C for the universe of sex offenses by police department.

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

discontinuously at age 21 in Section 5.2.1 and conclude that, for most types of crimes, there is little evidence of differential crime reporting at the MLDA.

Prior to describing our empirical models and results, we pause here to present a brief descriptive analysis of the age-victimization profile in our administrative data. In Figure 1, we present the share of victimizations by age, using a local polynomial smoother. Violent crimes are presented in Panel A; property crimes are presented in Panel B. For both crime types, we present results separately for males (using a black line) and females (using a gray line). Consistent with a longstanding empirical regularity that has been documented by scholars of victimization, crime victimization generally rises throughout childhood, peaking between the ages of 20 and 30 and falling steeply thereafter (Stafford and Galle, 1984). Several exceptions are worth noting. First, there are important gender differences with respect to the victimization-age profile of sexual assault. Among males, sexual assaults are most prevalent in childhood and the risk declines substantially thereafter. Among females, sexual assault risk peaks just prior to age 20. Second, while homicide risk peaks for both genders in the early 20s, the peak is especially large for men reflecting the ubiquity of gang violence and "vendetta-like antagonisms," often referred to colloquially as "beefs" (Kennedy, 1996). The opposite pattern holds for assaults with women experiencing an especially high degree of vulnerability in their early 20s while men's victimization risk declines more slowly throughout their lifespan.

Referring to Appendix Figure A1, readers can contrast patterns in our data, derived from crime reports, with survey data reported to the NCVS.[12] Violent victimization patterns are, on the whole, extremely similar to those derived from our administrative data. However, there are some notable differences with respect to property crime victimization. In contrast to the law enforcement data from our eight cities, in the NCVS, the age-crime profile for burglary and larceny suggests that crime victimization drops off less dramatically after age 30. Notably, both the NCVS and

[12]Homicide is not included in this figure as all victims are deceased and are therefore unavailable to complete the survey.

our administrative data highlight that the age period affected by the MLDA, the early 20s, is an important period for policy given high victimization rates for nearly all crimes.

4 Methods

We estimate the causal effect of legal access to alcohol on victimization using a regression discontinuity design, leveraging the discrete change in legal alcohol access at age 21. The primary 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. The design likewise assumes that other features of the social environment that affect crime do not change discontinuously at age 21. While age is a common running variable in empirical applications in applied microeconomics — see e.g., Lemieux and Milligan (2008); Smith (2009); McCrary and Royer (2011) — we discuss potential violations of this assumption in Section 5.2.3.

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 operationalized using Poisson regression:

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

$Y_i \sim$ Poisson(γ_i) is the count of victimizations occurring at relative age i (measured as days relative to age 21), (X_i-c) is the number of days relative to a given crime victim's 21st birthday and D_i is an indicator variable for whether or not the criminal incident occurred prior to or after the victim's

21st birthday.[13] The coefficient on D_i, τ, identifies the causal effect of legal access to alcohol. 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 allow us to account for numerous factors which may 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 the likelihood that an individual lives alone. We also estimate several non-parametric RD equations.

Equation (1) is estimated for a given bandwidth, h, so that the regression is estimated for those observations within $c - h \leq X_i \leq c + h$. Our primary models use a bandwidth of two years. 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 additional robustness checks which test the sensitivity of the results to alternative modeling strategies.

In addition to estimating the effect of reaching 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, controlling for a victim's birthday 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. 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 whether the victim's birthday occurred on the subsequent weekend.

[13] Following a persuasive argument put forth in Osgood (2000), our default model is Poisson regression as it makes assumptions about error distributions that are consistent with the nature of event counts. Sometimes crime counts are modeled using negative binomial regression models due to concerns about overdispersion in the data. We prefer Poisson regression for two reasons. First, tests for overdispersion do not distinguish between overdispersion and misspecification — see Berk and MacDonald (2008) and Blackburn (2015). Consequently, it is *a priori* unclear when overdispersion actually exists and is consequently an issue. Second, Poisson regression is first order equivalent to negative binomial regression when robust standard errors are used — as we do Wooldridge (2010).

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 females, respectively. 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 a second order polynomial and a third order polynomial in age, respectively. In column (4), we focus on the quadratic specification and add a dummy variable for whether an individual is victimized on his or her birthday in order to distinguish legal access to alcohol from birthday celebration effects. 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.[14]

For males, legal access to alcohol leads to a 7 percent increase in both violent and property victimization. Effects are especially large for sex offenses (12-120 percent; 74 percent in our preferred model) though these are not precisely estimated as sexual assaults with male victims are relatively uncommon in the data. Effects are also meaningful and significant at conventional levels for robbery (8 percent), non-sexual assault (7 percent), larceny (8 percent) and motor vehicle theft (12 percent). Effects for burglary are close to zero in all specifications. For females, legal access to alcohol does not, in general, increase the likelihood of a violent victimization. This is consistent with Carpenter and

[14]We also estimate our models including an interaction between our birthday effect variables and the indicator for age over 21. Results are unchanged.

Dobkin (2017) who find female hospitalizations and emergency department visits do not increase for injuries intentionally caused by others. Their measure, like overall violent crime, is a composite of many different types of injuries.[15] Disaggregating crimes into finer categories reveal substantial heterogeneity across crime types. While assaults do not increase generally for female victims, sexual assaults increase considerably — by approximately 24 percent. Property crimes likewise increase — by approximately 12 percent for burglary and larceny. Unlike for males, we find little evidence that motor vehicle theft victimization is sensitive to the MLDA for women. We further note that the estimated effects are, for the most part, not sensitive to our choice of polynomial and persist regardless of how we account for birthday effects. The estimated effects for males can be seen graphically in Figure 2 and Figure 3. For females, the equivalent figures are Figure 4 and Figure 5. In each set of figures, we fit a local quadratic regression through the data.

5.2 Robustness

The results presented in Section 5.1 suggest that the probability of crime victimization changes discontinuously at age 21, an effect that we attribute to the minimum legal drinking age. In this section, we subject these results to greater scrutiny in order to establish that the change in victimization that we observe is the result of legal access to alcohol and not another feature of the social world.

5.2.1 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 crime reporting among individuals who have reached the minimum legal drinking age. This might be the case, for instance, if underage victims

[15] Indeed if we study sexual assault and assault aggregated together, we estimate a 1 percent increase in this combined crime category, which is similar in size and precision to their estimates.

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.

The 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 whether there is differential crime reporting by age, we turn to survey data and focus our attention on 18 to 35 year old respondents to the 2006-2016 waves of the National Crime Victimization Survey, the principal source of national data on crime reporting behavior (Lauritsen et al., 2009; Gutierrez and Kirk, 2017). Leveraging the fact that the NCVS captures whether an individual was victimized as well as whether or not she reported a given crime to law enforcement, we observe the extent to which reporting rates change discretely at age 21.

Figure 6 presents the age-path of the reporting rate separately by gender. We do so by regressing a dummy variable for whether a crime is reported on a series of age dummies, conditional upon interacted crime type by survey year fixed effects. We focus on reporting rates for the violent and property crime aggregates as reporting rates for individual crime types are noisy due to small numbers of victims in the NCVS.[16] There is little evidence of a discrete change in the reporting of violent crimes at 21. Likewise, for property crimes, there are no clear reporting trends among women aged 19-23 — the slight jump in crime reporting at 21 (approximately 2 percentage points) is small and statistically insignificant. Among men there is a secular increase in crime reporting with age and there is some evidence that property crime reporting increases discretely at age 21. However estimated difference is small (approximately 3.5 percentage points) and is not statistically significant at conventional levels. We note that even if the estimated reporting difference is taken at face value it is unlikely to be large enough to explain the magnitude of our point estimate. We

[16] We report the number of survey respondents in each crime type by age by gender bin in Appendix F.

further note that the increase in property crime reporting that we observe among males in the overall NCVS sample is not seen among the sample of NCVS respondents who live in an urban area and who therefore better accord with our analysis sample (see Appendix Figure A2).

One caveat in using the NCVS is that the survey records a respondent's age at the time that the survey was administered, not the respondent's age at this time she was victimized. As a result, there are likely to be a number of instances in which the victim's age is mislabeled with respect to when the victimization occurred. In Figure A3 we subtract 1 from each respondent's age to account for the possibility that a respondent's calendar age does not reflect their age at the time of victimization. Patterns are qualitatively similar to those in Figure 6 but provide even less compelling evidence for a discontinuous change in crime reporting at 21. Overall, the evidence provides support for our claim that the changes in victimization that we observe at the minimum legal drinking age are driven primarily by legal access to alcohol and are unlikely to be an artifact of age-graded reporting patterns among crime victims.

5.2.2 Bandwidth Selection

In order to test the sensitivity of our preferred estimates to bandwidth selection, we re-estimate our primary outcome model for a range of bandwidths between 180 and 730 days, in 10-day increments. The results of this exercise are presented in Figure 7 (violent crimes) and Figure 8 (property crimes). The figures demonstrate that our principal estimates are unlikely to be driven by a strategic choice of bandwidth. With the exception of motor vehicle theft, the estimates are, if anything, more conservative at our preferred bandwidth of two years than they are at smaller bandwidths. We also re-estimate results using the optimal bandwidth calculation of Calonico et al. (2014); these results are presented in Appendix Table A1 (using a uniform kernel) and Appendix Table A2 (using a triangle kernel) and are substantively similar to our preferred estimates.

5.2.3 Sample Selection

While our administrative dataset provides incredibly granular data on the timing of victimization, an inherent limitation is that we observe only those individuals who are victimized by a crime. As such, our estimates could potentially be compromised by sample selection bias — that is, differential selection into the sample local to the minimum legal drinking age. Given that the data we use to draw inferences are victimization counts by relative age, the most pressing concern is that sample selection bias changes the risk of entering our sample as a function of the running variable. In particular, we note the possibility that, upon reaching the minimum legal drinking age, individuals who live in outlying areas become differentially likely to travel to the cities in our sample — for instance, to consume alcohol in bars or nightclubs in the closest large city. To the extent that this is true, there would be more 21 year olds than 20 year olds available to victimize in municipal law enforcement data and, as such, we could observe increased victimization after age 21 that is an artifact of geographic selection rather than a genuine change in the vulnerability of potential victims at age 21. Some of our prior analyses are inconsistent with this notion, as several categories of common crimes do not show increases. For instance, we do *not* find that general assaults increase for females. In this section, we offer a more formal test for geographic sorting at the MLDA.

Leveraging national data from the NCVS and additional detail in our microdata from Dallas, we offer three different tests for geographic sorting at the minimum legal drinking age. First, using NCVS data, we assess the extent to which the share of victimizations that occurred in a crime victim's county of residence changes as a function of age. A *decrease* in the share of home county victimizations would be consistent with geographic sorting effects. As discussed in Section 5.2.1, a limitation to the NCVS is that exact dates of birth and victimization are not provided. Accordingly victimizations that occur at age 21 will apply to individuals who are both age 21 and age 22 at the time they were surveyed by the NCVS. These data are plotted in Figure 9. Overall, while there is evidence for a secular rise in the share of home county violent crime victims with age, there is little evidence that the share of victimizations in a victim's home county decreases between the ages of 19 and 23.

Next, in Dallas, we observe each crime victim's home municipality, allowing us to discern whether the crime victim is a Dallas resident or not. We use these data to construct two additional tests for geographic sorting. First, given that sample selection will be predominantly driven by selection into our sample among non-residents, we begin by focusing our attention on crime victims who are *local residents*. The assumption is that, among local residents, we would not expect the number of potential victims to change discontinuously at the threshold of the running variable. While an effect of the MLDA on victimization among this sub-sample provides evidence that sample selection is not driving our main results, we note that to the extent that local residents and non-residents themselves differ with respect to victimization risk, there is, of course, no requirement that the results of this analysis should mirror our main estimates.

We present RD estimates for local residents in Dallas in Table 4. For each model, a positive point estimate indicates that, among local residents, who are less subject to geographic sorting concerns, victimization rises at the minimum legal drinking age. Referring to the table, there is clear evidence for an increase in property victimization among both male and female Dallas residents. For violent offenses, the evidence is less compelling. In particular, we do not see clear evidence for the increase in violent victimization for men that is reported in Table 2 or the increase in sexual assault victimization that we reported in Table 3. That said, the estimates use data from a single city and, as such, are imprecise. There is, therefore, little evidence against the estimates reported in Tables 2 and 3. For example, among male Dallas residents, point estimates suggest an increase in robbery victimization of 5 percent and an increase in assault victimization of 4 percent. These estimates are extremely similar to those reported in Column (6) of Table 2 where we estimate that robbery and assault victimizations rise among men by 8 percent and 7 percent, respectively. For sexual assaults with female victims, our point estimate suggests that these, if anything, decline at the MLDA among Dallas residents. However, the standard error (0.258) is large and, accordingly, the estimate is not inconsistent with the 25 percent increase reported in Table 3.

To provide further clarity, we present a third test of geographic sorting and consider whether the *share of crime victims who are local residents* changes discontinuously at age 21. To the extent that this share falls at age 21, this might constitute evidence of geographic sorting. Because some crime types are rare in the data, there are relative ages, measured in days, in which there are zero crimes in the data. Accordingly the share of local residents, the dependent variable in this specification, is undefined in some bins. To address this issue, we collapse our data into monthly bins and, given that there are a relatively small number of data points to fit, we fit a local linear regression to the data. These results are presented in Figure 10 and Figure 11. With the exception of overall property crimes for males, one test out of thirteen, there is little evidence of a discontinuous increase in the share of victims who are local residents at age 21 for any of our crime types or for the crime aggregates. Taken as a whole, our reading of the evidence — from Dallas as well as the NCVS — is that the effect of the MLDA on victimization is unlikely to be an artifact of geographic sorting.

5.2.4 Other Robustness Checks

We conduct several additional robustness tests. First, we consider the possibility that the results reported in Section 5.1 might be driven disproportionately by one of the eight cities in our sample and thus might be sensitive to the removal of any one of these cities from the data. In Tables A3 and A4, we re-estimate our preferred models — those that use a quadratic polynomial and are reported in column (2) of Tables 2 and 3 — dropping one city at a time from our data. In all cases, estimates are remarkably insensitive to the exclusion of any one of our eight cities. This analysis is also helpful in addressing the possibility that legal access to recreational marijuana in Denver or medical marijuana in San Diego — both of which occur at the age of 21 — is confounding our results. Dropping either (or both) of these cities from the analysis has no substantive impact on point estimates for any of the crimes we study. Given the link between crime and access to gambling (Gazel et al., 2001; Grinols and Mustard, 2006), we also consider whether our results might be an artifact of the fact, in most states, the minimum legal age to gamble at a casino is 21. To address this, we focus on Milwaukee and San Diego, the two cities in our sample in which the legal age to gamble in a casino is below the age of 21. We report estimates for this sub sample in Appendix Table A5. While the estimates are less precise given that the sample is smaller, they are substantively similar to our preferred estimates.

Next, we show that the increase in victimization that we observe at age 21 is unique and is not present at other ages that are, to first order, unaffected by the MLDA. Figure 12 and Figure 13 present 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 IRR bounded by a 95 percent confidence interval 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. 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 test for whether our estimates are robust to different functional forms. We begin by re-estimating results using least squares regression (Appendix Table A6 and A7) and negative binomial regression (Appendix Table A8). Next, we note that our primary estimates aggregate victimizations by relative age across our entire sample of cities. In Appendix Table A9 and Appendix

Table A10, we instead aggregate by the city × relative age bin, including city fixed effects in the model and clustering standard errors by relative age.[17,18]

5.3 Extensions

Having considered the robustness of our main results, we next consider two extensions which provide further context for these results. First, we consider the potential mechanisms through which legal access to alcohol increases crime victimization. Next, we consider the extent to which crime victimization rises, in general, around an individual's birthday — whether that birthday is an individual's 21st or not.

5.3.1 Location of Victimization

There are two primary mechanisms through which legal access to alcohol might affect victimization: exposure and vulnerability. By exposure, we are referring to the change in alcohol *access* that occurs at age 21, understanding that even though minors regularly access alcohol before reaching the minimum legal drinking age, the ease through which alcohol can be accessed changes at age 21 (Carpenter and Dobkin, 2009). By vulnerability, we are referring to the change in the ways in which alcohol is consumed at the MLDA, independent of any change in exposure. In particular, does legal access to alcohol increase victimization by shifting the location of problematic drinking (e.g., drinking in bars of nightclubs) or does legal alcohol access operate primarily by increasing alcohol use?

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

[17]Similar robustness checks for birthday celebration effects can be found in Appendix Table A11 and Appendix Table A12.

[18]We also re-estimate our RD models using local quadratic regressions which we present in Appendix Table A1 and Appendix Table A2. In all cases, estimates are quantitatively and qualitatively similar to our preferred estimates.

locations. Location information was shared by the following 5 police departments in our sub-sample: Dallas, TX, Denver, CO, Houston, TX, Milwaukee, WI and St. Louis, MO.[19] 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 celebration effect. As in previous analyses, we further disaggregate results by crime type and gender. For males, effects on violent victimization are, on the whole, driven by crimes that occur in non-residential locations. This is especially true for assaults and also, to a lesser extent, for robberies.[20] Effects on property victimization vary less by location type. 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.

More broadly, some types of crimes, for instance burglary, might largely capture the exposure or away from effect. While the effect of the MLDA is large and significant for burglary, this point estimate is considerably smaller than the point estimate for the most social costly crimes. This suggests while exposure could account for some of the impacts on sexual assaults, it likely would not account for the majority of the estimated impacts.[21]

5.3.2 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). Until this point, we have conditioned on the period of time just around a victim's

[19] Appendix E contains the department-specific location tags that we consider to be "residential."

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

[21] In Appendix Table A13, we consider whether effects vary according to whether local universities are in session. This could be a potentially important source of heterogeneity in our estimates if college students are especially likely to be inframarginal actors with respect to the MLDA. While we cannot perfectly disambiguate between the availability of college students and seasonality more generally, the table provides little evidence that effects vary along this dimension.

birthday in order to more reliably identify the effect of the MLDA. In this section, we investigate whether there are "birthday effects," that is, a general 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 an individual's birthday and the following three days and an indicator for the week around an individual's birthday.[22] Table 6 presents Poisson estimates of the change in the likelihood of victimization on or around an individual's birthday — these estimates are derived from the same model used to estimate effects of the MLDA in Tables 2 and 3 in which we controlled for birthday effects.[23] 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. 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 on or around their birthdays. 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.[24]

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

[22]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.

[23]Appendix Table A11 reports log-linear estimates of the birthday effect.

[24]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 Appendix Figure A4 and Appendix Figure A5, 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.

attention in the literature. Given that recent media attention and research related to criminal justice highlight the high social costs of over-policing (Fagan et al., 2016) and the widespread application of harsh prison sentences (Aizer and Doyle Jr, 2015), there is increasing appeal to understanding whether other policies can affect crime while potentially imposing 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 novel administrative dataset that contains 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 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. Victims of both genders experience a modest increase in the incidence of property crimes. 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.[25]

This research provides some of the first causal evidence that alcohol increases crime victim-

[25] The effects of legal access to alcohol that we report in this paper are qualitatively large and empirically important. However, these estimates potentially point to an even larger role of *alcohol use* in crime victimization. To see this, consider that the reduced form estimates reported in this paper can be seen as intent-to-treat estimates of the effect of alcohol use, understanding that alcohol will tend to change discontinuously, albeit imperfectly, with legal access to alcohol. The magnitude of the effect of alcohol *use* on victimization will thus depend on the first stage relationship between the MLDA and alcohol use. Thus, subject to an assumption that the MLDA affects victimization only through the increased use of alcohol, our results can be seen a conservative estimate of the aggregate impact of problematic alcohol use on crime victimization.

ization. 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 differences-in-difference estimates (Anderson et al., 2017). The local average treatment effect (LATE) of the former is based on criminality, and the LATE of the latter is based on the combination of victimization and criminality. Our LATE, identifies the impact of the MLDA on victimization alone. 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. Moreover, while Carpenter and Dobkin (2017) find injuries caused by others do not increase for females, their measure is an aggregation of both assaults and sexual assaults. Given the victimization data reveal assaults are many times more common than sexual assaults, it is entirely possible medical visits for sexual assaults might have increased in their sample, but that this increase was nullified by a lack of increase in other assaults. 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 their self control due to increased alcohol use.

More generally, this research highlights the possibility that 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 victimization. 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. It is also a means through which public safety interventions can be optimally tailored to achieve maximum impact on social welfare.

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.[26] As it stands, our estimates suggest that the increased consumption of alcohol at age 21 is met with additional costs previously not considered. Moreover, there are a number of other policies worth considering that may interact with both exposure and alcohol consumption mechanisms which shift at age 21. These include zoning and licensing, operating hours restrictions, and alcohol taxes.

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.

[26] 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	0.781	0.769	0.779
	(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**	1.078*	1.080*	1.078*
	(0.0278)	(0.0424)	(0.0585)	(0.0424)	(0.0426)	(0.0425)
Assault	1.058***	1.067**	1.083*	1.063**	1.058*	1.065**
	(0.0212)	(0.0325)	(0.0452)	(0.0321)	(0.0309)	(0.0317)
Property						
All	1.023	1.074***	1.077***	1.069***	1.066***	1.069***
	(0.0142)	(0.0222)	(0.0302)	(0.0221)	(0.0219)	(0.0217)
Burglary	1.003	1.047	0.976	1.021	1.023	1.023
	(0.0307)	(0.0493)	(0.0620)	(0.0493)	(0.0496)	(0.0495)
Larceny	1.032*	1.079***	1.108***	1.066**	1.063**	1.066**
	(0.0181)	(0.0280)	(0.0385)	(0.0297)	(0.0294)	(0.0293)
Motor Vehicle Theft	1.007	1.091	1.089	1.121**	1.113**	1.121**
	(0.0402)	(0.0682)	(0.0914)	(0.0542)	(0.0534)	(0.0533)

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	1.014	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	1.233**	1.226**	1.240**
	(0.0649)	(0.108)	(0.137)	(0.105)	(0.102)	(0.105)
Robbery	1.014	1.034	1.015	1.033	1.034	1.033
	(0.0360)	(0.0568)	(0.0771)	(0.0569)	(0.0572)	(0.0567)
Assault	0.998	1.002	0.992	1.000	0.999	1.002
	(0.0129)	(0.0198)	(0.0258)	(0.0198)	(0.0196)	(0.0198)
Property						
All	1.036***	1.108***	1.091***	1.103***	1.099***	1.106***
	(0.0135)	(0.0216)	(0.0289)	(0.0207)	(0.0200)	(0.0204)
Burglary	1.010	1.116***	1.068	1.117***	1.115***	1.119***
	(0.0259)	(0.0424)	(0.0525)	(0.0443)	(0.0444)	(0.0442)
Larceny	1.049***	1.117***	1.114***	1.119***	1.115***	1.124***
	(0.0166)	(0.0270)	(0.0371)	(0.0265)	(0.0256)	(0.0265)
Motor Vehicle Theft	0.988	1.022	0.976	1.021	1.015	1.018
	(0.0413)	(0.0638)	(0.0803)	(0.0482)	(0.0479)	(0.0477)

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 RD Effects For Local Residents, Dallas Subsample

	Male		Female	
	(1)	(2)	(3)	(4)
Violent				
All	1.045	1.045	0.973	0.973
	(0.0605)	(0.0604)	(0.0404)	(0.0404)
Homicide	0.816	0.819	0.475	0.510
	(0.526)	(0.528)	(1.355)	(1.461)
Sex Offenses	6.726	6.627	0.910	0.912
	(7.999)	(7.857)	(0.256)	(0.258)
Robbery	1.050	1.050	0.686**	0.686**
	(0.121)	(0.121)	(0.122)	(0.122)
Assault	1.038	1.038	0.997	0.997
	(0.0695)	(0.0694)	(0.0437)	(0.0436)
Property				
All	1.141*	1.141*	1.191***	1.190***
	(0.0818)	(0.0818)	(0.0723)	(0.0723)
Burglary	1.106	1.107	1.420***	1.420***
	(0.158)	(0.159)	(0.158)	(0.158)
Larceny	1.380**	1.374**	1.258*	1.257*
	(0.222)	(0.221)	(0.156)	(0.155)
Motor Vehicle Theft	1.100	1.101	1.029	1.029
	(0.0963)	(0.0964)	(0.0882)	(0.0881)

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 for residents of Dallas. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Even numbered columns include indicator variables for the week around birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

	(1)	(2)	(3)	(4)
	Male		**Female**	
	Residential	Non-Residential	Residential	Non-Residential
Violent				
All	0.988	1.097***	1.020	1.013
	(0.0486)	(0.0340)	(0.0289)	(0.0293)
Homicide	0.653	0.780	0.333	3.447*
	(0.306)	(0.178)	(0.343)	(2.283)
Sex Offenses	0.431	5.957***	1.121	1.247
	(0.252)	(2.886)	(0.156)	(0.170)
Robbery	1.170	1.086*	1.113	0.999
	(0.157)	(0.0540)	(0.161)	(0.0606)
Assault	0.974	1.102**	1.013	1.001
	(0.0502)	(0.0464)	(0.0295)	(0.0337)
Property				
All	1.087*	1.054*	1.106***	1.111***
	(0.0493)	(0.0285)	(0.0414)	(0.0293)
Burglary	1.086		1.128**	
	(0.0630)		(0.0540)	
Larceny	1.085	1.025	1.091	1.137***
	(0.102)	(0.0340)	(0.0722)	(0.0341)
Motor Vehicle Theft	1.095	1.123**	0.748	1.032
	(0.271)	(0.0578)	(0.198)	(0.0512)

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 and indicator variables 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 6: Poisson Birthday Effects

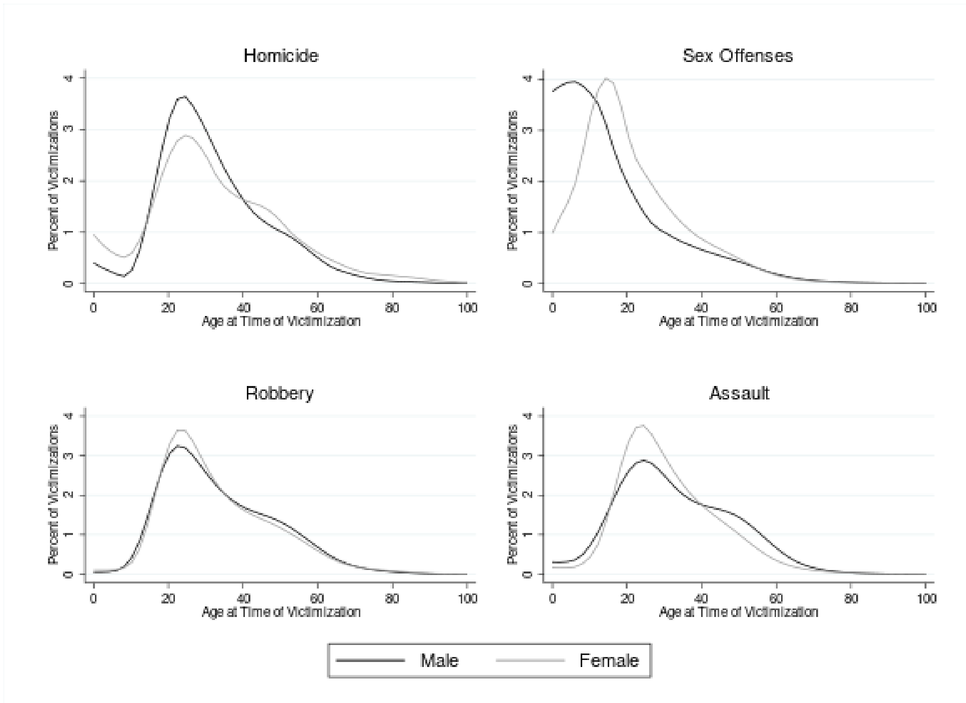
	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	1.148***	1.128***	1.074**	1.167***	1.093***	1.054**
	(0.0437)	(0.0421)	(0.0341)	(0.0547)	(0.0332)	(0.0246)
Homicide	0.850	1.429	1.243	1.567	1.417	1.439
	(0.519)	(0.369)	(0.244)	(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	1.194***	1.010	1.042	1.291***	1.148***	1.132***
	(0.0426)	(0.0819)	(0.0697)	(0.0534)	(0.0573)	(0.0440)
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

7 Figures

Age Profile of Victimization

Panel A: Violent Victimizations



Panel B: Property Victimizations

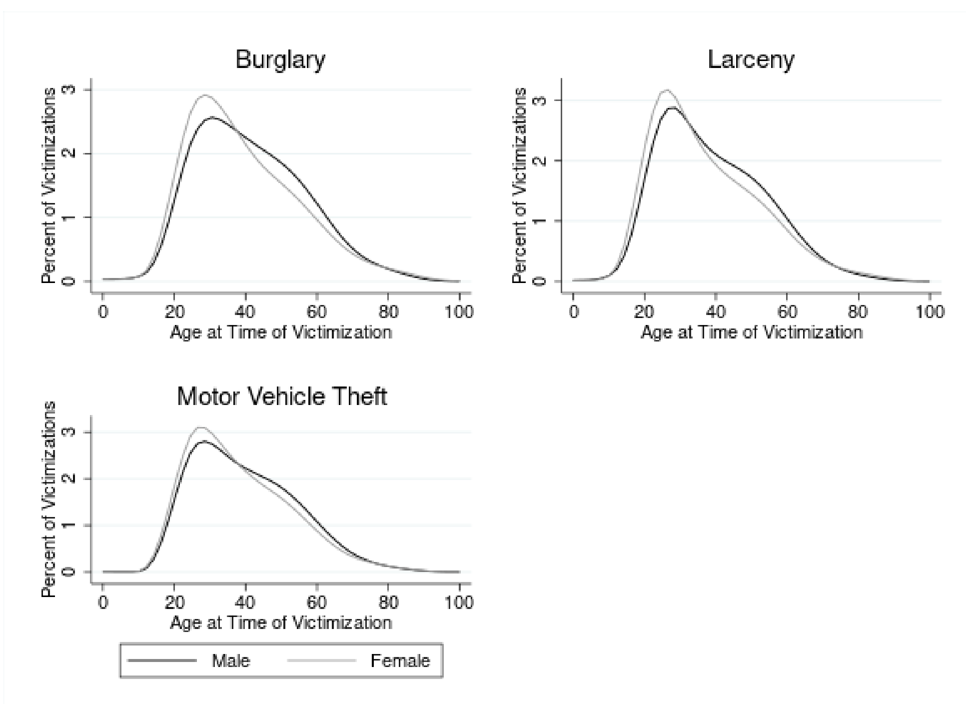


Figure 1: This figure presents local polynomial regressions of age on the share of victimizations in the data. Each observation is the share of all victimizations within crime type and gender that occur at a given age.

Effect of MLDA on Male Victimization

Violent Victimizations

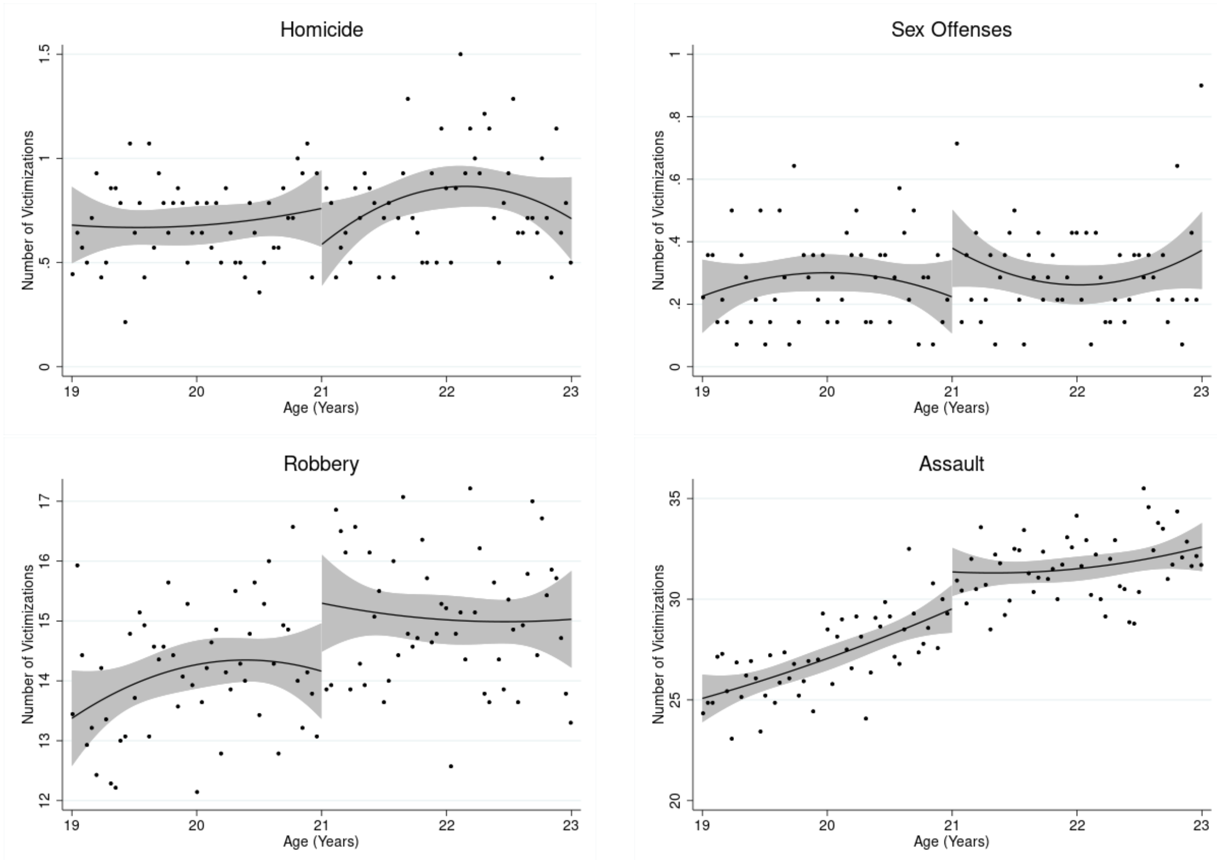


Figure 2: This figure contains fitted Poisson estimates and average victimization counts in 14 day bins for violent crimes. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls. 95% confidence intervals included.

Effect of MLDA on Male Victimization

Property Victimizations

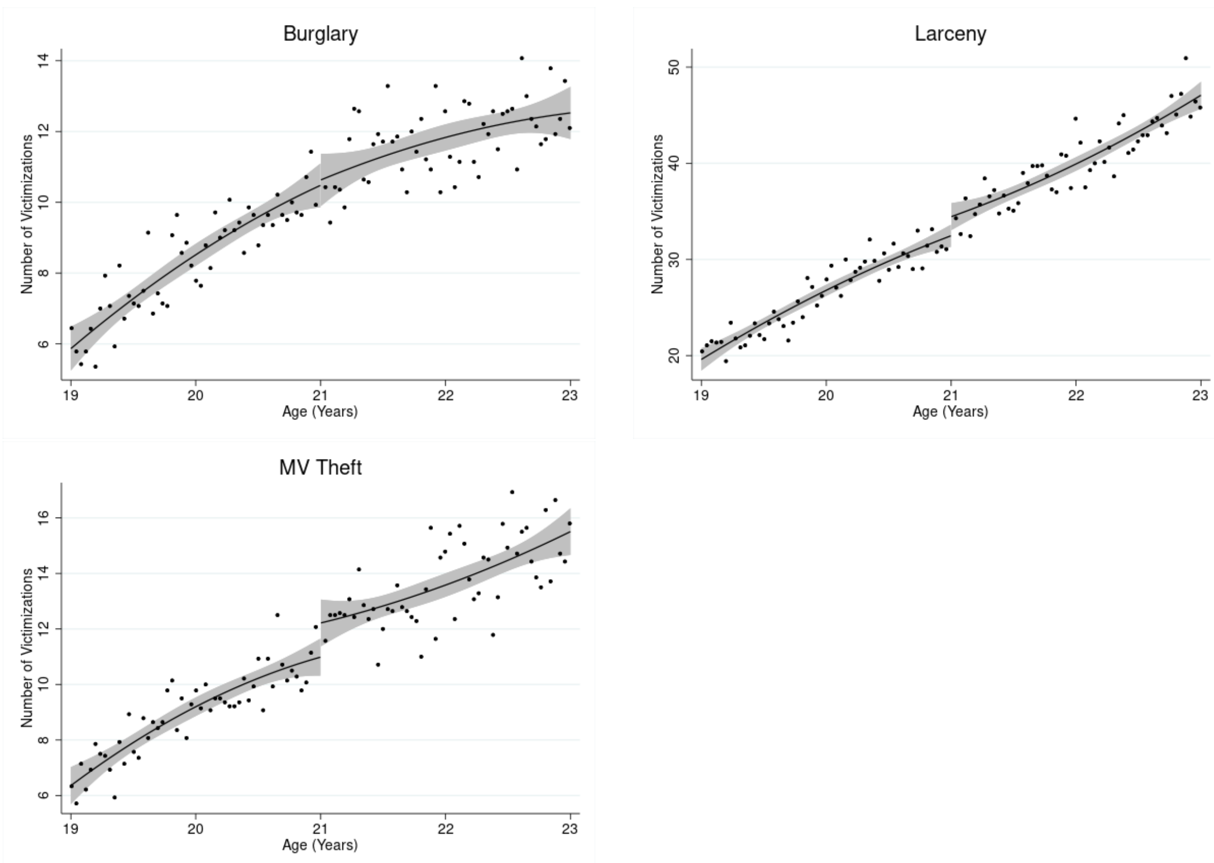


Figure 3: This figure contains fitted Poisson estimates and average victimization counts in 14 day bins for property crimes. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls. 95% confidence intervals included.

Effect of MLDA on Female Victimization

Violent Victimizations

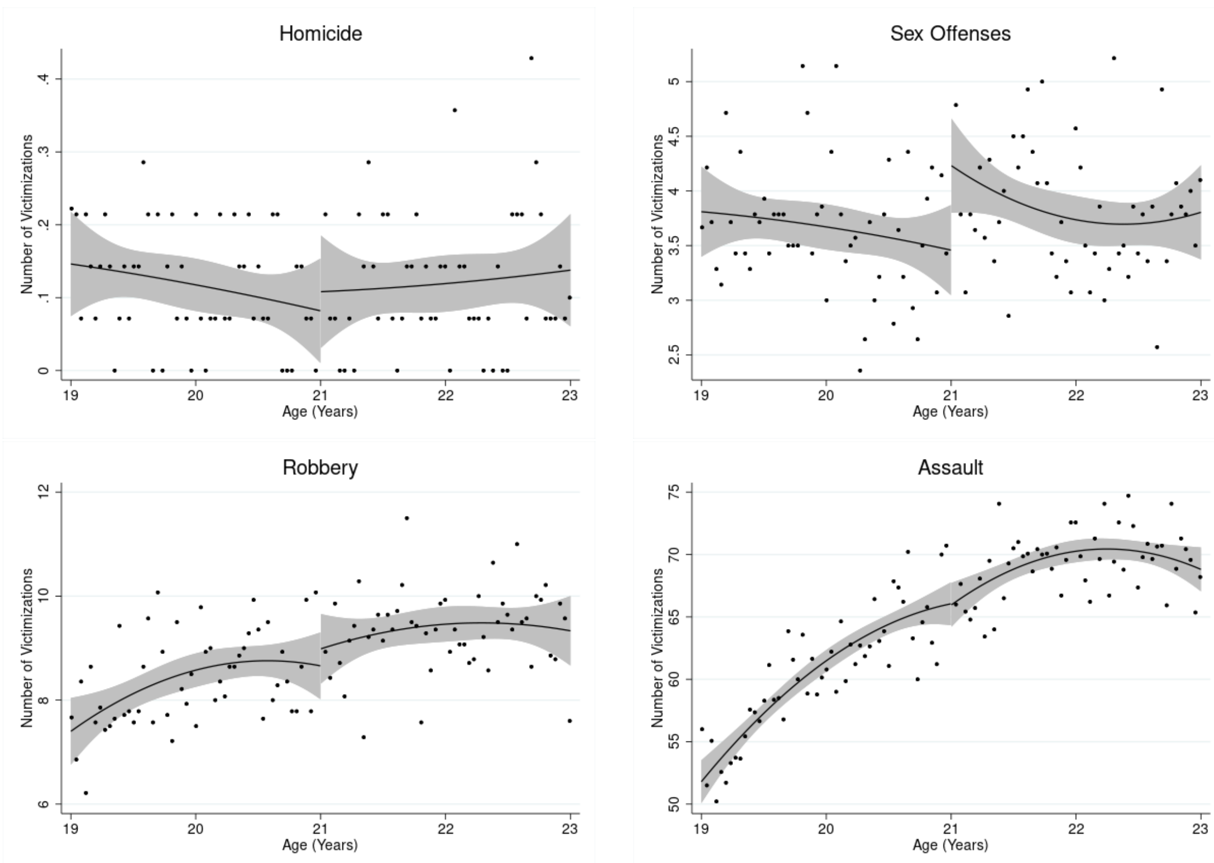


Figure 4: This figure contains fitted Poisson estimates and average victimization counts in 14 day bins for violent crimes. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls. 95% confidence intervals included.

Effect of MLDA on Female Victimization

Property Victimizations

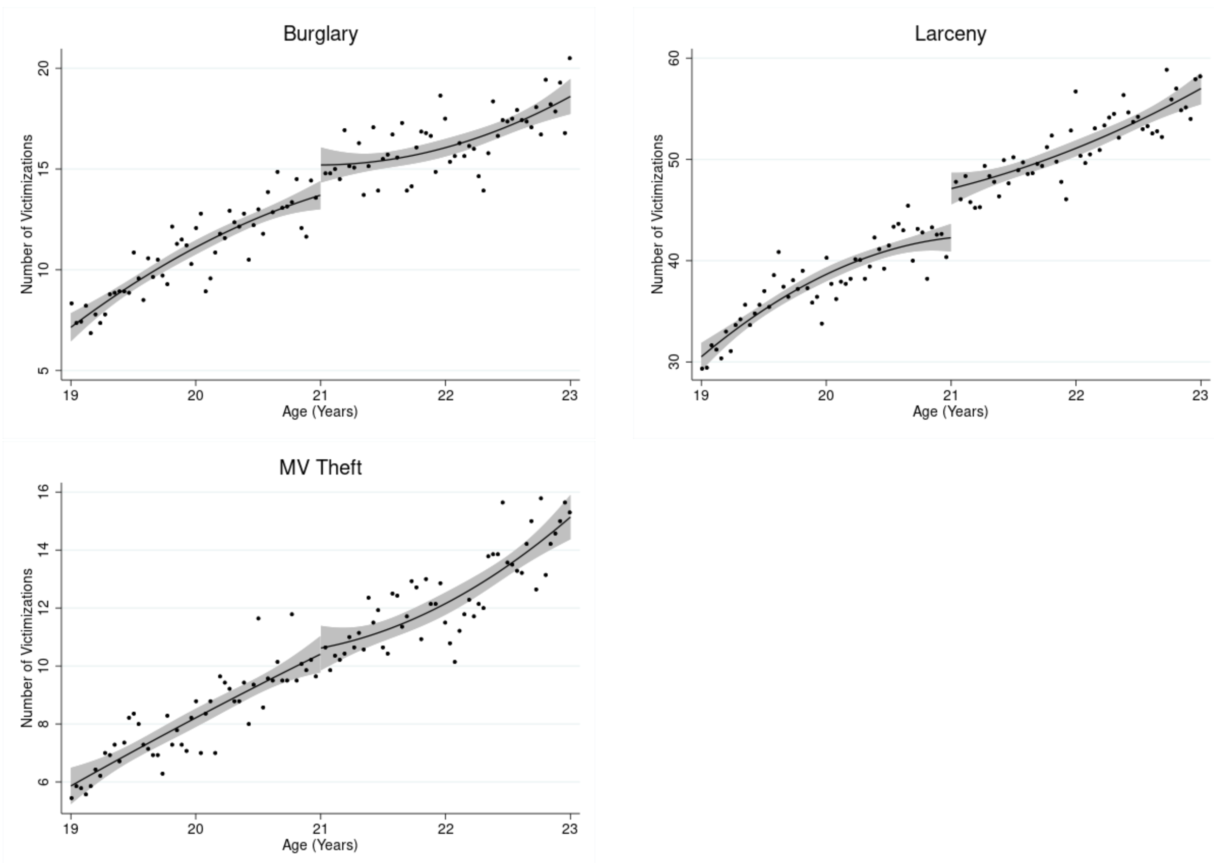
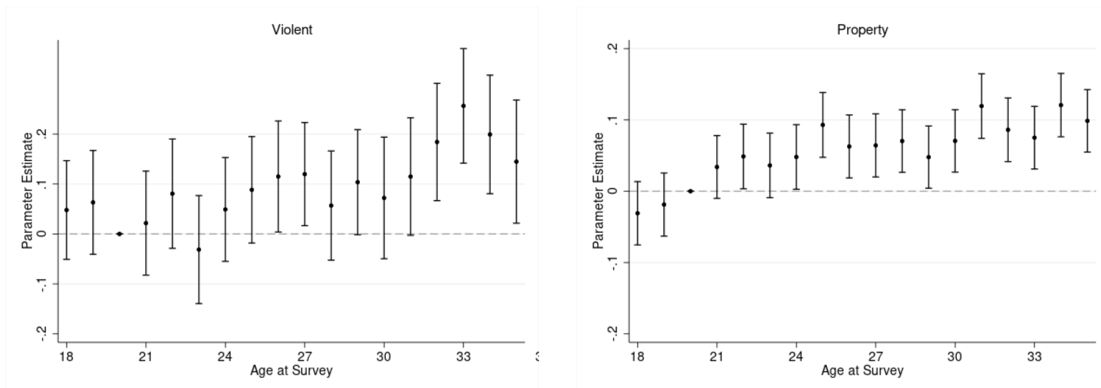


Figure 5: This figure contains fitted Poisson estimates and average victimization counts in 14 day bins for property crimes. Poisson estimates include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls. 95% confidence intervals included.

Reported Victimization by Age

Panel A: Males



Panel B: Females

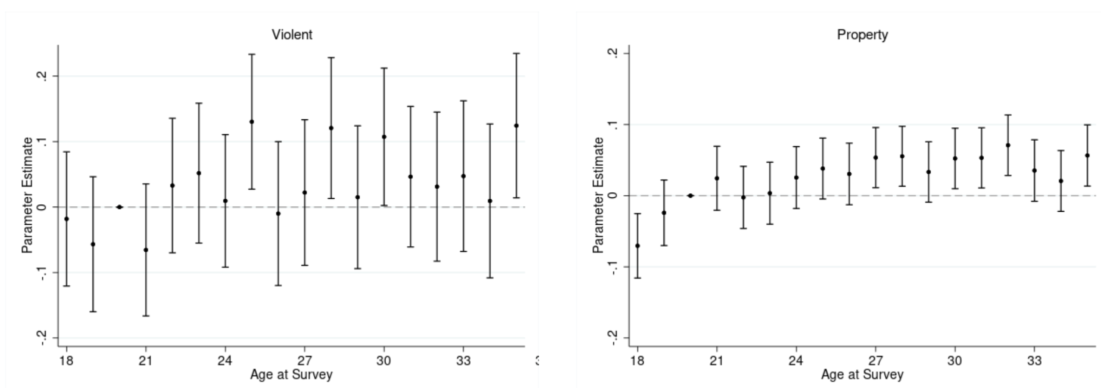


Figure 6: This figure presents estimates of β_k from $Report_{ikt} = \alpha + \sum_{18}^{35} \beta_k Age\_Ind_{ik} + \gamma_{tw} + \epsilon_{ikt}$ where i is individual, k is age in years at time of survey, and t crime type. $Report_{ikt}$ is an indicator variable that is 1 if an individual reported his or her victimization to the police and 0 otherwise. Age_Ind_{ik} is an indicator variable that is 1 if individual i was age k at the time of survey wave w. 95% confidence intervals included based on robust standard errors. Estimates are based on the 2006-2016 waves of the National Crime Victimization Survey and relative to age 20.

Bandwidth Sensitivity:MLDA Effect

Violent Victimizations

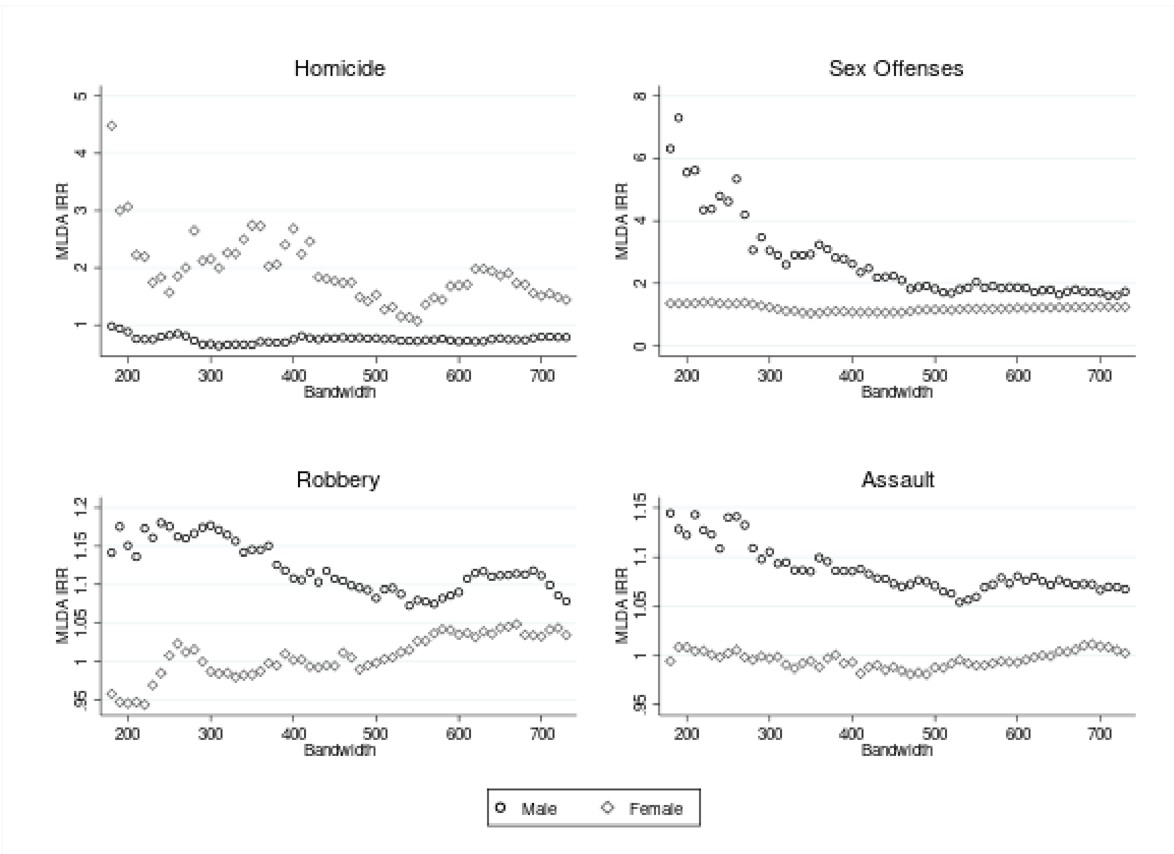


Figure 7: This figure presents estimates of the sensitivity of the RD estimates presented in Tables 2 and 3 to the choice of bandwidth for violent crimes. In the figures, the bandwidth is plotted on the x-axis; incident rate ratios are reported on the y-axis. In each case, regressions include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Bandwidth Sensitivity:MLDA Effect

Property Victimizations

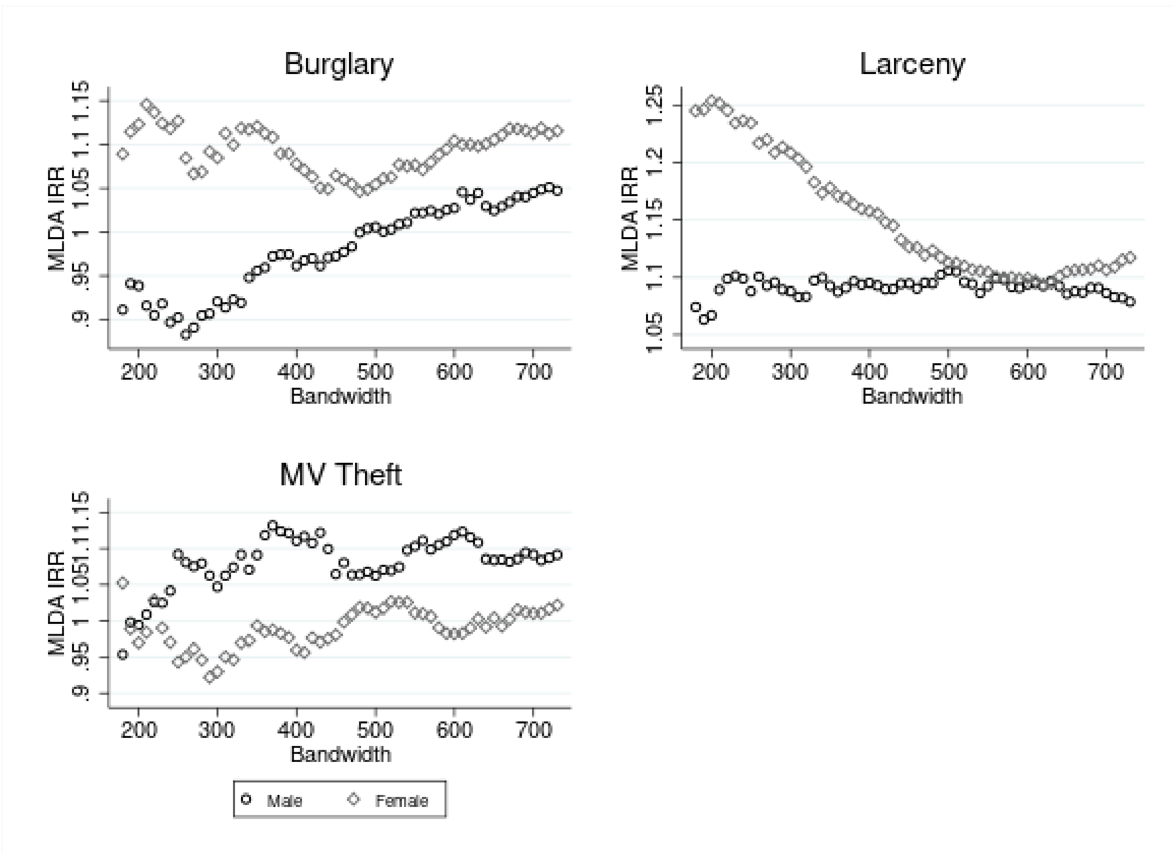
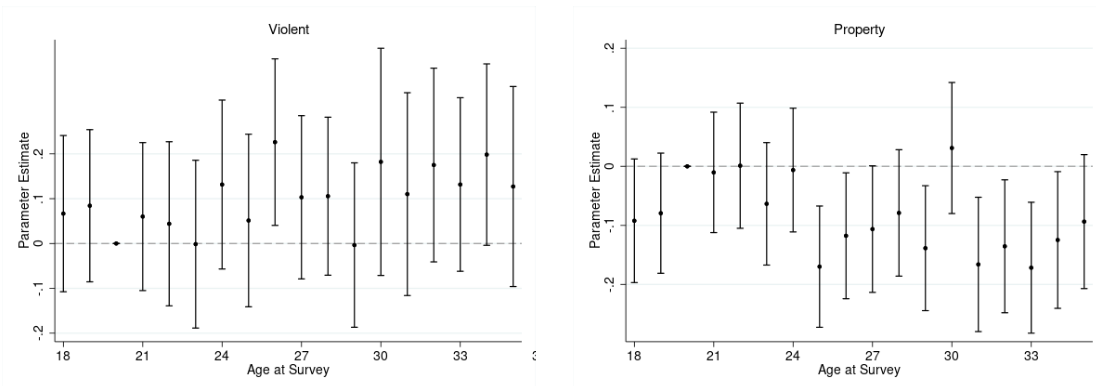


Figure 8: This figure presents estimates of the sensitivity of the RD estimates presented in Tables 2 and 3 to the choice of bandwidth for property crimes. In the figures, the bandwidth is plotted on the x-axis; incident rate ratios are reported on the y-axis. In each case, regressions include a second order polynomial in age fully interacted with an indicator for age over 21 and no birthday controls.

Home County Victimization By Age

Panel A: Males



Panel B: Females

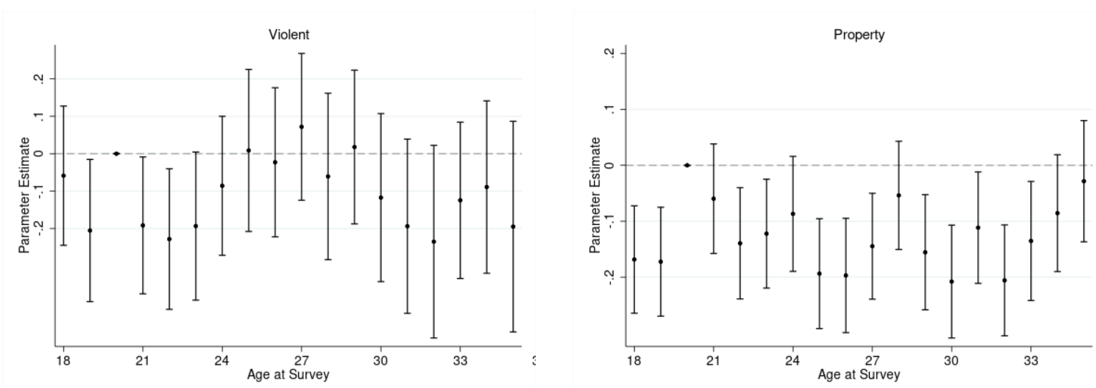
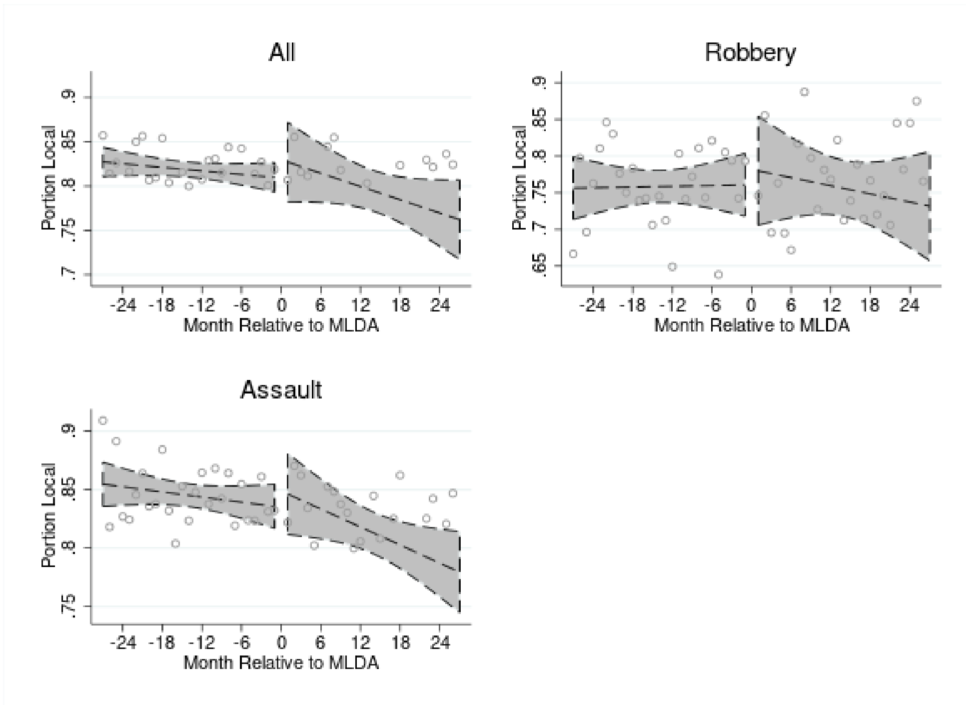


Figure 9: This figure presents estimates of β_k from $Home\_County_{ikt} = \alpha + \sum_{18}^{35} \beta_k Age\_Ind_{ik} + \gamma_{tw} + \epsilon_{ikt}$ where i is individual, k is age in years at time of survey minus one year, and t crime type. $Home\_County_{ikt}$ is an indicator variable that is 1 if an individual was victimized in his or her county of residence and 0 otherwise. Age_Ind_{ik} is an indicator variable that is 1 if individual i was age k at the time of survey wave w. 95% confidence intervals included based on robust standard errors. Estimates are based on the 2006-2016 waves of the National Crime Victimization Survey and relative to age 20.

Sample Selection Test: Male Victims, Dallas Subsample

Panel A: Violent Victimizations



Panel B: Property Victimizations

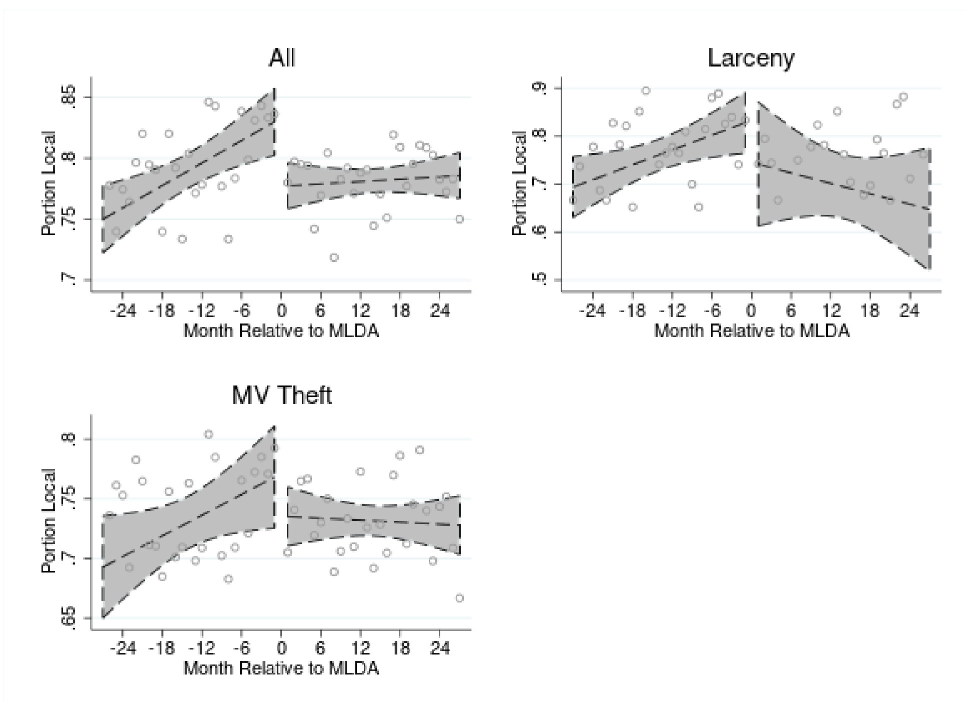
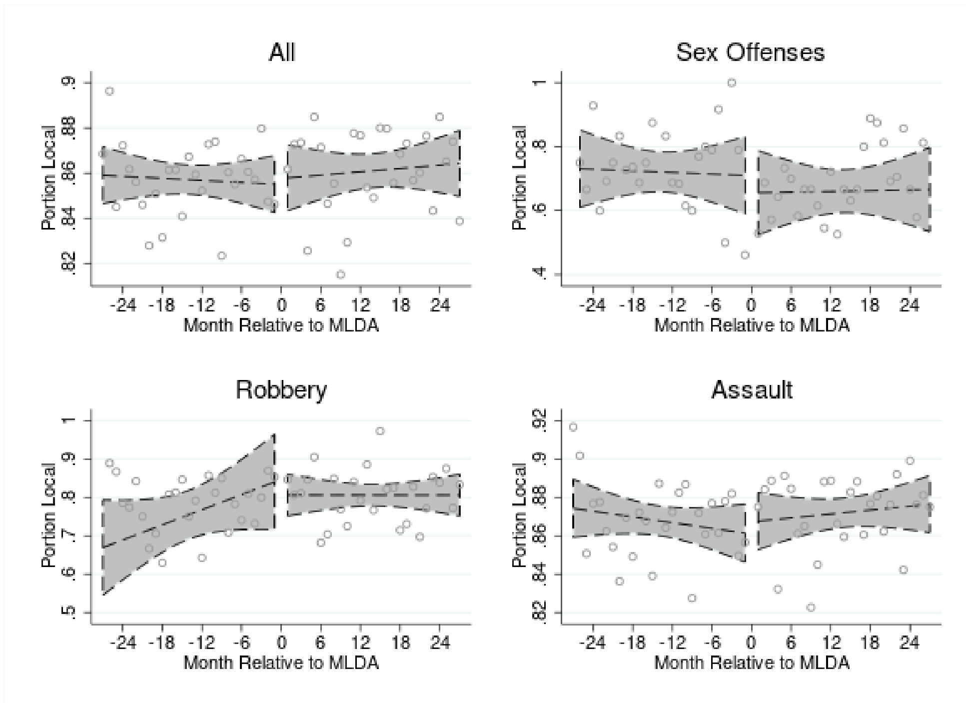


Figure 10: This figure contains local linear regressions of relative age (months) on the share of male victims who are local to Dallas. 95% confidence intervals are included.

Sample Selection Test: Female Victims, Dallas Subsample

Panel A: Violent Victimizations



Panel B: Property Victimizations

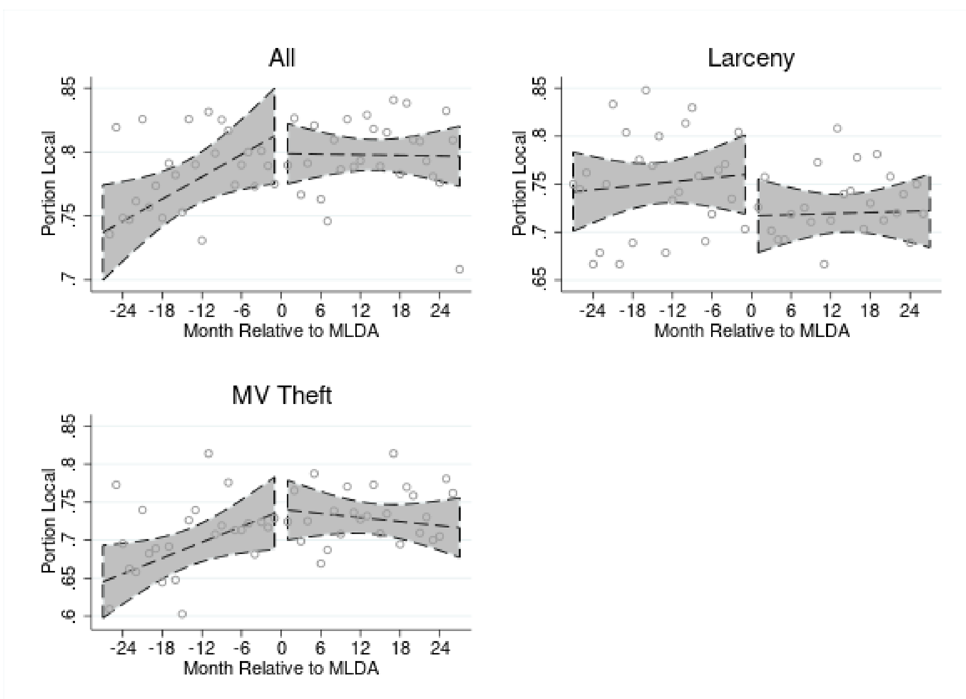


Figure 11: This figure contains local linear regressions of relative age (months) on the share of female victims who are local to Dallas. 95% confidence intervals are included.

Placebo Test of RD Effect on Male Victimization

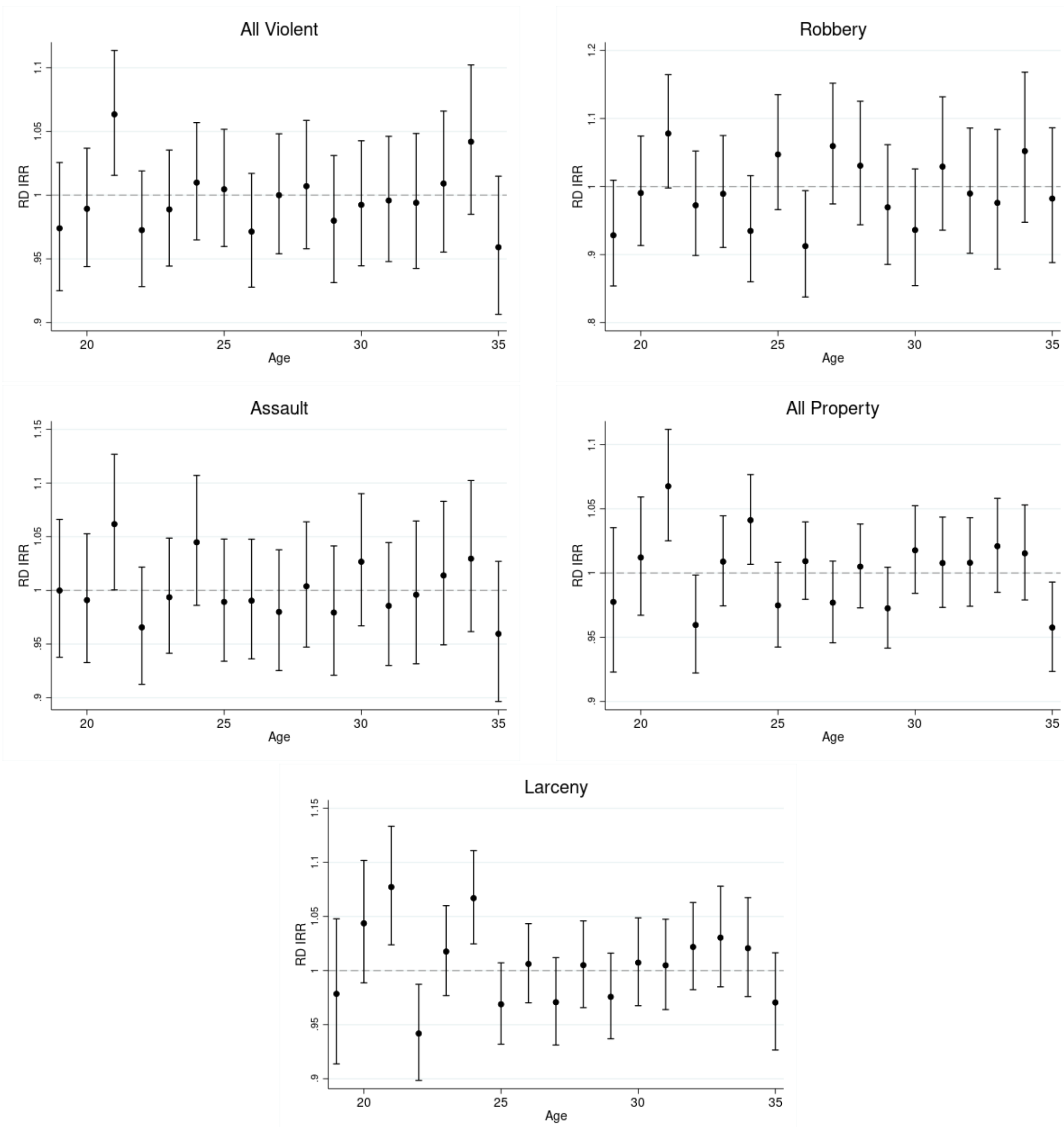


Figure 12: This figure contains IRR estimates for the RD effect of the MLDA on male victimization for each age from 19 to 35. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

Placebo Test of RD Effect on Female Victimization

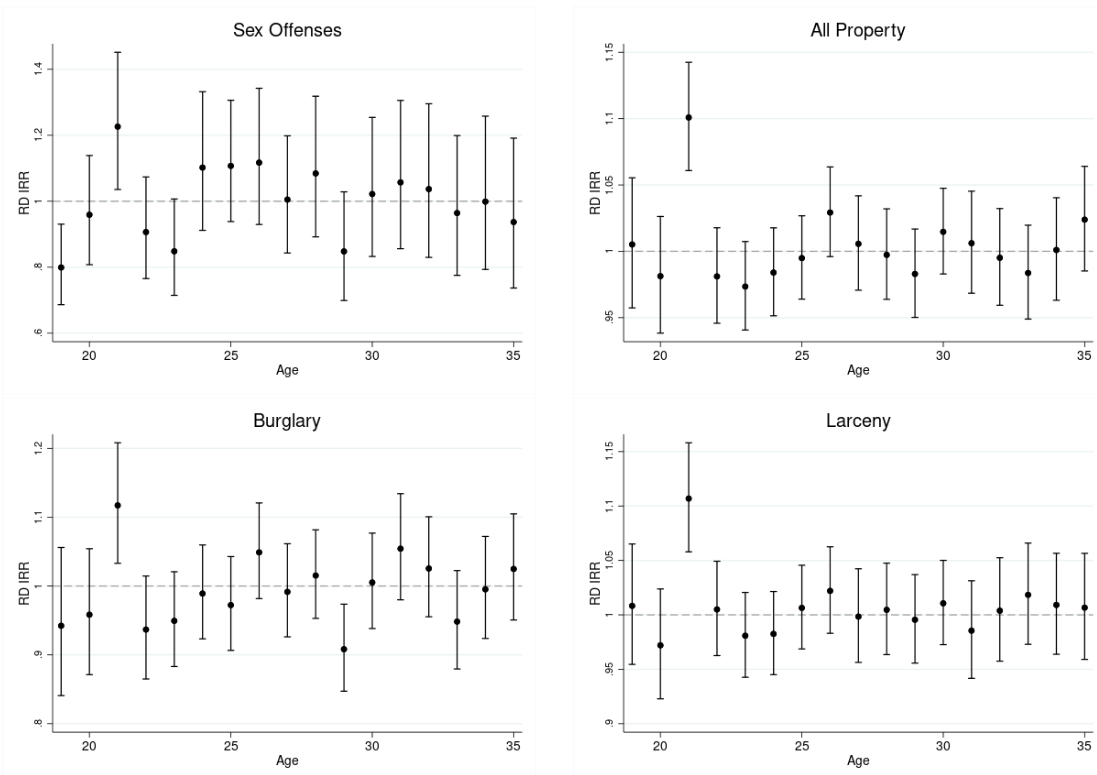


Figure 13: This figure contains IRR estimates for the RD effect of the MLDA on female victimization for each age from 19 to 35. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

ONLINE APPENDIX

Appendix A: Supplementary Tables

Table A1: Log-Linear RD Effects: Optimal Bandwidth, Using a Uniform Kernel

	Male		**Female**	
	No BDay	BDay	No BDay	BDay
	(1)	(2)	(3)	(4)
Violent				
All	0.135***	0.116***	-0.00737	-0.00741
	(0.0438)	(0.0418)	(0.0371)	(0.0371)
Homicide	-0.166	-0.175*	0.0351	0.0312
	(0.102)	(0.102)	(0.0514)	(0.0523)
Sex Offenses	0.211**	0.212**	0.318*	0.298*
	(0.0843)	(0.0844)	(0.179)	(0.176)
Robbery	0.160**	0.153*	-0.0267	-0.0215
	(0.0784)	(0.0780)	(0.0928)	(0.0932)
Assault	0.123**	0.112**	-0.00829	-0.0139
	(0.0544)	(0.0533)	(0.0379)	(0.0389)
Property				
All	0.0426	0.0430	0.169***	0.169***
	(0.0337)	(0.0333)	(0.0325)	(0.0332)
Burglary	-0.169*	-0.135	0.0651	0.0690
	(0.0870)	(0.0892)	(0.0790)	(0.0784)
Larceny	0.0640	0.0670	0.229***	0.234***
	(0.0443)	(0.0444)	(0.0434)	(0.0428)
Motor Vehicle Theft	0.123	0.0996	0.0355	0.0417
	(0.0904)	(0.0912)	(0.0882)	(0.0893)

This table contains estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime type. The estimates in this table are produced using the optimal bandwidth of Calonico et al (2014), a rectangular kernel, and local quadratic regression to construct the point estimator. Odd numbered columns do not include birthday controls; even numbered columns control for the week around each birthday. Each observation is the natural log of the total number of victims in each (days) relative to the 21st birthday. Adjustment from Chalfin and McCrary (2018) used when necessary. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table A2: Log-Linear RD Effects: Optimal Bandwidth using a Triangle Kernel

	Male		**Female**	
	No BDay	BDay	No BDay	BDay
	(1)	(2)	(3)	(4)
Violent				
All	0.131***	0.124***	0.0160	0.0158
	(0.0416)	(0.0392)	(0.0355)	(0.0349)
Homicide	-0.107	-0.0977	0.0590	0.0414
	(0.112)	(0.114)	(0.0546)	(0.0518)
Sex Offenses	0.264***	0.269***	0.479**	0.410**
	(0.0925)	(0.0936)	(0.195)	(0.185)
Robbery	0.152**	0.147**	-0.0324	-0.0365
	(0.0708)	(0.0704)	(0.0896)	(0.0912)
Assault	0.121**	0.114**	0.000423	-0.000882
	(0.0540)	(0.0506)	(0.0345)	(0.0321)
Property				
All	0.0380	0.0308	0.185***	0.172***
	(0.0352)	(0.0363)	(0.0306)	(0.0295)
Burglary	-0.136*	-0.133*	0.0922	0.0870
	(0.0806)	(0.0798)	(0.0639)	(0.0637)
Larceny	0.0673	0.0618	0.252***	0.227***
	(0.0452)	(0.0443)	(0.0432)	(0.0409)
Motor Vehicle Theft	0.103	0.0782	0.0374	0.0331
	(0.0832)	(0.0929)	(0.0845)	(0.0851)

This table contains estimates for the RD effect of the minimum legal drinking age on male and female victimization rates for each crime type. The estimates in this table are produced using the optimal bandwidth of Calonico et al (2014), a triangular kernel, and local quadratic regression to construct the point estimator. Odd numbered columns do not include birthday controls; even numbered columns control 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: Poisson Male RD Effects, Excluding One City

	Excluded City							
	(1) Charlotte	(2) Dallas	(3) Denver	(4) Houston	(5) Kansas City	(6) Milwaukee	(7) San Diego	(8) St. Louis
Violent								
All	1.115	1.083***	1.071	1.071	1.103*	1.071	1.007	1.071
	(0.0873)	(0.0295)	(0.0559)	(0.0559)	(0.0598)	(0.0559)	(0.0645)	(0.0559)
Homicide	0.318	0.823	0.952	0.952	1.024	0.952	1.644	0.952
	(0.245)	(0.178)	(0.481)	(0.481)	(0.565)	(0.481)	(0.976)	(0.481)
Sex Offenses	1.065	1.411	1.867	1.867	2.041	1.867	2.664	1.867
	(1.188)	(0.463)	(1.247)	(1.247)	(1.367)	(1.247)	(2.234)	(1.247)
Robbery	1.042	1.081*	1.002	1.002	1.025	1.002	0.961	1.002
	(0.185)	(0.0468)	(0.0983)	(0.0983)	(0.104)	(0.0983)	(0.110)	(0.0983)
Assault	1.160*	1.087**	1.093	1.093	1.126*	1.093	1.012	1.093
	(0.104)	(0.0381)	(0.0662)	(0.0662)	(0.0711)	(0.0662)	(0.0763)	(0.0662)
Property								
All	1.112	1.059**	1.093*	1.093*	1.081	1.093*	1.098*	1.093*
	(0.0757)	(0.0251)	(0.0505)	(0.0505)	(0.0520)	(0.0505)	(0.0623)	(0.0505)
Burglary	0.864	1.003	0.903	0.903	0.889	0.903	0.957	0.903
	(0.112)	(0.0535)	(0.0789)	(0.0789)	(0.0790)	(0.0789)	(0.108)	(0.0789)
Larceny	1.236**	1.069**	1.179***	1.179***	1.168**	1.179***	1.159**	1.179***
	(0.110)	(0.0326)	(0.0696)	(0.0696)	(0.0708)	(0.0696)	(0.0832)	(0.0696)
MV Theft	1.213	1.096	1.118	1.118	1.107	1.118	1.100	1.118
	(0.337)	(0.0681)	(0.157)	(0.157)	(0.180)	(0.157)	(0.155)	(0.157)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on male victimization rates for each rates for each crime type. All regressions include second order polymonials in age fully interacted with an indicator for age over 21. Column labels indicate the city whose data is excluded from the regressions. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Poisson Female RD Effects, Excluding One City

	Excluded City							
	(1) Charlotte	(2) Dallas	(3) Denver	(4) Houston	(5) Kansas City	(6) Milwaukee	(7) San Diego	(8) St. Louis
Violent								
All	1.056	1.029	1.016	1.016	1.014	1.016	1.000	1.016
	(0.0764)	(0.0227)	(0.0428)	(0.0428)	(0.0442)	(0.0428)	(0.0489)	(0.0428)
Homicide	6.778	1.755	1.076	1.076	0.956	1.076	0.592	1.076
	(11.88)	(0.908)	(0.928)	(0.928)	(0.820)	(0.928)	(0.505)	(0.928)
Sex Offenses	1.292	1.268**	1.426**	1.426**	1.495**	1.426**	1.453	1.426**
	(0.276)	(0.126)	(0.241)	(0.241)	(0.260)	(0.241)	(0.342)	(0.241)
Robbery	1.089	1.084	1.134	1.134	1.180	1.134	1.099	1.134
	(0.273)	(0.0663)	(0.156)	(0.156)	(0.169)	(0.156)	(0.172)	(0.156)
Assault	1.021	1.006	0.978	0.978	0.967	0.978	0.972	0.978
	(0.0789)	(0.0235)	(0.0447)	(0.0447)	(0.0463)	(0.0447)	(0.0509)	(0.0447)
Property								
All	1.146**	1.104***	1.098**	1.098**	1.092**	1.098**	1.081	1.098**
	(0.0778)	(0.0242)	(0.0465)	(0.0465)	(0.0484)	(0.0465)	(0.0519)	(0.0465)
Burglary	1.175	1.093**	1.091	1.091	1.073	1.091	1.054	1.091
	(0.130)	(0.0473)	(0.0887)	(0.0887)	(0.0924)	(0.0887)	(0.109)	(0.0887)
Larceny	1.122	1.118*	1.107*	1.107*	1.113**	1.107*	1.094	1.107*
	(0.100)	(0.0298)	(0.0583)	(0.0583)	(0.0595)	(0.0583)	(0.0645)	(0.0583)
MV Theft	1.301	1.032	1.023	1.023	0.883	1.023	1.053	1.023
	(0.388)	(0.0649)	(0.185)	(0.185)	(0.195)	(0.185)	(0.197)	(0.185)

This table contains IRR estimates for the RD effect of the minimum legal drinking age on female victimization rates for each rates for each crime type. All regressions include second order polymonials in age fully interacted with an indicator for age over 21. Column labels indicate the city whose data is excluded from the regressions. Each observation is the total number of victims in each age (days) relative to the 21st birthday. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Poisson RD Effects: Milwaukee and San Diego Supsample

	Male		**Female**	
	No BDay	BDay	No BDay	BDay
	(1)	(2)	(3)	(4)
Violent				
All	1.178***	1.177***	1.044	1.042
	(0.0580	(0.0578)	(0.0422)	(0.0418)
Homicide	0.411**	0.412**	4.287	4.334
	(0.169)	(0.167)	(4.171)	(4.126)
Sex Offenses	1.364	1.360	1.131	1.127
	(0.891)	(0.880)	(0.186)	(0.184)
Robbery	1.158*	1.161*	1.361***	1.360***
	(0.0889)	(0.0897)	(0.149)	(0.149)
Assault	1.217***	1.214***	0.988	0.987
	(0.0797)	(0.0787)	(0.0437)	(0.0432)
Property				
All	1.039	1.038	1.186***	1.183***
	(0.0486)	(0.0480)	(0.0474)	(0.0461)
Burglary	0.916	0.916	1.154*	1.149*
	(0.0873)	(0.0872)	(0.0875)	(0.0853)
Larceny	1.057	1.056	1.207***	1.204***
	(0.0622)	(0.0618)	(0.0617)	(0.0603)
Motor Vehicle Theft	1.180	1.173	1.139	1.141
	(0.148)	(0.145)	(0.116)	(0.116)

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. All regressions include second order polynomials in age fully interacted with an indicator for age over 21.Odd numbered columns do not include birthday controls; even numbered columns control 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 A6: 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	0.0533***	0.0695***	0.0897***	0.0677***	0.0653***	0.0687***
	(0.0158)	(0.0235)	(0.0309)	(0.0234)	(0.0229)	(0.0232)
Homicide	-0.00837	-0.0893	-0.124	-0.0887	-0.0947	-0.0903
	(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	0.0473*	0.0758*	0.139**	0.0765*	0.0782*	0.0765*
	(0.0282)	(0.0415)	(0.0544)	(0.0416)	(0.0417)	(0.0417)
Assault	0.0620***	0.0733**	0.0791*	0.0703**	0.0662**	0.0719**
	(0.0203)	(0.0302)	(0.0407)	(0.0300)	(0.0294)	(0.0298)
Property						
All	0.0162	0.0661***	0.0733**	0.0644***	0.0625***	0.0651***
	(0.0144)	(0.0211)	(0.0284)	(0.0209)	(0.0209)	(0.0207)
Burglary	-0.0285	0.0137	-0.0637	0.0108	0.0142	0.0136
	(0.0345)	(0.0513)	(0.0685)	(0.0513)	(0.0515)	(0.0514)
Larceny	0.0181	0.0592**	0.0965**	0.0576**	0.0550*	0.0580**
	(0.0197)	(0.0291)	(0.0386)	(0.0290)	(0.0288)	(0.0286)
Motor Vehicle Theft	0.0397	0.121**	0.115*	0.119**	0.116**	0.120**
	(0.0341)	(0.0512)	(0.0677)	(0.0511)	(0.0512)	(0.0507)

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 A7: 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	0.00516	0.0160	0.00125	0.0139	0.0127	0.0153
	(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***	0.236***	0.153	0.229**	0.225**	0.234***
	(0.0588)	(0.0909)	(0.125)	(0.0904)	(0.0900)	(0.0906)
Robbery	0.000943	0.0411	0.00140	0.0405	0.0414	0.0407
	(0.0387)	(0.0591)	(0.0813)	(0.0593)	(0.0597)	(0.0591)
Assault	-0.00380	0.00246	-0.00726	0.000585	-0.000605	0.00193
	(0.0131)	(0.0198)	(0.0262)	(0.0198)	(0.0197)	(0.0198)
Property						
All	0.0335**	0.106***	0.0840***	0.103***	0.0996***	0.105***
	(0.0133)	(0.0196)	(0.0264)	(0.0190)	(0.0185)	(0.0186)
Burglary	0.00367	0.109**	0.0660	0.105**	0.103**	0.107**
	(0.0294)	(0.0432)	(0.0573)	(0.0431)	(0.0432)	(0.0430)
Larceny	0.0576***	0.123***	0.116***	0.118***	0.115***	0.121***
	(0.0165)	(0.0248)	(0.0338)	(0.0240)	(0.0234)	(0.0237)
Motor Vehicle Theft	-0.0416	0.0303	-0.0401	0.0320	0.0266	0.0293
	(0.0367)	(0.0537)	(0.0694)	(0.0538)	(0.0538)	(0.0536)

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 A8: Negative Binomial RD Effects

	Male		**Female**	
	No BDay	BDay	No BDay	BDay
	(1)	(2)	(3)	(4)
Violent				
All	1.069***	1.068***	1.016	1.016
	(0.0251)	(0.0246)	(0.0190)	(0.0188)
Homicide	0.780	0.779	1.437	1.429
	(0.148)	(0.147)	(0.685)	(0.674)
Sex Offenses	1.743*	1.741*	1.244**	1.240**
	(0.530)	(0.529)	(0.107)	(0.105)
Robbery	1.078*	1.078*	1.034	1.033
	(0.0423)	(0.0425)	(0.0568)	(0.0567)
Assault	1.067**	1.065**	1.002	1.002
	(0.0325)	(0.0317)	(0.0198)	(0.0198)
Property				
All	1.071***	1.069***	1.109***	1.106***
	(0.0224)	(0.0217)	(0.0217)	(0.0204)
Burglary	1.024	1.023	1.121***	1.119***
	(0.0495)	(0.0495)	(0.0447)	(0.0442)
Larceny	1.067**	1.066**	1.127***	1.124***
	(0.0298)	(0.0293)	(0.0282)	(0.0265)
Motor Vehicle Theft	1.124**	1.121**	1.019	1.018
	(0.0545)	(0.0533)	(0.0480)	(0.0477)

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. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Odd numbered columns do not include birthday controls; even numbered columns control 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 A9: Poisson Male RD Effects–City FEs

	(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	0.992	0.779	0.706	0.781	0.769	0.779
	(0.120)	(0.148)	(0.166)	(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	1.047*	1.078*	1.138**	1.078*	1.080*	1.078*
	(0.0278)	(0.0423)	(0.0583)	(0.0424)	(0.0426)	(0.0425)
Assault	1.058***	1.067**	1.083*	1.063**	1.058*	1.065**
	(0.0213)	(0.0325)	(0.0452)	(0.0321)	(0.0309)	(0.0317)
Property						
All	1.021	1.071***	1.073**	1.069***	1.066***	1.069***
	(0.0144)	(0.0224)	(0.0304)	(0.0221)	(0.0219)	(0.0217)
Burglary	0.991	1.024	0.943	1.021	1.023	1.023
	(0.0316)	(0.0495)	(0.0613)	(0.0493)	(0.0496)	(0.0495)
Larceny	1.022	1.067**	1.100**	1.066**	1.063**	1.066**
	(0.0192)	(0.0298)	(0.0415)	(0.0297)	(0.0294)	(0.0293)
Motor Vehicle Theft	1.049	1.124**	1.120*	1.121**	1.113**	1.121**
	(0.0332)	(0.0545)	(0.0712)	(0.0542)	(0.0534)	(0.0533)

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 and city fixed effects. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21, city fixed effects, 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 clustered at the relative age (days) level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Poisson Female RD Effects–City FEs

	(1) Order 1	(2) Order 2	(3) Order 3	(4) Birthday 1	(5) Birthday 2	(6) Birthday 3
Violent						
All	1.008	1.016	1.001	1.014	1.013	1.016
	(0.0122)	(0.0190)	(0.0250)	(0.0189)	(0.0187)	(0.0188)
Homicide						
Over 21	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	1.169***	1.244**	1.134	1.233**	1.226**	1.240**
	(0.0649)	(0.107)	(0.136)	(0.105)	(0.102)	(0.105)
Robbery	1.014	1.034	1.016	1.033	1.034	1.033
	(0.0360)	(0.0568)	(0.0772)	(0.0569)	(0.0572)	(0.0567)
Assault	0.998	1.002	0.991	1.000	0.999	1.002
	(0.0129)	(0.0198)	(0.0258)	(0.0198)	(0.0196)	(0.0198)
Property						
All	1.037***	1.109***	1.094***	1.103***	1.099***	1.106***
	(0.0136)	(0.0217)	(0.0292)	(0.0207)	(0.0200)	(0.0204)
Burglary	1.014	1.121***	1.081	1.117***	1.115***	1.119***
	(0.0272)	(0.0447)	(0.0562)	(0.0443)	(0.0444)	(0.0442)
Larceny	1.061***	1.127***	1.127***	1.119***	1.115***	1.124***
	(0.0173)	(0.0282)	(0.0388)	(0.0265)	(0.0256)	(0.0265)
Motor Vehicle Theft	0.961	1.019	0.978	1.021	1.015	1.018
	(0.0310)	(0.0480)	(0.0598)	(0.0482)	(0.0479)	(0.0477)

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 and city fixed effects. The regressions in Columns (4) - (6) contain second order polynomials in age fully interacted with an indicator for age over 21, city fixed effects, 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 clustered at the relative age (days) level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: 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	0.146**	0.0995**	0.0891***	0.297***	0.188***	0.125***
	(0.0616)	(0.0452)	(0.0315)	(0.0460)	(0.0345)	(0.0279)
Burglary	0.239***	-0.0122	0.0102	0.287***	0.150**	0.143***
	(0.0455)	(0.0985)	(0.0724)	(0.0504)	(0.0628)	(0.0437)
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 A12: Poisson Birthday Effects–City FEs

	(1)	(2)	(3)	(4)	(5)	(6)
		Male			**Female**	
	Birthday 1	Birthday 2	Birthday 3	Birthday 1	Birthday 2	Birthday 3
Violent						
All	1.149***	1.129***	1.073**	1.168**	1.093**	1.054
	(0.0466)	(0.0437)	(0.0311)	(0.0723)	(0.0489)	(0.0417)
Homicide	0.851	1.431**	1.245	1.567	1.417	1.439
	(0.558)	(0.224)	(0.209)	(1.716)	(0.761)	(0.604)
Sex Offenses	0.637	0.777	1.045	1.664**	1.379***	1.208*
	(0.637)	(0.357)	(0.371)	(0.387)	(0.147)	(0.131)
Robbery	0.945	0.943	0.948	1.076	1.007	1.048
	(0.0631)	(0.0505)	(0.0523)	(0.116)	(0.0430)	(0.0371)
Assault	1.261***	1.218***	1.131***	1.150**	1.087*	1.045
	(0.0814)	(0.0655)	(0.0348)	(0.0701)	(0.0507)	(0.0415)
Property						
All	1.130*	1.112***	1.101***	1.362***	1.230***	1.142***
	(0.0784)	(0.0375)	(0.0301)	(0.0606)	(0.0257)	(0.0178)
Burglary	1.194	1.010	1.042	1.291***	1.148**	1.132***
	(0.247)	(0.129)	(0.0758)	(0.116)	(0.0735)	(0.0483)
Larceny	1.089	1.099**	1.091***	1.508***	1.286***	1.160***
	(0.0889)	(0.0408)	(0.0304)	(0.0757)	(0.0446)	(0.0316)
Motor Vehicle Theft	1.195**	1.240***	1.182***	0.825	1.090	1.081
	(0.0838)	(0.0757)	(0.0662)	(0.170)	(0.0998)	(0.0661)

This table contains IRR estimates for the birthday effect on male and female victimization rates for each crime type. All regressions include city fixed effects, 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 clustered at the city level in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

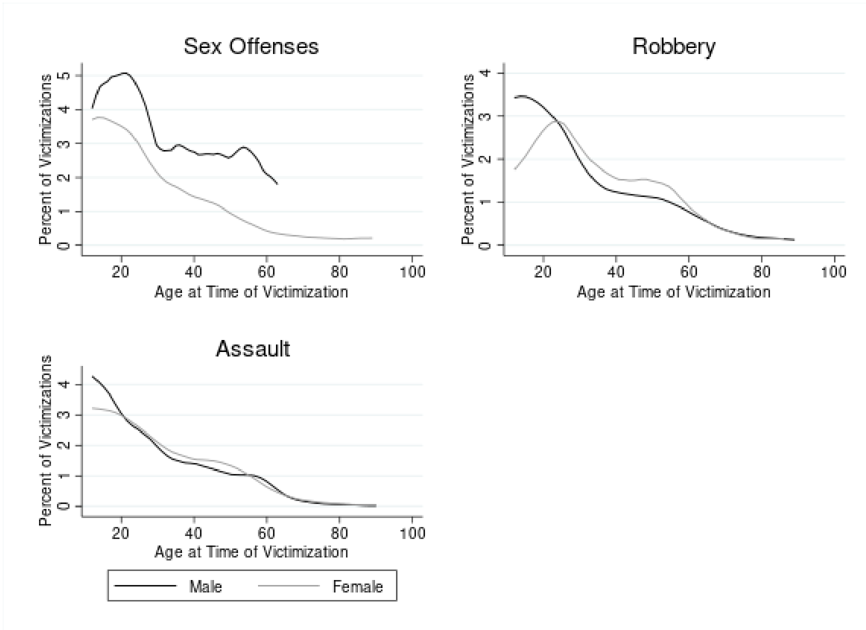
Table A13: Poisson RD Effects by University Schedule

	Male				Female			
	In Session		On Break		In Session		On Break	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent								
All	1.075**	1.074**	1.060	1.060	1.027	1.026	1.002	1.001
	(0.0323)	(0.0319)	(0.0399)	(0.0396)	(0.0227)	(0.0226)	(0.0314)	(0.0312)
Homicide	0.932	0.931	0.614	0.614	1.136	1.129	1.690	1.684
	(0.231)	(0.230)	(0.185)	(0.185)	(0.750)	(0.735)	(1.172)	(1.157)
Sex Offenses	1.813	1.809	1.651	1.652	1.250**	1.246**	1.234	1.231
	(0.680)	(0.678)	(0.815)	(0.815)	(0.131)	(0.129)	(0.162)	(0.160)
Robbery	1.042	1.042	1.134**	1.136**	1.040	1.039	1.029	1.028
	(0.0538)	(0.0540)	(0.0670)	(0.0672)	(0.0689)	(0.0686)	(0.0912)	(0.0911)
Assault	1.089**	1.087**	1.036	1.035	1.014	1.013	0.985	0.984
	(0.0416)	(0.0411)	(0.0529)	(0.0522)	(0.0245)	(0.0245)	(0.0324)	(0.0322)
Property								
All	1.094***	1.093***	1.034	1.033	1.136***	1.133***	1.067**	1.066**
	(0.0288)	(0.0281)	(0.0341)	(0.0338)	(0.0273)	(0.0258)	(0.0330)	(0.0325)
Burglary	1.045	1.043	0.991	0.992	1.088	1.087	1.173**	1.168**
	(0.0674)	(0.0670)	(0.0774)	(0.0774)	(0.0564)	(0.0563)	(0.0802)	(0.0790)
Larceny	1.095***	1.094***	1.027	1.025	1.160***	1.157***	1.075*	1.075*
	(0.0378)	(0.0374)	(0.0441)	(0.0436)	(0.0365)	(0.0344)	(0.0406)	(0.0401)
MV Theft	1.137**	1.134**	1.102	1.100	1.095	1.094	0.912	0.911
	(0.0708)	(0.0698)	(0.0831)	(0.0822)	(0.0660)	(0.0656)	(0.0668)	(0.0667)

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 university schedule. All regressions include second order polynomials in age fully interacted with an indicator for age over 21. Even numbered columns include indicator variables for the week around birthdays. Each observation is the total number of victims in each age (days) relative to the 21st birthday. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B: Supplementary Figures

Age Profile of Victimization: NCVS
Panel A: Violent Victimizations



Panel B: Property Victimizations

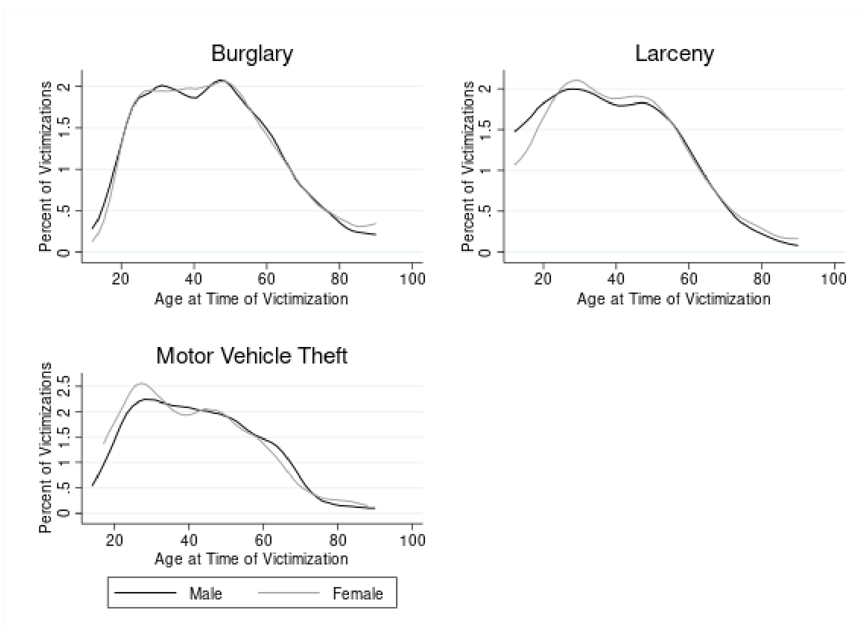
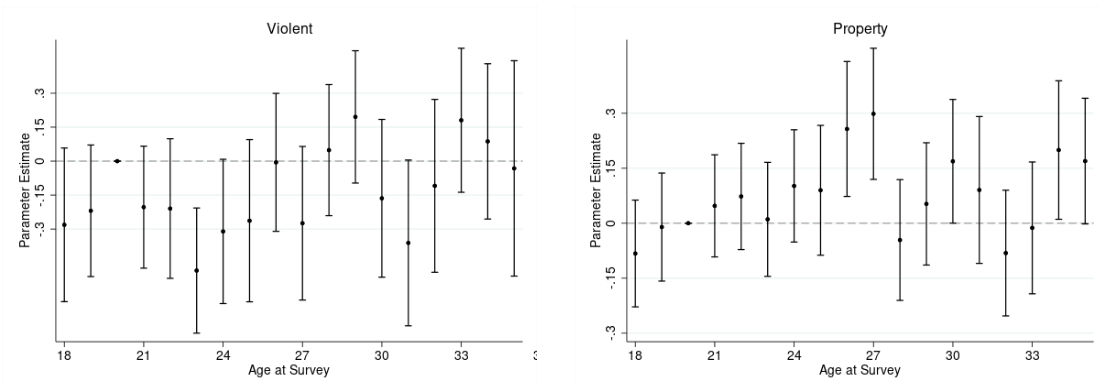


Figure A1: This figure contains local polynomial regressions of age in years at the time of survey on percent of victimizations at that age for the 2006-2016 waves of the NCVS. Each observation is the percent of all victimizations within crime type and gender that occur at a given age.

Reported Victimization by Age, Urban Victimizations Only

Panel A: Males



Panel B: Females

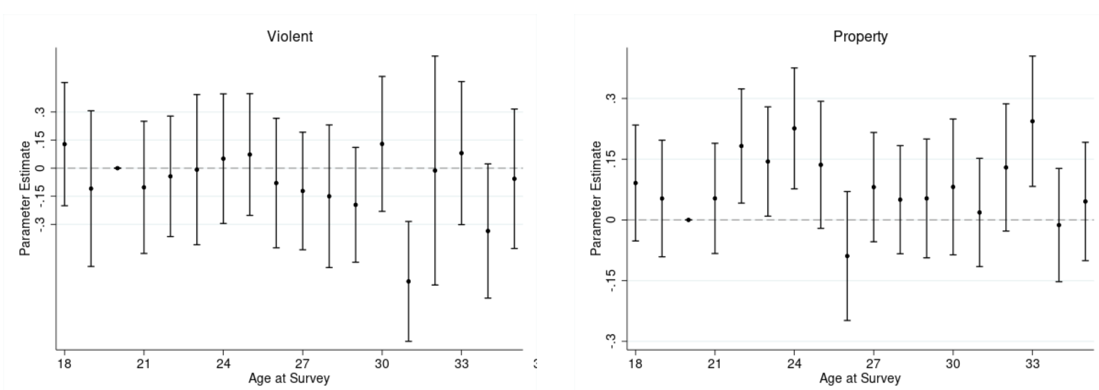
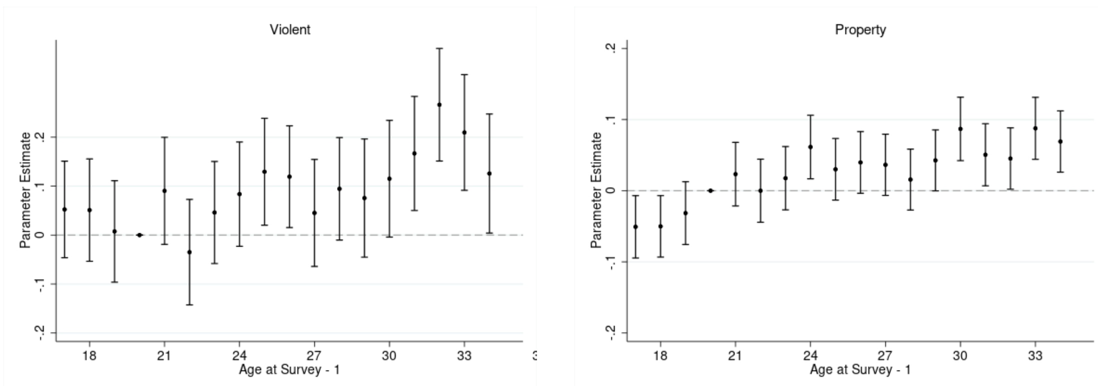


Figure A2: This figure presents estimates of β_k from $Report_{ikt} = \alpha + \sum_{18}^{35} \beta_k Age\_Ind_{ik} + \gamma_{tw} + \epsilon_{ikt}$ where i is individual, k is age in years at time of survey minus one year, and t crime type. $Report_{ikt}$ is an indicator variable that is 1 if an individual reported his or her victimization to the police and 0 otherwise. Age_Ind_{ik} is an indicator variable that is 1 if individual i was age k at the time of survey wave w. 95% confidence intervals included based on robust standard errors. Only victimizations where an individual was victimized in his or her home county and for whom the home county is urban are included. Estimates are based on the 2006-2016 waves of the National Crime Victimization Survey and relative to age 20.

Reported Victimization by Age, Using NCVS Age Minus 1

Panel A: Males



Panel B: Females

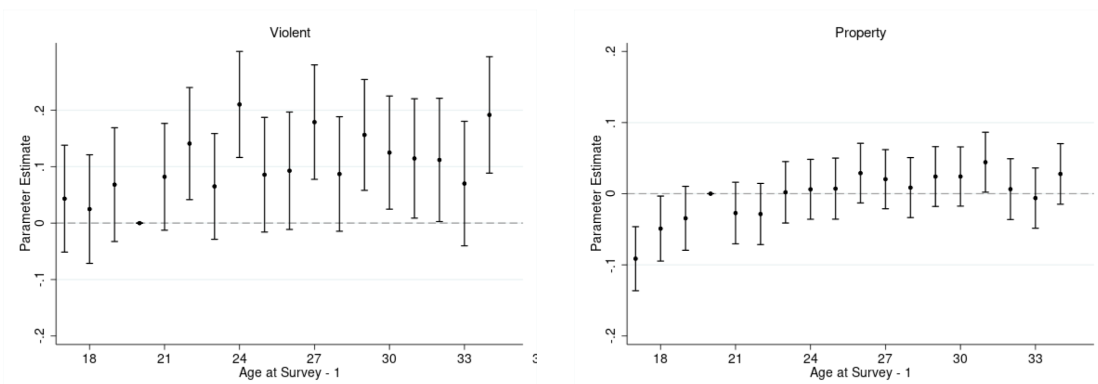


Figure A3: This figure presents estimates of β_k from $Report_{ikt} = \alpha + \sum_{18}^{35} \beta_k Age\_Ind_{ik} + \gamma_{tw} + \epsilon_{ikt}$ where i is individual, k is age in years at time of survey minus one year, and t crime type. $Report_{ikt}$ is an indicator variable that is 1 if an individual reported his or her victimization to the police and 0 otherwise. Age_Ind_{ik} is an indicator variable that is 1 if individual i was age k at the time of survey wave w. 95% confidence intervals included based on robust standard errors. Estimates are based on the 2006-2016 waves of the National Crime Victimization Survey and relative to age 20.

Placebo Test of Birthday Effect on Male Victimization

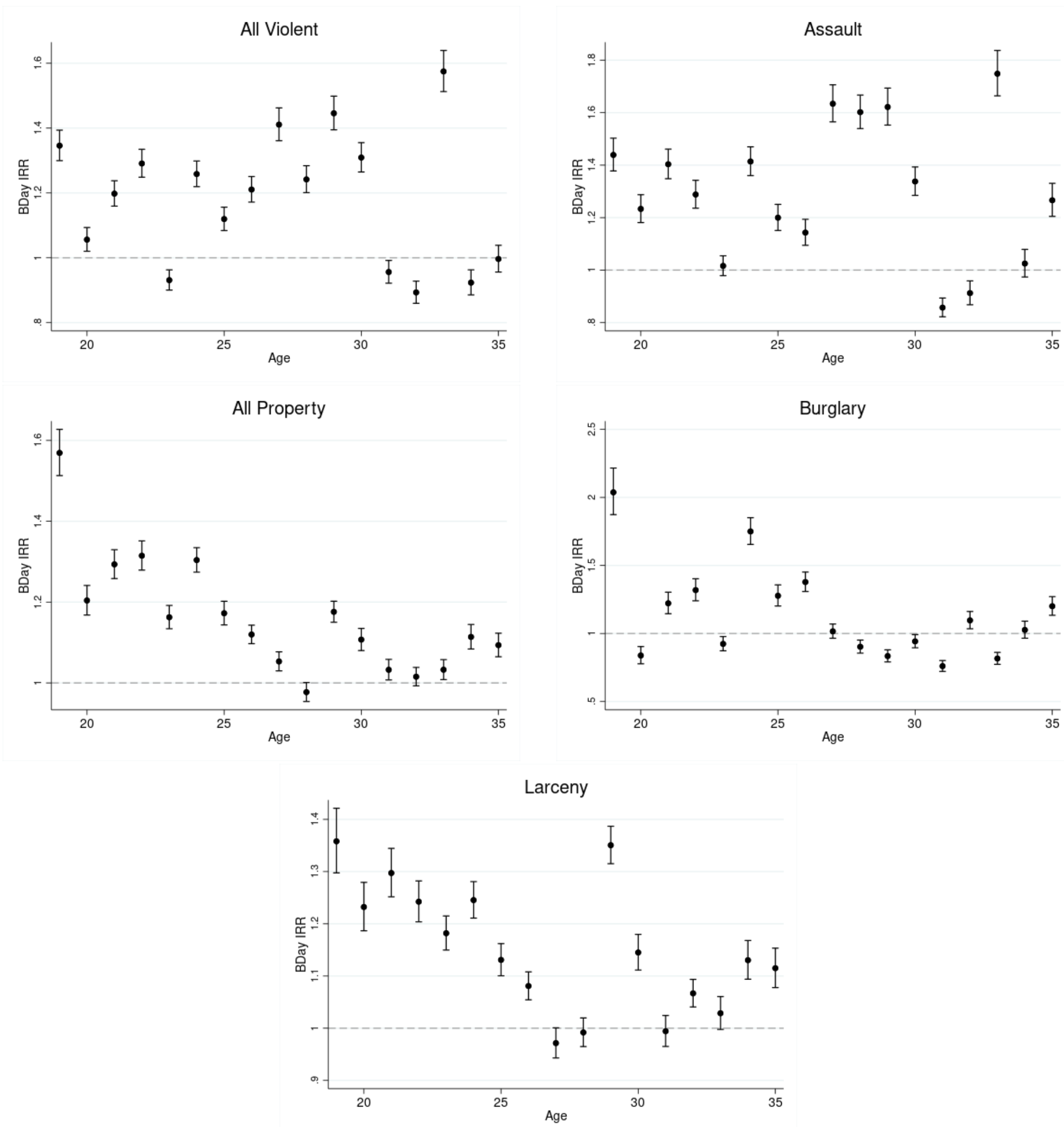


Figure A4: This figure contains IRR estimates for the birthday celebration effect for each age from 19 to 35 for male victims. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

Placebo Test of Birthday Effect on Female Victimization

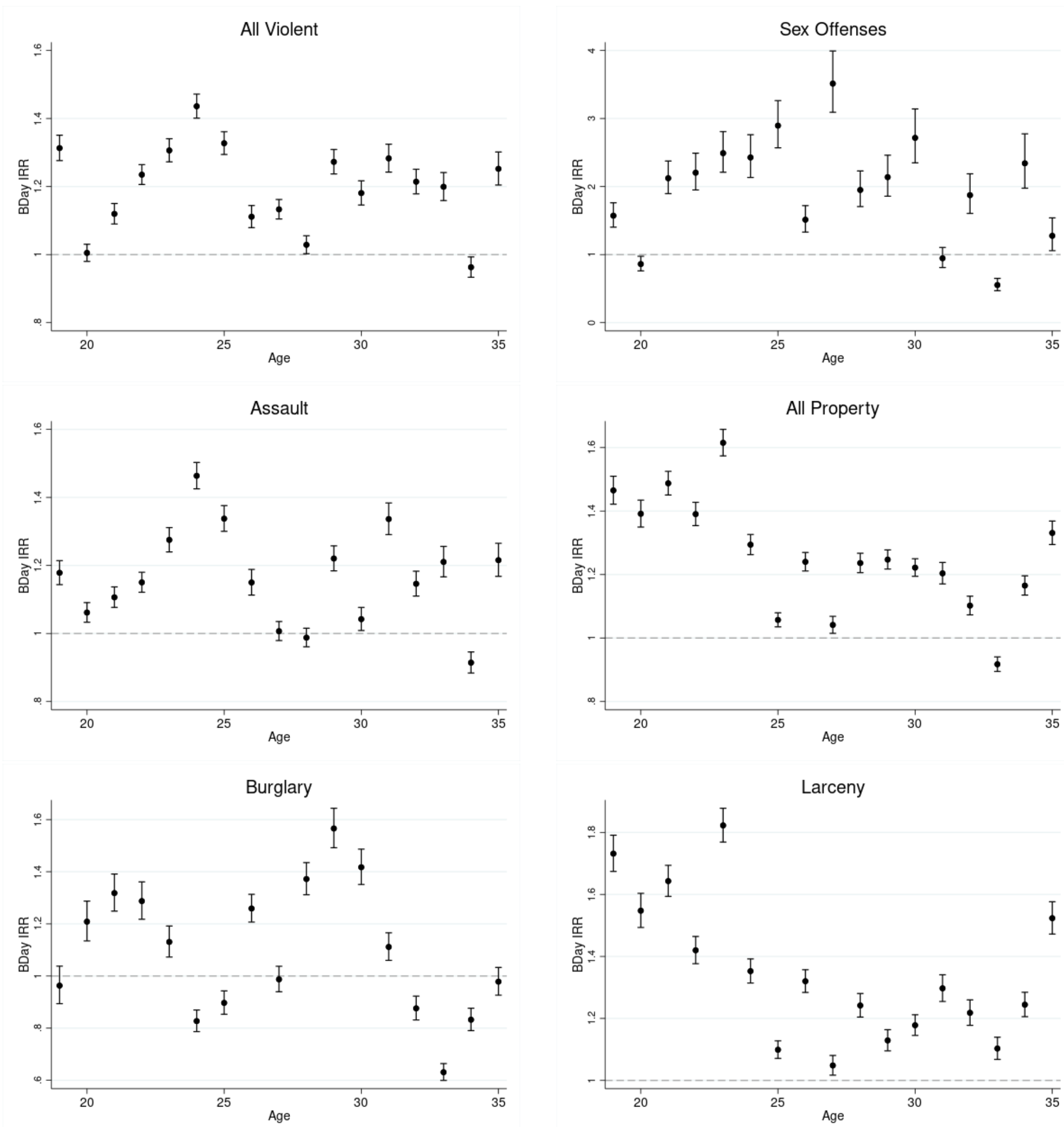


Figure A5: This figure contains IRR estimates for the birthday celebration effect for each age from 19 to 35 for female victims. Regressions include a second order polynomial in age fully interacted with an indicator for age over the cutoff age as well as an indicator for the exact birthday of the cutoff age.

Appendix C: 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 object 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 D: 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

Appendix E: Residential Locations, by Police Department

- Dallas: apartment complex/building, apt, condomin, foster home, mobile home, single family, residential property
- Denver: residence/home
- Houston: home/apartment, home/residence
- Milwaukee: offender residence, offender temporary, other residence, other temporary, victim residence, victim temporary
- St. Louis: apartment/condo, housing shelter, other residence, public housing, residence/home

Appendix F: NCVS Victim Counts, 2006-2016

Male Victims

Age at Survey	Report to Police?	All Violent	Sex Offenses	Robbery	Assault	All Property	Burglary	Theft	Motor Vehicle Theft
18	Yes	113	0	35	78	207	24	162	21
18	No	106	1	26	79	535	46	483	6
19	Yes	101	2	33	66	241	50	154	37
19	No	83	0	21	62	557	79	468	10
20	Yes	89	0	19	70	242	66	158	18
20	No	93	2	21	70	536	61	460	15
21	Yes	82	0	19	63	303	83	190	30
21	No	95	6	15	74	569	80	483	6
22	Yes	85	0	35	50	281	75	173	33
22	No	59	0	21	38	474	46	423	5
23	Yes	64	0	17	47	286	50	199	37
23	No	84	1	11	72	524	66	445	13
24	Yes	94	0	18	76	287	47	204	36
24	No	85	0	19	66	505	66	433	6
25	Yes	85	0	26	59	331	83	215	33
25	No	72	3	14	55	475	57	412	6
26	Yes	86	2	23	61	354	86	227	41
26	No	55	0	9	46	554	92	453	9
27	Yes	106	0	17	89	343	96	209	38
27	No	77	0	9	68	535	80	450	5
28	Yes	79	0	16	63	355	81	236	38
28	No	71	0	13	58	566	73	486	7
29	Yes	93	0	28	65	340	79	220	41
29	No	72	0	7	65	590	80	497	13
30	Yes	54	0	11	43	368	79	249	40
30	No	49	1	9	39	576	66	499	11
31	Yes	65	0	10	55	370	102	228	40
31	No	48	2	5	41	470	59	407	4
32	Yes	66	0	9	57	347	84	218	45
32	No	40	2	7	31	499	50	438	11
33	Yes	79	1	27	51	367	106	236	25
33	No	26	0	8	18	565	75	483	7
34	Yes	67	0	15	52	408	113	252	43
34	No	35	0	9	26	496	61	425	10
35	Yes	61	1	10	50	393	98	253	42
35	No	36	0	5	31	547	71	465	11

Female Victims

Age at Survey	Report to Police?	All Violent	Sex Offenses	Robbery	Assault	All Property	Burglary	Theft	Motor Vehicle Theft
18	Yes	103	7	14	82	207	38	154	15
18	No	102	20	5	77	527	51	473	3
19	Yes	89	10	19	60	261	58	176	27
19	No	99	26	9	64	496	72	418	6
20	Yes	85	5	12	68	310	78	209	23
20	No	73	16	6	51	522	64	448	10
21	Yes	103	11	18	74	355	83	237	35
21	No	117	28	11	78	527	68	445	14
22	Yes	104	8	22	74	366	96	229	41
22	No	87	19	5	63	594	73	508	13
23	Yes	93	2	24	67	375	104	240	31
23	No	61	15	7	39	603	88	501	14
24	Yes	113	3	29	81	409	111	243	55
24	No	92	15	11	66	583	77	496	10
25	Yes	124	10	21	93	442	104	293	45
25	No	60	5	8	47	634	82	545	7
26	Yes	86	8	16	62	413	102	271	40
26	No	71	6	10	55	613	78	527	8
27	Yes	83	15	10	58	503	118	323	62
27	No	66	7	3	56	643	72	561	10
28	Yes	99	11	26	62	496	108	327	61
28	No	50	8	8	34	670	58	596	16
29	Yes	90	12	14	64	456	127	297	32
29	No	74	17	10	47	661	77	573	11
30	Yes	110	9	23	78	487	140	302	45
30	No	64	9	6	49	638	94	538	6
31	Yes	96	10	15	71	477	123	300	54
31	No	72	12	4	56	633	64	564	5
32	Yes	82	3	18	61	514	110	348	56
32	No	53	7	8	38	646	84	555	7
33	Yes	71	7	8	56	431	85	313	33
33	No	55	11	9	35	637	82	545	10
34	Yes	67	6	8	53	432	118	284	30
34	No	55	5	5	45	662	99	555	8
35	Yes	91	2	17	72	474	129	302	43
35	No	47	8	4	35	596	92	498	6