Alcohol Use and Assault: Regression Discontinuity Evidence from the Minimum Legal Drinking Age

Maggie Hu

Professor Jeffrey DeSimone, Faculty Advisor

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Abstract

While it has long been observed that alcohol consumption is a risk factor for violence, the economics literature has up until recently provided minimal persuasive evidence regarding the causal nature of this relationship. In this study, we employ a regression discontinuity (RD) framework to examine how arrest and victimization rates from assault change at age 21, the U.S. minimum legal drinking age (MLDA-21). Utilizing annual FBI arrest data from the past 36 years since 1988, when the last states adopted the MLDA-21, we estimate that for both males and females, reaching the MLDA increases arrest rates for aggravated and other simple assaults by 5 – 8%, with the aggravated assault effect for females restricted to the latter half of the sample period. Analogous effects at slightly older ages are small and insignificant, as well as the effects for demographic and population characteristics expected to trend smoothly across the MLDA-21 threshold. We extend our analysis of assault-related violence by assessing victimization outcomes, particularly the effect of the MLDA-21 nonfatal injury, by leveraging emergency department (ED) data from the CDC's Web-based Injury Statistics and Query Reporting System (WISQARS) spanning the period 2001–2022. Notably, we observe that ED visits for "struck by or against" assaults rise significantly by 7–10%, indicating increased participation in violent altercations and increased risk of victimization upon obtaining legal access to alcohol. Taken together, these results suggest that alcohol use increases aggression and violent behavior, the consequences of which thereby represent criminal justice and public health costs that would be exacerbated by lowering the MLDA.

JEL classification: I18, I12, K0, K32

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1. Introduction

Alcohol regulation around the world has been a subject of contentious policy debate, driven by conflicting concerns over public health, crime, and individual freedoms. While the consumption of alcohol is rooted in nearly all cultures and practices, it is widely recognized as a catalyst of social harm and a precursor to numerous health conditions. Perhaps a byproduct of this duality, there exists little consensus on the appropriate age at which individuals should be granted legal access to alcohol. Global policies regarding purchase and consumption of the substance vary significantly, with countries like Wales setting a minimum legal drinking age of five years under certain conditions[1] and many European countries upholding 16- to 18-year-old age limits.[2] Several countries—including Saudi Arabia, Afghanistan, and Sudan—ban the sale and consumption of alcohol in its entirety.[3] Presently, our study focuses on the United States, where the minimum legal drinking age of 21 (MLDA-21), considered to be one of the stricter limits compared to other developed nations. While various cultural, religious, and political differences influence these policies, they all demonstrate an overarching recognition of the dangers of early alcohol access and the need for regulation.

The World Health Organization (WHO) reports that alcohol consumption remains a major driver of both morbidity and mortality worldwide. Recent data show that 2.6 million deaths per year—4.7% of all global deaths—are attributable to alcohol consumption, with 400 million individuals living with alcohol use disorders in 2019 (WHO, 2023). Notably, 13% of these alcohol-attributable deaths were among young people aged 20–39 (WHO, 2023). Alongside chronic health risks, alcohol consumption is strongly associated with social, mental,

[1] In Wales, five-year-olds can drink under supervised conditions in a private setting.
[2] Other European countries like Austria, Belgium, and Switzerland offer on-and-off premise consumption and sales of alcoholic beverages to 16-year-olds (IARD).
[3] Islamic law deems alcohol “haram,” or forbidden.

and psychological consequences, extending even to non-drinkers who become victims of alcohol-related aggression (WHO, 2002). A significant body of economics and public health literature has studied this relationship, linking drinking to violent crime, impaired judgment, and heightened risk-taking behavior (Cooke et More 2000; Carpenter, 2009). Studies have been conducted in numerous settings, from Scandinavia to Australia, finding that policy restrictions, broadly speaking, have public health benefits for youth and society at large (Carpenter & Dobkin, 2012, Carpenter & Dobkin 2005, Lindo et al, 2015). Presently, we aim to contribute to this growing area of research by investigating how the U.S. federal minimum legal drinking age policy of 21 affects criminal arrests and morbidity of non-fatal injuries.

Within the setting of our study, the United States, sentiment towards alcohol has been expressed through various policy levers designed to minimize the substance's social and health consequences. Historically, this has included regulations on where, when, and to what extent alcohol can be legally consumed. Importantly, in the 1970s, following the passage of the 26th Amendment and the reduction of the voting age to 18 years, numerous states experimented with reductions in their minimum legal drinking ages (Toomey, 1996). Research quickly exposed that the repercussions of these changes were detrimental: youth drinking rates and alcohol-related motor vehicle accidents in the period right after increased significantly, particularly among teens and young adults (Douglass et al. 1974; Wagenaar 1993; Whitehead 1977). A combination of headline stories about "blood border"[4] incidents and public outcry eventually gave way to legislative response (FTC). Thus, a defining moment in modern alcohol policy came with the passage of the National Minimum Drinking Age Act of 1984, which required states to raise the Minimum Legal Drinking Age to 21 or face cuts of up to 10% in federal highway funding (U.S.

[4] "Blood Border" cases refer to highly publicized instances where youth drove to other states with lower MLDAs to drink lawfully, only to crash on their way home (FTC).

Congress. House). By 1988, all 50 states had adopted the MLDA-21, marking the implementation of a national-wide effort to curb the detrimental outcomes related to alcohol access (Bonnie & O'Connell, 2004).

Despite the establishment of the MLDA-21, alcohol consumption and abuse remain prevalent among youth in the United States. According to the 2023 National Survey on Drug Use and Health (NSDUH), nearly 50% of adults aged 18 to 25 reported drinking alcohol in the past month, including 4.4 million full-time college students (SAMHSA, 2023). Among all alcohol-related harms, unintentional injuries and deaths remain particularly striking: an estimated 1,519 college students aged 18 to 24 die annually from alcohol-related unintentional injuries, including motor vehicle crashes (NIAAA, 2023). Furthermore, alcohol-related emergency department visits represent a significant public health burden. Recent data indicates that alcohol-related ED visits among young adults results in approximately $3.6 billion in healthcare costs annually, with assault-related injuries accounting for a substantial portion of these visits (CDC, 2021). Alongside physical health risks, alcohol is implicated in criminal behavior through multiple pathways, such as reduced inhibitions, impulsivity, and aggression. In the U.S., roughly 40–45% of homicides and physical assaults involve alcohol use by the offender (BJS). Young drinkers are especially prone to risky behaviors—impaired judgment and peer pressure can quickly lead to fights and vandalism, which in turn, can influence future outcomes.

Given the substantial consequences, the effectiveness of the MLDA-21 has been widely studied in the economics and public health literature. Studies suggest that alcohol restrictive policies reduce consumption among young adults and subsequent alcohol-related harms, including traffic fatalities, hospitalizations, and death (Carpenter & Dobkin 2005, 2009, 2011, 2015, 2017). Still, the law has not been without pushback. In July of 2008, a consortium of over

100 university presidents and chancellors acted under the Amethyst Initiative, calling for a re-examination of the national MLDA-21. Their central argument was that a lower drinking age would encourage safer, regulated consumption rather than fostering an illicit culture of excess (Choose Responsibility, 2008). Critics of the policy continue to push that raising the drinking age pushes alcohol consumption underground, leading to riskier drinking behaviors — particularly binge drinking among college students and young adults — defined as consuming five or more drinks for males and four or more drinks for females on a single occasion. This behavior was reported by 9.8 million young adults (28.7% of this age group) including 29.3% of full-time college students (NSDUH, 2023). More recently, although states have attempted to enact legislation that would decrease the MLDA, none have successfully been implemented to this date.

Although prior work has documented the increase in arrests for criminal offenses at age 21 (Carpenter & Dobkin, 2015), relatively few studies have evaluated morbidity and victimization. Overlooking these common outcomes has costly consequences in terms of the labor market, healthcare systems, and individual outcomes (Chalfin et. Al, 2023). To address this gap, the present study investigates how turning 21—the point of obtaining legal alcohol access—impacts both assault perpetration and assault-related injuries to present a comprehensive analysis of the influence of alcohol on violent interactions. By utilizing a regression discontinuity design in on the United States, we identify the causal effects of MLDA-21 on violent crime. This approach examines both sides of violent interactions, finding that alcohol-related violence is driven by mutual participation in violent fights rather than premeditated crimes. Our findings suggest that violence related assaults and hospital stays increase significantly at age 21, indicating that obtaining alcohol access near the MLDA threshold increases the likelihood of both committing and being a victim of assault.

The remainder of the paper will continue as follows: Section 2 will provide an overview of the existing literature on alcohol regulations and empirical findings of their effects on crime and public health outcomes. It will provide the relevant context to introduce the setting of our quasi-experimental approach. Section 3 will describe our data sources and the unique national level construction of our sample. Section 4 will outline the empirical framework underlying our RD design. Section 5 provides descriptive statistics of our data before presenting the main results. Finally, section VI offers a discussion of the findings, acknowledging limitations and offering concluding remarks for future research and policy considerations.

2 Background and Literature Review

Previous studies have investigated alcohol use and rates of crime in similar settings. Greenfeld (1998) found that over a third of convicted offenders in the U.S. had consumed alcohol at the time of their crime, demonstrating the prevalence of alcohol in crime. However, the mechanisms and causal pathways influencing this relationship remain a subject of debate. One body of research examines the most direct pathway that alcohol consumption leads to crime, namely, its pharmacological effects: the substance can heighten aggression in individuals by reducing inhibitions, increasing the likelihood of impulsive or emotionally charged responses (Fagan, 1990; Pernanen, 1981; Carpenter, 2012). These physiological effects are especially relevant for younger individuals who already exhibit lower levels of self-control and a greater propensity for risk-taking (Lipsey et al., 2002). Accordingly, there is an increase in interpersonal violence and a greater likelihood of victimization, in which location can often play a role. Alcohol-serving venues such as bars and nightclubs provide settings where risky social interactions can further facilitate crime (Carpenter, 2012). As such, this field of work has laid the groundwork for arguments that alcohol access—particularly at legal thresholds such as the MLDA-21—might directly contribute to violent outcomes.

Early research sought to exploit state-level variation in alcohol taxes and prices, as well as the differential adoption of the MLDA-21 following the federal mandate, to identify causality. Given the high levels of heavy drinking and risk (DHHS, 2000l; Johnston, O'Malley, & Bachman, 2002), studies have focused on youth extensively, finding that higher taxes or prices reduce both frequency and intensity of youth drinking, with effects often strongest among heavier drinkers (Grossman, Coate, & Arluck, 1987; Coate & Grossman, 1988; Laixuthai & Chaloupka, 1993). In a study on youth zero tolerance laws, Carpenter (2007) exploited variation in state rollout to examine how stricter underage drinking enforcement reduced heavy drinking and crime among 18-20-year-olds. The findings were that alcohol resulted in a 13% reduction in binge drinking among young men and a 5% reduction in property and nuisance crimes, though no significant effect on violent crimes. Carpenter and Dobkins (2009) similarly found that heavy episodic drinking increases at 21, leading to higher rates of alcohol consumption at both the extensive and intensive margin, in addition to risky behavior and outcomes such as traffic fatalities.

Critics have also raised endogeneity concerns regarding state adoption of drinking-age laws (Miron, 2007) and the relatively weak influence of tax instruments (Chaloupka 1998; Carpenter, 2012). As temporal and cross-state variation in alcohol policies exhausted itself, regression discontinuity methods emerged as an alternative source of causality, exploiting the sharp legal age cutoff at 21. Carpenter and Dobkin (2009) first analyzed age-based restrictions leveraging the MLDA-21 and its effect on consumption and mortality, finding significant increases in heavy drinking and 9% increase in death, driven in large by alcohol-related causes. Leveraging a similar regression discontinuity design, Lindo et. al evaluated the 18 year old MLDA in New South Wales, Australia, to study motor vehicle accidents while making use of a unique zero BAC limit in the setting.

Subsequent work by Carpenter and Dobkin (2015) used California's universe of crime data to show a 5.9% increase in total arrests at age 21, with disproportionate increases in assaults, DUIs, and disorderly conduct. Disaggregating by crime type, they found that violent crime (aggravated assault and robbery), alcohol-related offenses (drunkenness, DUI), and nuisance crimes accounted for most of the spike, while property and drug crimes saw much smaller increases. Similarly, Hansen and Waddell (2018) found evidence of increased assault and drunk driving using judicial records from Oregon, but no parallel rise in higher grade offenses like robbery or rapes. Combined, these studies results suggest a potential role of alcohol in facilitating impulsive and aggression driven behaviors, many of which our present study's findings support.

Up until recently, most of the economics literature on alcohol control has had a disproportionate focus on the offenders, the perpetrators of crime, neglecting relevant victim outcomes (Carpenter 2012). The pharmacological effects of alcohol make separating the effects of crime commission from its effect on criminal victimization particularly difficult; individuals can become easier targets of crime, leading them to riskier situations. However, alcohol can also increase impulsive behavior, reducing cognitive functioning and altering judgement. Early studies were limited in their abilities to disentangle these pathways, but the increased availability and granularity of data regarding mechanism and direct outcomes has opened the door to more detailed analyses (Carpenter, 2012). Chalfin et al. (2023) sought to address this gap in victimization by examining the impact of alcohol access at age 21 on crime victimization, finding that both violent and property crime victimization increase at the MLDA threshold. Their estimates showed particularly strong effects for sexual assault and public space violence, suggesting that alcohol consumption not only spurs aggressive behavior, but also impairs self-defense mechanisms and increases exposure to risky environments. Importantly, they ruled out simple “birthday celebration” effects, arguing instead that legal access to alcohol elevates both the likelihood of perpetrating crime and of being victimized, resulting in a more costly overall picture of crime. Our study makes use of the increased availability of high-quality administrative data to address prior limitations in research on victimization mechanism.

Another understudied area in the alcohol literature relates to nonfatal injuries. While mortality from alcohol—particularly drunk driving fatalities and weapon induced deaths— is often the center of policy discourse, morbidity represents a far more common and costly consequence. Nonfatal incidents can often lead to serious outcomes, imposing grave burdens on individuals, healthcare systems, and society at large, as seen from increased U.S. hospital expenditures (11%), despite declining inpatient and ED volumes during the COVID-19 pandemic (Kaiser Family Foundation, 2023). In 2021, nearly 60% of inpatient hospitalizations were preceded by an ED visit, and 18% of U.S. adults had visited the ED in the past year, with alcohol-related assaults forming a significant portion of these encounters, especially among those aged 18-24. Given that ED visits for injury average over $1,500 in charges and disproportionately affect young adults (CDC, 2021), morbidity represents a major externality of alcohol consumption, borne not only by the consumer but also the broader healthcare systems.

Limited studies on morbidity have used quasi-experimental methods to evaluate the causal relationship between morbidity and alcohol. Studies by Lindo, Siminski, and Yerokhin

(2016) and Carpenter and Dobkin (2017) have provided early morbidity estimates from MLDA policies. Carpenter (2017) found that emergency department visits in the U.S. rose by 71.3 per 10,000 person-years and inpatient hospital admissions increased by 8.4 per 10,000 at the MLDA threshold, largely due to accidental injuries, alcohol overdoses, and injuries inflicted by others — including assaults.

2.1 Contribution of the Present Study

Our current study advances the literature by building on key theoretical and empirical insights with a regression discontinuity design to evaluate both the arrest and victimization components of alcohol-related assault. We combine aggregate arrest data on assault with assault-related ED visit data to capture the dual nature of violent altercations. Our addition of nonfatal ED visits as an outcome addresses causal determinants of both criminalization and victimization. Further, we leverage nationally reported data from both the FBI (for arrests) and the CDC WISQARS (for ED injuries). While previous work relied on state-specific administrative records or crime data from a select number of police jurisdictions (California, Oregon, North Carolina, etc.), our study draws from aggregate public datasets at the national level.

Taken together, our study offers a comprehensive evaluation of alcohol's role in violence and extends the policy debate on whether—and how—the MLDA-21 effectively prevents societal harm. By demonstrating that legal access at age 21 increases assault and victimization rates, we aim to show that lowering the MLDA would generate potentially harmful consequences for emergency room burden, law enforcement, and policy.

3. Data Description

3.1 Institutional Setting and Context

In the United States, legal access to alcohol begins at age 21 under a minimum legal drinking age policy. As discussed in the previous sections, studies have demonstrated that turning 21 leads to discrete increases in both alcohol consumption and crime commission of a variety of offenses (Carpenter & Dobkin, 2009, 2011). While the connection between alcohol and crime has been explored extensively, much of the existing research has primarily focused on the perpetration of crime and overlooked the likelihood of victimization, omitting key components of its full social consequences. Additionally, many analyses prioritize discussions on mortality, especially from motor vehicle accidents and homicides, rather than morbidity, representing another gap in our understanding of alcohol's societal impact we seek to fill.

Our study draws from two primary sources of data to capture both sides of assault: crime commission data from arrests and nonfatal injury data from emergency departments. Our arrest data comes from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) Program. We gather emergency department (ED) visits for assault-related injuries from the Centers for Disease Control and Prevention's (CDC) Web-based Injury Statistics Query and Reporting System (WISQARS) nonfatal injury database. In the following section, we will outline our primary data sources, describing how we construct key variables and acknowledging the limitations of our datasets in measuring the relationship.

3.2 FBI Uniform Crime Reporting (UCR) Program Arrest Data

We obtain a universe of arrest data from the FBI's Uniform Crime Reporting program, which collects detailed, incident level information from over 16,000 participating agencies, including state, county, city, university/college, and tribal agencies. As of 2023, these datasets record over 14 million criminal offenses, covering a combined 94.3% of the U.S. population (FBI, 2024). Historically, these data were reported via the Summary Reporting System (SRS), which aggregates incidents by offense category and demographic characteristics, including age, sex, and race. From 2021 onward, the FBI began a phase out of the SRS in favor of the National Incident-Based Reporting System (NIBRS), an incident-level framework that collects more detailed information about victims, offenders, and circumstances surrounding the crime. Since not all agencies have transitioned to NIBRS reporting to date, our data sample, from 1988-2023 includes both types of submissions. We focus on the period after all states adopted the MLDA-21, thereby allowing for a consistent policy environment.

For the scope of this study, we utilize UCR arrest data spanning 1988 to 2023 in order to capture the period after which all states adopted the MLDA-21. Our crimes of interest include several "index crimes," which are those categorized by the FBI as the most serious offenses that can be committed. These include murder and nonnegligent homicide, forcible rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, and more recently, arson (EBSCO/McKnight, 2024).

Table 1: Descriptive Statistics by Offense and Gender, 1988–2023

	All	Male	Female
Panel A: Violent Arrests (per 1,000)			
Forcible Rape	0.235	0.459	0.008
Robbery	1.525	1.631	0.292
Aggravated Assault	2.397	5.414	0.292
Other Assault	8.982	12.949	1.316
Weapons Offense	1.991	3.667	0.288
Panel B: Property Arrests (per 1,000)			
Burglary	2.043	5.721	0.225
Larceny	11.679	12.043	8.896
Motor Vehicle Theft	1.676	1.864	0.469
Vandalism	2.855	2.866	1.695
Panel C: Alcohol Arrests (per 1,000)			
Driving Under the Influence (DUI)	7.469	12.100	2.773
Liquor Law Violations	7.472	10.937	2.491
Drunkenness	3.160	4.871	0.848
Disorderly Conduct	6.067	6.246	0.987
Vagrancy	0.187	0.246	0.077
Panel D: Drug Arrests (per 1,000)			
Possession	2.100	3.190	0.821
Sale	1.292	2.594	0.767
Panel E: Other Arrests (per 1,000)			
All Other Offenses	26.291	41.084	17.229

Notes: Arrest rates are estimated per 1,000 population for individuals aged 20–21. Offense categories follow FBI Uniform Crime Reporting.

Table 1 presents descriptive statistics on select offenses, providing average arrest rates for individuals just under the MLDA threshold, prior to turning 21. Although our present analysis of results will focus on assault, we report major offense categories reported by the FBI Uniform Crime report in Table 1 to maintain comparability with prior studies and to contextualize the frequency of assault within the crime landscape. Importantly, we disaggregate by gender to account the differential crime behavior patterns between the two. Unsurprisingly, males are arrested at higher rates for many violent crimes, with approximately 5.4 arrests for aggravated assault recorded per 1,000 person years and similar results for male rates of property crime commission. Still, even with female arrest rates being on average, lower for these offenses, they are not negligible. A full list of the crimes captured in our dataset, as well as their respective FBI codes, can be referenced in Appendix Figure 1.1.

3.2.2 FBI Reporting and Sample Construction

Several important considerations arise in our data that could influence our results. The UCR Program reports arrest data for single year ages from ages 15 to 24, after which they are grouped into five-year intervals, from 25–29, 30–34, etc. This means we only have individual year level data up to 24 years of age. To account for counts beyond 24, we convert the counts for the 25-29 bin into single year proxies by dividing the estimates by 5, the number of years they account for. Given that grouping of arrest counts occurs far enough away from the cutoff, we are not too concerned about the results. To best interpret our single year age bins, we assign each to a midpoint – for example, 18 is coded as 18.5 – under the rationale that individuals spend approximately half of each calendar year at that exact age. Thus, our analysis focuses on individuals aged 18 and up, comparing the average arrest rate between 20-22 for the primary RD estimations around age 21.

Since law enforcement participation in UCR is voluntary, some agencies may have provided incomplete coverage or choose to report some years and not others, leading to biases and potential measurement error in total arrests. Given that not all agencies report consistently to the program, so those that do not submit data for at least six months in a calendar year are excluded from our sample. We further account for non-reporting by standardizing counts by coverage-adjusted population denominators, as we cannot assume nationwide coverage from the reporting agencies nor guarantee the same ones will report each year. Further, it is important to consider that enforcement and policing practices might differ across locations or over time, biasing estimates if a spike in arrests at age 21 reflects, for instance, differential policing of young adults. Such variations in policing intensity could inflate or deflate arrests independently of actual changes in violent behavior. Regardless of these limitations, the UCR Program remains the most comprehensive national repository for data on arrests in the U.S.

3.3 CDC WISQARS Nonfatal Injury Data

To capture victimization and injury outcomes, we turn to the Centers for Disease Control and Prevention's (CDC) Web-based Injury Statistics Query and Reporting System (WISQARS), spanning 2001 to 2022. These data are derived from the National Electronic Injury Surveillance System–All Injury Program (NEISS-AIP), which tracks ED visits at approximately 100 hospitals nationwide[5]. This stratified sample is intended to be representative of all U.S. hospitals, collecting information on the mechanism of injury, situational context of the injury, demographics of the patient, and treatment disposition. A more detailed breakdown of the causes of nonfatal injury can be referenced in Appendix Table 2A.

Given that NEISS-AIP is designed to provide injury surveillance rather than reported crime, it is less sensitive to variation in local policing, helping address previous concerns about under-reporting or enforcement. However, one limitation of the ED data is the absence of information on several interesting socioeconomic and health outcomes such as educational status, employment, hospital availability, and more. In addition, the aggregate nature of the data prevents us from analyzing finer variation in coverage and outcomes. For example, hospital availability and accessibility can differ by region and the socioeconomic status of the individual may affect the willingness of an injured individual to seek treatment, potentially leading to disparities in ED admission.

[5] WISQARS nonfatal injury data are generated from a stratified probability sample of U.S. hospital EDs and scaled to produce national estimates. Variables are constructed based on ICD-9-CM and ICD-10 external cause codes assigned at intake.

3.3.1 Defining Assault-Related Injuries

We gather CDC WISQARS data that include injuries treated in U.S. hospital EDs between 2001 and 2022, including national estimates stratified by age, sex, injury mechanism, intent, and discharge status. To isolate victimization events consistent with violence, we apply several data filters to identify assault-related emergency department visits from cause-of-injury codes (PCAUSE). These specify whether injuries are caused by assault, self-harm, or unintentional. As such, we focus on violence related injuries, which includes confirmed and suspected assaults, for our assault-related outcomes and disregard unintentional injuries and self-harm. Within these assault related injuries, we consider their mechanism: “struck by and against” (physical fighting), sexual assault, and other assault mechanisms. All struck by/against mechanisms isolate assaults via physical striking, while sexual assaults include all sexual assaults, regardless of mechanism. Sexual struck by/against capture sexual assaults where the injury mechanism was being struck by or against another person or object.

To calculate victimization rates, we use age-specific population denominators from Census data, comparable across intent types and mechanisms of assault, to determine whether the spike at MLDA-21 arises from general violence or forms of assault. We normalize by age- and year-specific population estimates from the U.S. Census. Our incidence measures are annual assault-related ED visit rates for 12- to 29-year-olds, focusing on the 20–21 average. Importantly, ED data capture only a subset of all assaults. Assaults not treated in the ED, whether due to underreporting, lack of access, or other care pathways, are not observed in this dataset. Thus, our analysis reflects the only the incidence of treated assault victimization, as it understates the true population rate of victimization.

Building off the existing literature, our present study uses crime victimization and hospitalization data to examine whether changes in reported violence are a result of changes in monitoring or reporting or actual changes in criminal behavior. By combining UCR arrest data with the CDC's WISQARS nonfatal injury data, our approach addresses both offender and victim outcomes at the legal drinking threshold. By observing consistent RD patterns in both arrests and ED admissions near age 21, we provide comprehensive evidence that these shifts reflect actual behavior tying alcohol consumption to crime. However, we also acknowledge that our datasets come with limitations, and both are limited by their aggregate level nature. In the next section, we outline our empirical methodology, which uses a regression discontinuity design centered at age 21. This design compares individuals just below and just above the MLDA threshold to isolate the causal effect of legal alcohol access on assault perpetration and victimization.

4. Empirical Specification

4.1 Identification

To estimate the causal effect of alcohol access on crime, we employ a local regression discontinuity analysis that exploits the "sharp" discontinuity in alcohol access at the MLDA-21 (Thistlethwaite and Campbell, 1960; Hahn, Todd, and Van der Klaauw, 2002). In regression discontinuity designs, several identification assumptions must be satisfied in order to produce valid estimates. One of these is the absence of running variable manipulation. In other words, units cannot be manipulated in a way that prevents or ensures treatment assignment. Since all individuals are subject to the treatment age, 21, without exception, all individuals become legally eligible to purchase alcohol on their 21st birthday, theoretically leaving no way to sort themselves around that cutoff.

The second identification assumption of this design is local randomization: individuals on either side of a cutoff must be "interchangeable," meaning individuals just below 21 and just above 21 do not differ with respect to both observable and unobservable characteristics other than the gain in legal alcohol access. In the absence of the cutoff, we would expect all traits to vary smoothly across age, including things like demographics, risk taking behaviors, etc. If none of the determinants of arrest or an ED visit change discreetly at 21, variation in alcohol access can be considered as good as random near the MLDA-21.

To check whether the second identifying assumption holds, we estimate local regressions of several different population characteristics and coverage measures on either side of the cutoff, replacing the outcome variable of interest (arrests or ED visits, shown in Table 2 and Table 3 respectively). We confirm this result graphically as well (Appendix Figure).

Table 2: Covariate Smoothness – Arrests

Covariates	**Linear (h = 2)**		**Quadratic (h = 3)**	
	Male	Female	Male	Female
Panel A: Population Weights				
Adjusted Pop (1,000)	5.76 (13.6)	5.07 (13.2)	6.98 (26.2)	7.74 (25.2)
Panel B: Demographic Composition				
Opposite Sex Share	−0.0003 (0.0014)	0.0003 (0.0015)	0.0006 (0.0026)	−0.0007 (0.0029)
White Share	−0.0005 (0.0011)	0.0001 (0.0009)	−0.0005 (0.0023)	−0.0006 (0.0019)
Black Share	0.0004 (0.0011)	0.0005 (0.0009)	0.0005 (0.0023)	0.0007 (0.0019)
Asian Share	−0.0002 (0.0004)	−0.0002 (0.0004)	0.0000 (0.0007)	0.0001 (0.0007)
Hispanic Share	−0.0007 (0.0008)	−0.0002 (0.0007)	−0.0011 (0.0018)	−0.0004 (0.0015)

Notes: Each cell presents the RD estimate for the discontinuity in the listed covariate at the MLDA-21. Estimates are based on local polynomial regressions with triangular kernel weighting using either a linear specification (bandwidth $h = 2$) or a quadratic specification (bandwidth $h = 3$). Covariates include adjusted population counts and demographic shares for individuals aged 20–21. Robust standard errors are denoted in parentheses. No discontinuities are statistically significant.

Table 2 presents our estimates for population weights around the MLDA threshold and Panel B reports results for demographic shares, all of which we find no evidence of a discontinuity at the cutoff. Across all specifications, the point estimates are small in magnitude and statistically insignificant, indicating no evidence of a jump in population size or demographic composition at the cutoff. We observe that many of the coefficients are close to zero in both the linear and quadratic specifications for both males and females, suggesting that the proportion of men versus women is stable through the cutoff. The same pattern holds for racial/ethnic shares: the estimated effects for White, Black, Asian, and Hispanic shares are near zero and statistically indistinguishable from zero: a lack of a shift evident across samples and both polynomial specifications.

Table 3: Covariate Smoothness – ED Visits

Covariates	**Linear (h = 2)**		**Quadratic (h = 3)**	
	Male	Female	Male	Female
Panel A: Population				
Population	8812.8 (14559)	8496.1 (14292)	10464 (28474)	11335 (27656)
Panel B: Demographic Composition				
White Share	−0.00038 (0.00082)	−0.00039 (0.00065)	−0.00074 (0.00174)	−0.00083 (0.00138)
Black Share	0.00042 (0.00087)	0.0005 (0.00073)	0.00052 (0.00184)	0.00068 (0.00155)
Asian Share	−0.000047 (0.00029)	−0.000045 (0.0003)	0.00023 (0.00055)	0.00027 (0.00058)
Hispanic Share	−0.00076 (0.00105)	−0.00018 (0.00087)	−0.00109 (0.00232)	−0.00034 (0.00182)
Multiracial Share	−3.2e−06 (0.00011)	−5.3e−06 (0.00012)	−2.1e−05 (0.00023)	−1.2e−05 (0.00025)

Notes: Each cell reports the RD estimate for the discontinuity in the listed covariate at the MLDA-21, using either a local linear (bandwidth $h = 2$) or quadratic (bandwidth $h = 3$) specification with triangular kernel weighting. Covariates include adjusted population counts using WISQARS coverage and race/ethnicity shares. Robust standard errors are denoted in parentheses. None of the covariates exhibits a statistically significant discontinuity at the threshold.

We repeat this procedure in Table 3 for our WISQARS dataset on emergency department visits, estimating trends in population and demographic traits using both linear and quadratic specifications. Again, our estimates are consistently small, statistically insignificant, and stable across both male and female samples. Further visual inspection verifies little evidence of a jump around the cutoff. Thus, for both the arrest and ED datasets, we reach the same conclusion: individuals near age 21 trend smoothly with respect to demographic and structural characteristics. Observed jump in arrests or ED-visit rates for assault can be credibly attributed to individuals gaining legal access to alcohol at 21, rather than to changes in population composition or other confounding factors at the threshold.

Confirming the internal validity of our design, we turn to considering the functional form of our running variable. In principle, a polynomial of age on both sides of the threshold can capture the relationship between age and the outcome of interest. However, polynomial overfitting is a common risk (Gelman & Imbens, 2014). To account for this, we visually inspect varying orders of polynomials to determine which best matches our results (Appendix). Given that age is discrete and measured in years, high-order polynomials are unlikely to improve fit.

Our primary regression model is estimated as follows:

$$Y_{it} = \alpha + \beta_1 \, Legal_{it} + f(Age_{it} - 21) + Legal_{it} \times f(Age_{it} - 21) + \gamma X_{it} + \lambda_t + \epsilon_{it} \tag{1}$$

Here, Y_{it} represents the outcome of interest (arrest or ED inpatient rate) per 1,000 individuals *i* at time *t*. The key independent variable, $Legal_{it}$, is an indicator variable equal to one if the individual is above the MLDA-21 threshold ($Age_{it} \geq 21$) and zero otherwise. We allow the slope of the function, $f(Age_{it})$, to differ on each side of the cutoff by interacting it with $Legal_{it}$: $Legal_{it} \times f(Age_{it} - 21)$, accounting for the natural evolution of crime and pre-existing trends across years of age. In extended specifications, we replace $f(\cdot)$ with a polynomial of order 2 (p = 2) at varying bandwidths, following standard guidelines of the design (Lee & Lemieux, 2010).

Our model also includes a vector of control variables, X_{it}, to account for differences that that could affect arrest rates independently of the MLDA-21, including variables like demographic characteristics, population traits, economic conditions, and more. To further control for unobserved heterogeneity, we incorporate year fixed effects, λ_t, which account for time-varying factors**,** effectively absorbing national time trends and reduce noise. In subsequent specifications in which we pool regressions over specific time periods, I include fixed effects for each period. For subsample analyses, I adjust fixed effects accordingly: sample restrictions for ED assault related instances to 2001–2008 and 2009–2022 mean I include only the indicators for years 2002–2008 or 2010–2022. The error term, ϵ_{it}, is assumed to be independent and identically distributed.

Our parameter of interest in this model is β_1 , the discrete change in the arrest and ED rates at age 21, measure the causal effect of alcohol access on the outcome of interest. The preferred method for kernel weighting here is a triangular kernel, with observations closer to the cutoff of 21 assigned higher weight, decreasing linearly toward 0 at extreme ages to account for stronger counterfactuals at closer ages.

To address a potential concern arising from our analysis, mediator bias, we intentionally do not include certain endogenous controls. Mediator bias occurs when variables affected by the treatment itself—in this case, turning 21 and obtaining legal alcohol access—are included as controls, effectively mediating the effect of interest. To isolate the immediate causal impact of the MLDA threshold, we do not include post-determined controls that might change precisely because individuals gained legal access to alcohol. Instead, we focus on a minimal set of predetermined covariates, such as demographics and population traits, and rely on the assumption of local randomization around the cutoff. This approach helps preserve the internal validity of our RD design and prevents us from inadvertently taking out part of the treatment effect through other channels.

5. Main Results

In the following section, we document our main findings on the effect of reaching MLDA-21 on both arrest and victimization outcomes. While do not directly test the first stage responses to the MLDA-21 on alcohol consumption, we rely on the well-established literature for the causality of this relationship (Hansen and Waddell, 2018; Cooke and Moore, 2001).

5.1 The Effects of Legal Access to Alcohol on Crime Commission

For our analysis of crime specific outcomes to legal alcohol access, we estimate a symmetric window of three years on each side of the cutoff (h=3) using the quadratic form of our equation: this is is the minimum bandwidth that supports a second order estimation and avoiding pre-existing discontinuities at age 18. From Equation (1), we estimate the effect of reaching the minimum legal drinking age on rates of aggravated and other assaults by gender.

Table 4: MLDA-21 Effect on Arrests, 1988–2023

Panel A: Males (1988–2023)		
	Aggravated Assault	**Other Assault**
Point Estimate	0.442^{***} (0.108)	0.937^{***} (0.120)
Mean	6.24	14.00
Panel B: Females (1988–2023)		
	Aggravated Assault	**Other Assault**
Point Estimate	0.042 (0.025)	0.221^{***} (0.083)
Mean	1.51	4.75

Notes: Each coefficient is the local RD jump in arrests per 1,000 population at age 21 over the full period (1988–2023). Standard errors (in parentheses) are from `rdrobust` with a quadratic polynomial ($p = 2$), bandwidth = 3, and no covariates/weights. "Mean" is the average arrest rate (ages 20–21). Female aggravated assault (0.042) is not significant at conventional levels. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents our main discontinuity estimates for assault related arrests at age 21 for the full sample period, from 1988–2023. Among males, turning 21 increases arrests for aggravated assault by 0.44 per 1,000, representing a 7–8% increase above the 6.24 mean. Other (simple) assault arrests for males rise similarly by 0.94 per 1,000, about 7–8% above its baseline. For females, from 1988–2023, estimates show a significant increase of 0.22 per 1,000 (SE ≈ 0.083) for other assaults, indicating a 4–5% increase above the mean. However, aggravated assault arrest rates for females remain statistically insignificant over the full sample period.

Table 5: MLDA-21 Effect on Female Arrests by Period

	1988–2005	**2006–2023**
Point Estimate	-0.001 (0.026)	0.086^{**} (0.035)
Mean	1.66	1.35

Notes: Each coefficient is the local RD jump in *female* aggravated assault arrests (per 1,000 population) at age 21. Standard errors in parentheses come from **rdrobust** with a quadratic polynomial ($p = 2$), bandwidth = 3, no covariates/weights. "Mean" is the baseline arrest rate (ages 20–21) for each sub-period. Female aggravated assault is insignificant prior to 2006 but becomes significant afterward. Significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To investigate whether our effects are driven by recent years, rather than artifacts of the past, we divide our 36 years of arrest data into two periods: 1988 –2005 and 2006–2023. Among males, we observe that aggravated and other assault arrest effects remain statistically significant in both periods, but effect sizes are larger in the earlier sample (0.52 in the earlier period and 0.37 post 2006). Across our assault types of interest, patterns of aggravated and simple assaults remain relatively consistent across the periods, experiencing a general decline in crime rates in conjunction with U.S. trends. [6] Notably, our results indicate that in the latter half of our sample, from 2006 onwards, females exhibit a significant 0.086 (0.035) increase in aggravated assault arrests, suggesting a recent shift in prevalence of assault related crime for women.

[6] All estimates from the full and restricted sample can be viewed in Appendix Table 2.

Figure 1: Main RD Effect of MLDA-21 on Arrests

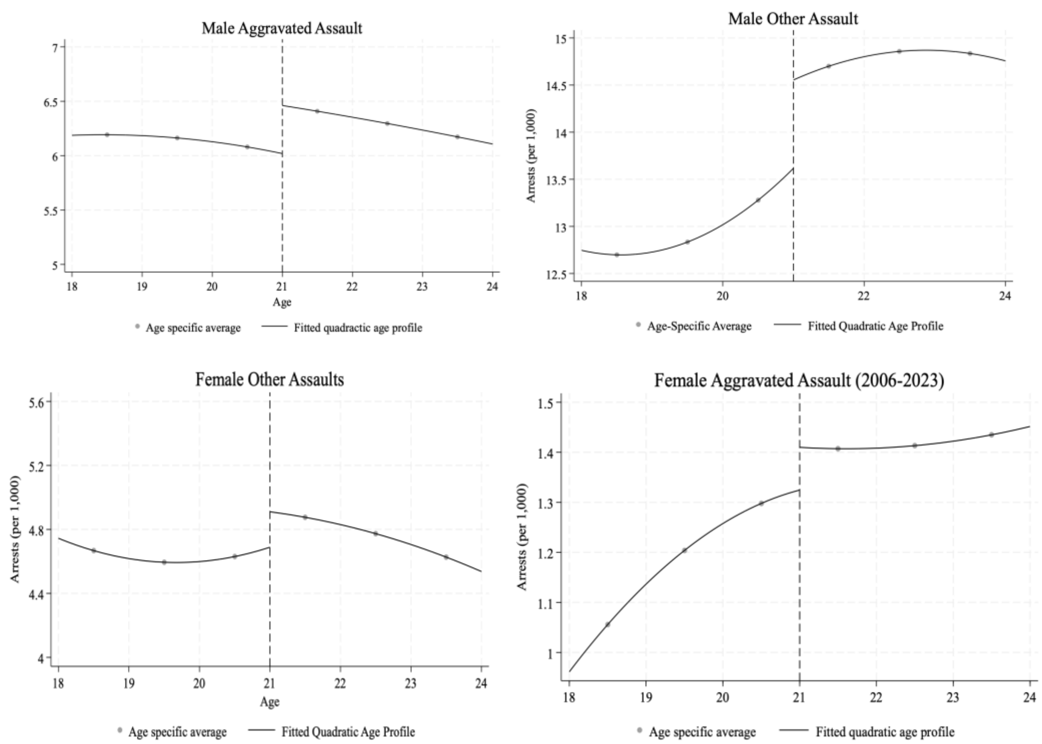


Figure 1 provides visual evidence of the significant discontinuities in assault arrests at age 21. Both males and females (the latter for other assault, and aggravated assault post-2006) show clear discontinuities at the MLDA threshold, consistent with a smooth age profile otherwise. The plotted local means and fitted curvature of the quadratic confirms the specification. These results attribute the discontinuous changes in arrests rates to the effect of the legal access to alcohol gained upon turning 21.

5.1.2 Alternative Bandwidth Sensitivity

To test the sensitivity of our preferred estimates, we test alternate linear and quadratic polynomials using varying bandwidths. Since age is discrete, we cannot employ the traditional local linear nonparametric approach (Imbens & Kalyanaraman, 2012) that relies on continuous data and bandwidth selectors. Instead, we consider windows around age 21 (aged 18–23 or 18–

29) and check whether for consistency. A larger bandwidth would account for a larger range of observations in the analyses, so checking sensitivity to choice of bandwidth is important. On one hand, more observations would enable greater precision, but concerns arise from too large of a window regarding the fit of local approximations. We replicate our regression analysis for our main findings using these specifications.

Table 6: Bandwidth Sensitivity: Effect of MLDA-21 on Arrests

Bandwidth =	**h(3 4)**	**h(3 9)**
Panel A: Aggravated Assault		
Males	0.427^{***}	0.448^{***}
	(0.100)	(0.104)
Females (2006–2023)	0.072^{**}	0.066^{**}
	(0.032)	(0.032)
Panel B: Other Assault		
Males	0.952^{***}	0.981^{***}
	(0.113)	(0.113)
Females	0.225^{**}	0.251^{**}
	(0.076)	(0.076)

Notes: Each cell reports the RD estimate (with standard error in parentheses) from a local polynomial of order 2 ($p = 2$), comparing two alternative bandwidths: **h(3 4)** and **h(3 9)**. Males use the 1988–2023 sample; female aggravated assault is restricted to 2006–2023, while female other assault is full-period. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 6, we present results for a bandwidth of three years prior to 21 and four years after (h=3,4), given the limits of FBI single year data on the upper end. Extending our analysis to nine years after reaching the MLDA-21 includes the 24–29-year bucket. These asymmetric specifications are representative of our data, excluding juveniles under 18 to avoid capturing any 18-year age discontinuities.

5.1.3 Robustness Checks

To further validate our main estimates, we conduct robustness checks using alternate specifications, re-estimating using log-transformed outcomes to address skewness in arrest rates, covariate adjustment with year fixed effects, log population adjustments, race/ethnicity shares, and population-weighted estimations.

Table 7: Robustness Checks – Log, Covariate, Weights

	Baseline	Log	Covariates	Weights	Mean
Panel A: Aggravated Assault					
Males	0.442^{***}	0.076^{***}	0.377^{***}	0.435^{***}	6.24
	(0.108)	(0.021)	(0.086)	(0.104)	
Females (2006–2023)	0.086^{**}	0.070^{**}	0.079^{***}	0.083^{**}	1.35
	(0.035)	(0.028)	(0.021)	(0.034)	
Panel B: Other Assault					
Males	0.937^{***}	0.066^{***}	0.891^{***}	0.931^{***}	14.00
	(0.120)	(0.015)	(0.115)	(0.122)	
Females	0.221^{**}	0.049^{**}	0.204^{***}	0.221^{**}	4.75
	(0.083)	(0.019)	(0.048)	(0.084)	

Notes: Each cell reports the RD estimate for the effect of reaching age 21 on arrest rates for aggravated and other assault. Models use a quadratic specification ($p = 2$) with triangular kernel. Baseline models use bandwidth $h = 3$; "Log" uses log of arrest rate as the outcome; "Covariates" include race/ethnicity shares; "Weighted" models apply population weights. Columns 6–7 use asymmetric bandwidths: $h = (3, 4)$ and $h = (3, 9)$. Means are the baseline arrests per 1,000 among 20–21 year-olds for each group. Male outcomes use the full sample (1988–2023). Female aggravated assault is restricted to post–2006, and female other assault covers the full period. Robust standard errors are reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

From Table 7, we observe that the main point estimates remain robust across a variety of models as well as narrower and wider age bandwidths. Log-transformed models yield similar elasticity estimates, roughly 7–8% for males and 4–5% for females, consistent with our baseline percentage changes. Including covariates, race shares, and weighting by population coverage of the UCR data also leaves our primary results for the most part, unchanged, as they remain substantively similar to our preferred estimates.

5.1.4 MLDA Effects on Other Crimes

Having considered the robustness of our main results, we next consider the effect of reaching the MLDA-21 and gaining alcohol access on other crime types. Namely, we focus on property crimes (burglary, larceny, and robbery), as these, along with assault, fall under the classification of serious FBI Index crimes. Estimating both linear and quadratic order specifications, we find negligible effect of legal access to alcohol on crime commission and observe no significant discontinuities are identifiable at the threshold (Appendix…)

Table 8: Effect of MLDA-21 on Robbery, Burglary, & Larceny

	h(2) p(1)	h(3) p(2)	Mean
Panel A: Robbery			
Males	0.098^{*} (0.052)	0.122 (0.120)	2.75
Females	0.000 (0.007)	0.003 (0.012)	0.292
Panel B: Burglary			
Males	0.217^{*} (0.133)	0.171 (0.319)	5.13
Females	0.007 (0.014)	0.015 (0.030)	0.715
Panel C: Larceny			
Males	0.451^{*} (0.268)	0.275 (0.643)	12.7
Females	-0.010 (0.129)	-0.139 (0.281)	8.08

Notes: Each cell shows the RD coefficient (upper line) and its standard error (lower line). The first column uses a linear polynomial ($p = 1$) with $h = 2$, while the second uses a quadratic polynomial ($p = 2$) with $h = 3$. "Mean" indicates the baseline arrests per 1,000 individuals aged 20–21. Significance stars (e.g. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$) are illustrative; please adjust as needed based on actual p-values. Males are the full 1988–2023 sample; females are similarly full-period unless otherwise noted for a 2006–2023 restriction.

We examine arrest rates for other crimes less likely to be impulsive or alcohol-induced, namely robbery, burglary, and larceny (Appendix Figures A2.1 and A2.2). These crimes do not exhibit significant discontinuities at age 21, despite being classified as index, or severe crimes, by the FBI. Figures 2 and 3 show that repeating our analysis at ages 23 and 24 produces negligible, statistically insignificant discontinuities in assault arrests, indicating that the effect truly centers on age 21. Table 8 reports results using both linear and quadratic order polynomials. Although we observe a marginally significant estimate for the outcomes under the linear specification, its weak effect disappears under the quadratic. Crimes like robbery, burglary, and larceny typically involve greater planning and material motives. Visually plotting the age profile demonstrates that the quadratic specification is better fitting. In this model, the three placebo crimes exhibit no significant jumps at 21, consistent with the premise that impulsive, alcohollinked aggression, manifesting as assault, could be the collective response to legal access to alcohol.

4.5 MLDA Effect at Other Ages – Placebos

To ensure that any observed discontinuity at age 21 is not due to general non-linearities in age or specification artifacts, we estimate the change in arrest rates across different age boundaries (Table 9).

Table 9: Effect of MLDA-21 on Arrests – Age 23 and Age 24 Placebos

	Baseline	Log	Covariates	Weights
Panel A: Aggravated Assault				
Males				
Age 21	0.426^{***}	0.069^{***}	0.392^{***}	0.411^{***}
	(0.045)	(0.010)	(0.039)	(0.043)
Age 23	0.032	0.004	0.023	0.030
	(0.051)	(0.008)	(0.041)	(0.050)
Age 24	-0.044	-0.003	-0.027	-0.035
	(0.049)	(0.008)	(0.039)	(0.077)
Females (2006–2023)				
Age 21	0.059^{***}	0.042^{***}	0.059^{***}	0.059^{***}
	(0.018)	(0.014)	(0.012)	(0.018)
Age 23	0.028	0.022	0.023^{*}	0.026
	(0.019)	(0.015)	(0.013)	(0.018)
Age 24	-0.013	-0.010	-0.013	-0.010
	(0.016)	(0.012)	(0.016)	(0.024)
Panel B: Other Assault				
Males				
Age 21	1.12^{***}	0.078^{***}	1.08^{***}	1.10^{***}
	(0.069)	(0.007)	(0.034)	(0.072)
Age 23	-0.032	-0.002	-0.065	-0.0234
	(0.073)	(0.007)	(0.050)	(0.072)
Age 24	0.061	0.005	0.122^{**}	0.063
	(0.070)	(0.006)	(0.059)	(0.140)
Females				
Age 21	0.279^{***}	0.060^{***}	0.255^{***}	0.277^{***}
	(0.041)	(0.010)	(0.028)	(0.042)
Age 23	-0.008	0.001	-0.022	-0.009
	(0.040)	(0.008)	(0.026)	(0.041)
Age 24	-0.022	-0.003	0.034	-0.013
	(0.026)	(0.006)	(0.023)	(0.047)

Notes: Each cell contains the RD estimate at Age 21 with a bandwidth of h=2, Age 23 with a bandwidth of h = 2, Age 24 with a bandwidth of h = 2,6 and estimating local linear polynomials ($p = 1$) on each side. Results are checked for robustness with alternate specifications. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Neither aggravated or other assault rates exhibit a meaningful jump at 23 or 24 under both narrow (h=2) or wider (h=2,6) bandwidths. This suggests that the MLDA-21 threshold uniquely predicts a significant jump in arrests. If we had observed a significant discontinuity at age 23 or 24, it would have signaled potentially confounding trends unrelated to legal alcohol access. Given that the population characteristics and arrest rates remain smooth across these alternative boundaries, these align with the indication that something behavioral about aggravated and other assault is occurring at the threshold that does not occur at other ages.

5.2 RD Effect of MLDA-21 on Assault-Related ED Visits

As Chalfin acknowledged in his recent work, nearly all focus has been on the causal determinants of criminality rather than on the casual determinants of victimization (Chalfin, 2023). Here, we aim to supplement our consideration of arrest data with the victimization view, helping fill this gap by estimating how nonfatal assault injuries change at the MLDA-21.

Table 10: MLDA Effect on Total Assaults and Components

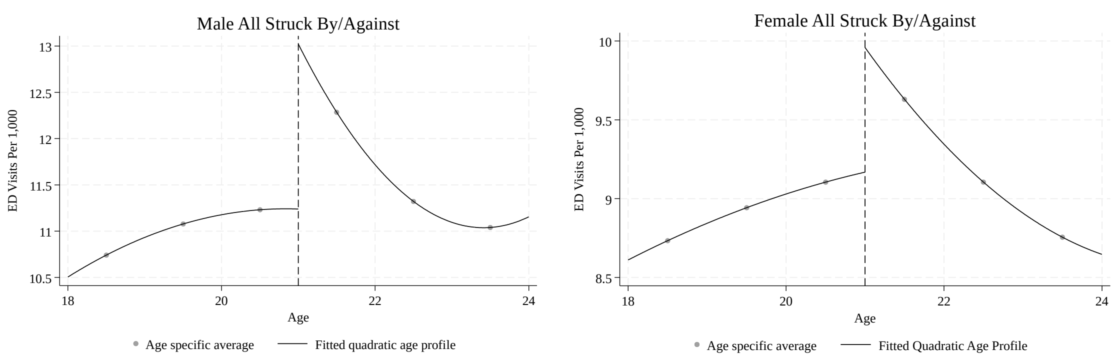
Mechanism	Sex	Estimate	Mean
Total Assault (All Assaults)			
	Female	0.676*** (0.244)	10.98
	Male	1.626*** (0.336)	15.75
Struck-by/Against			
	Female	0.707*** (0.204)	9.37
	Male	1.458*** (0.265)	11.76
All Other Assault			
	Female	–0.031 (0.086)	1.62
	Male	0.167 (0.157)	3.99

Notes: RD estimates measure the discontinuity in ED visit rates per 1,000 individuals at the MLDA-21 threshold. All models use triangular kernels with $h = 2$, $p = 0$, and include year fixed effects. Means refer to average ED visit rates for individuals aged 20–21. Significance of assaults come primarily from struck-by/against injuries.
Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

As shown in Table 10, both female and male ED visit rate for total assault rise significantly at the MLDA-21 threshold. For females, the estimated effect of gaining access to alcohol is 0.676 per 1,000 cases (SE = 0.244) and represents a significant 6% increase from the baseline mean of 10.98, and for males, the effect is larger, 1.626 per 1,000 (SE = 0.336), or 10.3%. Access to alcohol at the threshold of legality here, is leading to more violent interactions that thereby raise the risk of victimization.

Further disaggregation shows that this significant effect is primarily driven by the mechanism “struck by/against” assaults: these account for ~85% of female and ~75% of male assault ED visits, while other mechanisms (including bites, burns, cuts) remain minimal (Appendix Figure A3.1).

Figure 2: Main RD Effect of MLDA-21 on Struck By/Against Victimization



For females, the effect of reaching age 21, leads to a significant .707 increase per 1,000 ED visits for physical (struck-by/against) assaults. This is an increase of roughly 7.5%, based on a baseline of 9.37 per 1,000. For males, there is an increase of 1.458 per 1,000 ED visit cases, raising the rate by ~12.5 percent on a baseline of 11.75 per 1,000. The discontinuity effects in all struck by/against assaults are significant: the male effects are about 2.1x larger in magnitude, despite them having only about a 1.4x higher baseline mean. This would be consistent with marginal male victimization being more sensitive to alcohol access and increased participation in mutual fights, where both participants may end up in the sample.

5.2.3 Alternative Bandwidth Sensitivity and Robustness Checks

The discontinuity in all struck by/against ED visits at age 21 is large and robust: across specifications, the RD estimate for females ranges from 0.532 to 0.791 (SE: 0.171–0.352), and for males from 0.903 to 1.782 (SE: 0.213–0.461), with all estimates statistically significant. Relative to average ED visit rates of 9.37 for females and 11.76 for males, these effects represent increases of approximately 6% –8% for females and 8%–15% for males.

Table 11: MLDA Effect on Struck-by Assaults – Alternative Bandwidths

Bandwidth / Order	Sex	Estimate	Mean
$h(2)$, $p(1)$	Female	0.707*** (0.204)	9.37
	Male	1.458*** (0.265)	11.76
$h(3)$, $p(1)$	Female	0.651*** (0.176)	9.37
	Male	1.242*** (0.231)	11.76
$h(3)$, $p(2)$	Female	0.791** (0.352)	9.37
	Male	1.782*** (0.461)	11.76
$h(3,6)$, $p(1)$	Female	0.535** (0.170)	9.37
	Male	0.983*** (0.208)	11.76
$h(3,6)$, $p(2)$	Female	0.710** (0.295)	9.37
	Male	1.374*** (0.390)	11.76
$h(3,9)$, $p(1)$	Female	0.532*** (0.171)	9.37
	Male	0.903*** (0.213)	11.76
$h(3,9)$, $p(2)$	Female	0.585* (0.301)	9.37
	Male	1.191** (0.419)	11.76

Notes: Robust RD estimates of the effect of turning 21 on ED visits for all struck-by/against assault injuries. All specifications use a triangular kernel and include year fixed effects. Estimates are based on local constant models ($p = 0$ or $p = 2$) with bandwidths $h = 2$, $h = 3$, $h = 3,6$, and $h = 3,9$. Mean values reflect average ED visit rates per 1,000 individuals aged 20–21. Standard errors are in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sex	Baseline	Log	Covariates	Weights	Mean
$h(2), p(1)$					
Female	0.707*** (0.234)	0.074*** (0.022)	0.685*** (0.199)	0.695*** (0.204)	9.37
Male	1.458*** (0.305)	0.108*** (0.024)	1.356*** (0.245)	1.443*** (0.262)	11.76
$h(3), p(1)$					
Female	0.614*** (0.199)	0.067*** (0.019)	0.625*** (0.176)	0.646*** (0.175)	9.37
Male	1.098*** (0.265)	0.093*** (0.021)	1.204*** (0.217)	1.230*** (0.229)	11.76
$h(3,6), p(2)$					
Female	0.710** (0.295)	0.075** (0.033)	0.581** (0.289)	0.704** (0.295)	9.37
Male	1.374*** (0.390)	0.101*** (0.038)	1.059*** (0.317)	1.369*** (0.385)	11.76
$h(3,9), p(2)$					
Female	0.585* (0.301)	0.059* (0.034)	0.502** (0.254)	0.577* (0.300)	9.37
Male	1.191** (0.419)	0.086** (0.042)	1.042*** (0.280)	1.182*** (0.414)	11.76

Notes: RD estimates reflect the effect of turning 21 on arrests for struck-by/against assault using a triangular kernel. "Log" uses the log of the outcome; "Covariates" include race/ethnicity shares; "Weights" apply population weights. Polynomial orders and bandwidths vary by row. Mean values reflect average arrest rates per 1,000 for ages 20–21. *Significance levels:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11 provides additional robustness checks to confirm the stability of these estimates. The results hold across alternative bandwidths and both linear and quadratic local polynomials. Log-transformed models show smaller but still significant effects, indicating 7–10% increases in risk. Covariate-adjusted models that include race/ethnicity shares, as well as population-weighted models, produce nearly identical estimates to the baseline.

5.2.2 Gender Differences and Time-Period Splits

Table 12: Effect of MLDA on Struck-By Assault Victimization

Sex	Mechanism	Full Sample	2001–2008	2009–2022	Mean
Female					
	Sexual Struck-by	0.237**	0.765***	–0.064	1.26
		(0.097)	(0.121)	(0.112)	
	Non-Sexual Struck-by	0.470**	0.178	0.636***	8.10
		(0.210)	(0.418)	(0.212)	
Male					
	Sexual Struck-by	0.015	–0.003	0.025	0.06
		(0.019)	(0.022)	(0.028)	
	Non-Sexual Struck-by	1.444***	2.157***	1.036***	11.70
		(0.263)	(0.527)	(0.280)	

All models use local constant regressions ($p = 0$) with bandwidth $h = 2$, a triangular kernel, and include year fixed effects: $y2$–$y8$ for 2001–2008, $y10$–$y22$ for 2009–2022. Mean values reflect average ED visit rates per 1,000 individuals aged 20–22. *Significance levels:* * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Disaggregating further by mechanism, as well as splitting our sample into two time periods, we see that struck by/against victimization outcomes differ by gender. For females, sexual struck-by assaults account for nearly one-third of the overall RD effect prior to 2009, with an estimate of 0.765 (SE = 0.121) against a mean of 1.14 : this is about a 67% relative increase. However, this effect disappears following 2009, with a small and insignificant coefficient (–0.064). In contrast, non-sexual assault effects rise sharply, becoming the dominant driver of female victimization at age 21. Among males, sexual struck by/against assaults are near zero in all periods, while other assaults explain most of the MLDA-21 discontinuity in ED visits, with estimates over 1.0 at every point. Men appear more likely to be engaged in violent fights as well engage in mutual aggressive interactions.

5.2.3 Age Placebo Checks – Victimization

We estimate a series of placebo regressions using the same main specification at age 21, applying the cutoff at alternative ages from 23 to 28 years. Unlike FBI UCR data, our WISQARS ED visit data is available on a yearly basis for the entire sample, so we are able to extend of windows of analysis further. These placebo thresholds occur at a post-treatment range, one in which where no policy change or institutional shock is expected to take place. Our approach estimates a "false" discontinuity at arbitrary points along the running variable, age, to verify that the observed treatment effect is not merely driven by smooth trends or model artifacts (Lee and Lemieux, 2010).

Table 13; Effect of MLDA on All Struck By ED Visits, Ages 23-28

h, p	Sex	Age 23	Age 24	Age 25	Age 26	Age 27	Age 28
$h(2), p(1)$	Female	0.092 (0.234)	−0.121 (0.248)	0.286 (0.270)	−0.146 (0.263)	−0.081 (0.227)	−0.075 (0.203)
	Male	0.416 (0.282)	−0.150 (0.275)	0.196 (0.228)	−0.144 (0.258)	−0.054 (0.274)	0.078 (0.260)
$h(3), p(1)$	Female	–	0.140 (0.195)	0.219 (0.209)	−0.220 (0.201)	−0.262 (0.198)	–
	Male	–	0.126 (0.222)	0.057 (0.218)	−0.193 (0.209)	−0.114 (0.240)	–
$h(3), p(2)$	Female	–	−0.067 (0.345)	0.380 (0.363)	−0.147 (0.379)	0.146 (0.372)	–
	Male	–	−0.308 (0.427)	0.288 (0.410)	−0.188 (0.392)	0.036 (0.461)	–
$h(3, 6), p(1)$	Female	–	0.140 (0.195)	–	–	–	–
	Male	–	0.126 (0.222)	–	–	–	–
$h(3, 6), p(2)$	Female	–	−0.067 (0.345)	–	–	–	–
	Male	–	−0.308 (0.427)	–	–	–	–
$h(4), p(2)$	Female	–	–	0.266 (0.330)	−0.098 (0.323)	–	–
	Male	–	–	0.101 (0.342)	−0.109 (0.339)	–	–

Notes: RD estimates for placebo cutoffs at ages 23–28 using various bandwidths (h) and polynomial orders (p). All models use a triangular kernel and include year fixed effects. Standard errors are robust. Dashes (–) indicate no model was run for that specification and age. None of the placebo RD estimates are statistically significant.

Table 13 presents the results of these placebo tests for all struck-by/against ED visits. Across all specifications, including local linear and local quadratic models with bandwidths ranging from 2 to 4 years, and separate estimates for males and females, we find no statistically significant discontinuities at any of these cutoffs. Point estimates fluctuate in sign and magnitude but remain consistently negligible across the board. At age 25, the estimate for females under a linear model is 0.286 (SE = 0.270); for males, the estimate is 0.196 (SE = 0.228). Under more flexible polynomial specifications, such as those around age 27, the estimates are statistically indistinguishable from zero and exhibit wide confidence intervals.

Our results provide evidence that the increase in emergency department visits for assault victimization at age 21 is driven almost entirely by struck-by/against injuries, defined as assaults involving physical contact such as punching, pushing, or fighting. These types of violent interactions are plausibly heightened by alcohol access, through pharmacological links such as reduced judgement and myopia of younger individuals.

6 Discussion and Concluding Remarks

Presently, this study finds that upon obtaining legal access to alcohol at age 21, there is a significant increase in both assault perpetration and assault victimization. As captured by national arrest data, we observe arrest rates for aggravated and other (simple) assaults rise by around 5–8%, with stronger and more consistent effects for males across the entire 36-year sample. For females, the other (simple) assault effect is significant for the whole period, while the aggravated assault effect emerges only in the latter half. As captured by ED inpatient data, we observe similar substantial increases in victimization rates for assault related injuries, particularly physical ones. Our morbidity findings show that the rise in assault arrests corresponds to a parallel rise in assault injuries, a convergence of offender and victim evidence that supports an alcohol-induced increase in violent altercations upon legal access. While female victimization patterns have shifted over time, from sexual struck-by pre-2009 to more general physical aggression post 2009, it remains that legal alcohol access intensifies physical violence for both genders, with men's ED admissions increasing by as much as 12–13% in some specifications.

6.1 Limitations and Future Directions

Prior RD analyses (Chalfin et al., 2023) report modest increases in robbery victimization or property crimes among certain subgroups (12% increase for male burglary and larceny), for which we do not observe. There are a few plausible reasons for these inconsistencies. First, we use nationally aggregated data spanning many years, while other studies rely on more granular administrative or state-level data from shorter durations of time. Local variation in policing or social norms could lead to property offenses responding differently to the MLDA in certain areas or time frames. Second, across states, there are different definitions of "youth" or restricted geographies, which could yield different patterns for lower frequency crimes such as robbery, helping to explain our divergences. Lastly, robbery and larceny arrests often blur the line between attempted theft and aggressive behavior. Small classification or under-reporting differences might mean we fail to detect the same signals as prior work.

A longstanding concern in MLDA research is whether observed spikes at 21 reflect a brief "birthday celebration" phenomenon or legitimate, sustained increases in alcohol availability and consumption. Prior studies (Carpenter & Dobkin, 2011; Hansen & Waddell, 2018; Chalfin et al., 2023) have explicitly tested for short-term "partying surges" around the 21st birthday, finding that while some heightened incidents do occur near the exact birthday date, longer term discontinuities tended to persist for months or longer. Our results, comparing outcomes within multiple bandwidths around the cutoff, similarly suggest that the alcohol access effect extends beyond an acute "turning 21" celebration. Thus, the discrete jump in assault could be explained away by a one-day or one week celebration, but instead, appears driven by a more durable relaxation in access to alcohol and the subsequent increases in heavy or frequent consumption.

Further, in the U.S, the age of 21, like 18, is often an eligibility threshold for more than just alcohol. Notable activities, such as gambling, license to bear arms in certain states, cannabis, and more, enter the picture. While these might theoretically serve as confounding co-treatments, the consensus in the literature on the MLDA seems to provide evidence otherwise:
the dominant behavioral change at age 21 is the jump in legal alcohol consumption. Any additional exposures appear relatively minor in comparison or affect far fewer individuals. Thus, it is improbable that these other age 21 transitions explain the significant increase in assaults.

Ultimately, our analysis presents a strong case that the MLDA-21 more than a celebration, but rather, an enduring, societal level escalation in both alcohol-driven aggression and vulnerability to violent assaults. The parallels between arrest data (offenders) and ED visit data (victims) reinforce the notion that MLDA-21 meaningfully alters the social environment in which young adults consume alcohol, prompting impulsive and often mutual altercations that manifest in tangible harm. Crucially, our results remain robust across multiple specifications, bandwidths, and weighting schemes, leaving little doubt that the discontinuous grant of legal alcohol access is a pivotal contributor to these spikes in violence.

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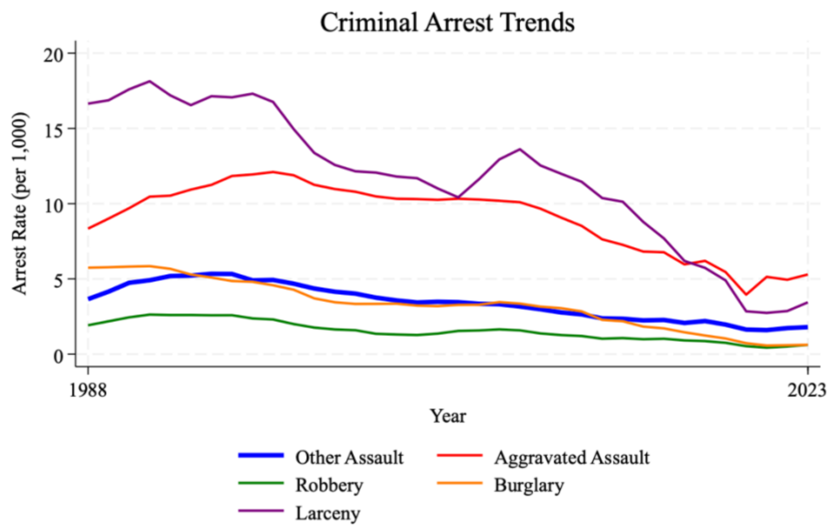
8. Appendix:

Figure A1.1



Notes: Five offense types are shown above. Consistent with the general decline in crime rates in the U.S. from the early 1990s onward, rates of arrest for these crimes have trended downward and occasionally diverged during our sampling period, from 1988 to 2023.

Table A.2: Causes of Nonfatal Injuries as reported by CDC WISQARS, 2023

10 Leading Causes of Nonfatal Injury for ages 15-24
All Injuries, 2023, Both Sexes, All Cases, All Races

Cause of Injury	Estimated Number	Percent	Cases (Sample)	Standard Error	CV	Lower 95% CI	Upper 95% CI
All Others	745,628	19.1%	-	-	-	-	-
Unintentional Bite: Other, including sting	77,414	2.0%	1,799	5,660	7.3%	66,321	88,507
Unintentional Other Transportation	86,157	2.2%	2,115	8,455	9.8%	69,585	102,729
Assault - Other Struck by /Against	223,898	5.7%	6,629	18,487	8.3%	187,664	260,133
Unintentional Poisoning	224,911	5.8%	5,626	21,797	9.7%	182,189	267,633
Unintentional Other Specified	244,954	6.3%	6,448	24,861	10.2%	196,226	293,682
Unintentional Cut/Pierce	298,500	7.7%	7,179	20,729	6.9%	257,871	339,129
Unintentional Overexertion	351,575	9.0%	9,299	30,089	8.6%	292,600	410,550
Unintentional Motor Vehicle Occupant	512,859	13.2%	13,455	52,117	10.2%	410,709	615,008
Unintentional Fall	556,860	14.3%	14,849	40,217	7.2%	478,034	635,686
Unintentional Struck by /Against	576,829	14.8%	15,384	38,960	6.8%	500,468	653,190
All Intents All Causes	3,899,585	100.0%	103,641	-	-	-	-

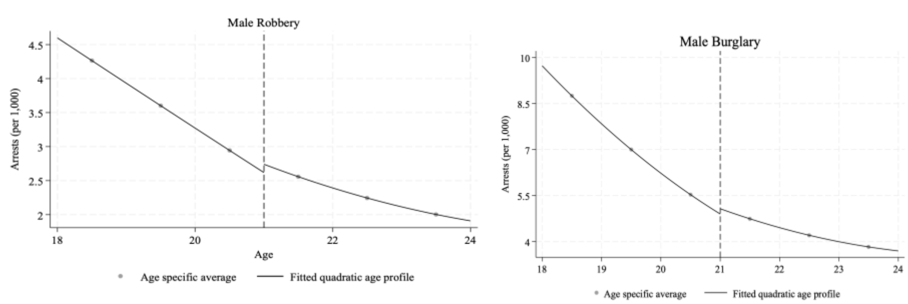
Table A.3: Effect of Alcohol Access on Youth Assault Arrest Rates (per 1,000)

Panel A: Males **Violent Crime**	**Estimate**	**Mean (Age 20–21)**
Full Sample (1988–2023)		
Aggravated Assault	0.442*** (0.108)	6.24
Other Assault	0.937*** (0.120)	14.00
Pre-2006 Period (1988–2005)		
Aggravated Assault	0.517*** (0.157)	8.18
Other Assault	1.070*** (0.156)	17.50
Post-2006 Period (2006–2023)		
Aggravated Assault	0.366*** (0.077)	4.31
Other Assault	0.805*** (0.171)	10.50

Panel B: Females **Violent Crime**	**Estimate**	**Mean (Age 20–21)**
Full Sample (1988–2023)		
Aggravated Assault	0.042 (0.025)	1.51
Other Assault	0.221** (0.083)	4.75
Pre-2006 Period (1988–2005)		
Aggravated Assault	–0.001 (0.026)	1.66
Other Assault	0.195** (0.069)	4.69
Post-2006 Period (2006–2023)		
Aggravated Assault	0.086** (0.035)	1.35
Other Assault	0.247** (0.101)	4.81

Notes: Each row presents an RD estimate of the discrete increase in assault arrest rates at age 21 (MLDA) for the specified subgroup. Estimates are from local polynomial regressions with a triangular kernel, bandwidth of 3 years on each side ($h = 3$), and a quadratic specification ($p = 2$). Means reflect average arrest rates per 1,000 individuals aged 20–21. Standards errors are denoted in parentheses.
Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A2.1: Placebo Discontinuities at Age 21 – Males



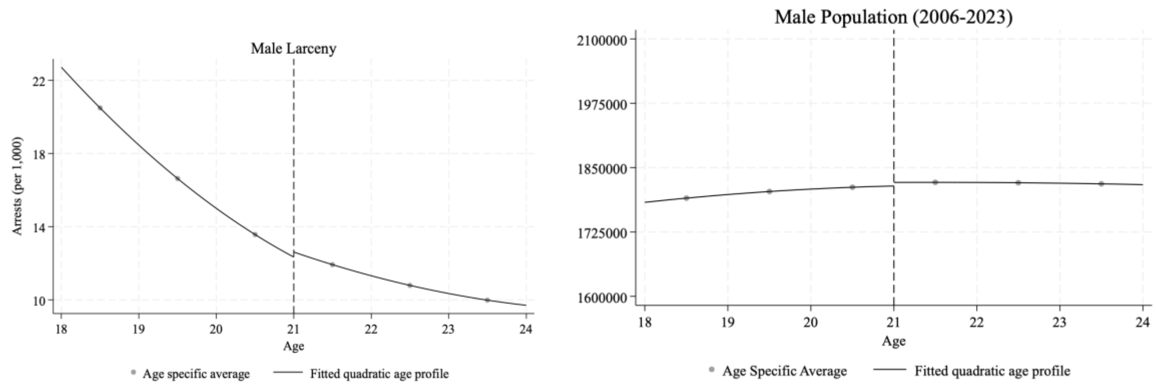


Figure A2.2: Placebo Discontinuities at Age 21 – Females

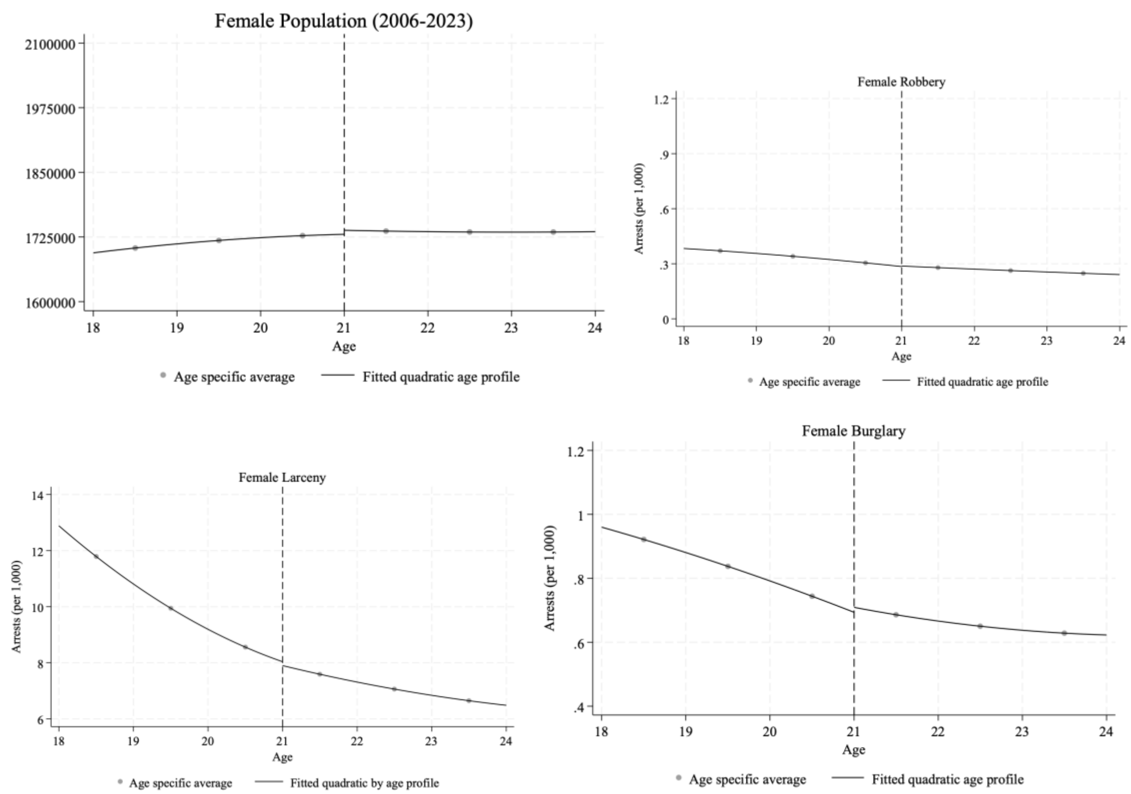


Figure A3.1: Placebo Assault Arrest Discontinuities at Age 23

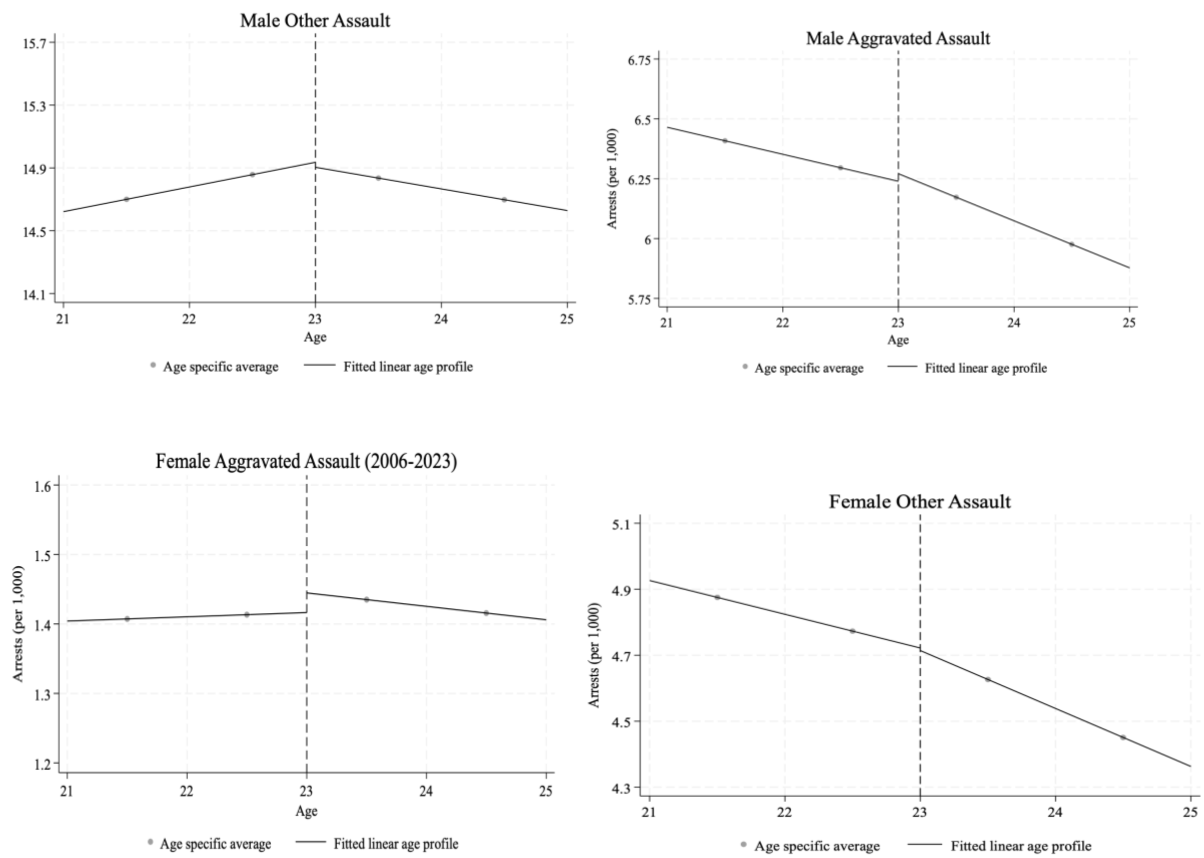


Figure A3.2: Placebo Assault Arrest Discontinuities at Age 24

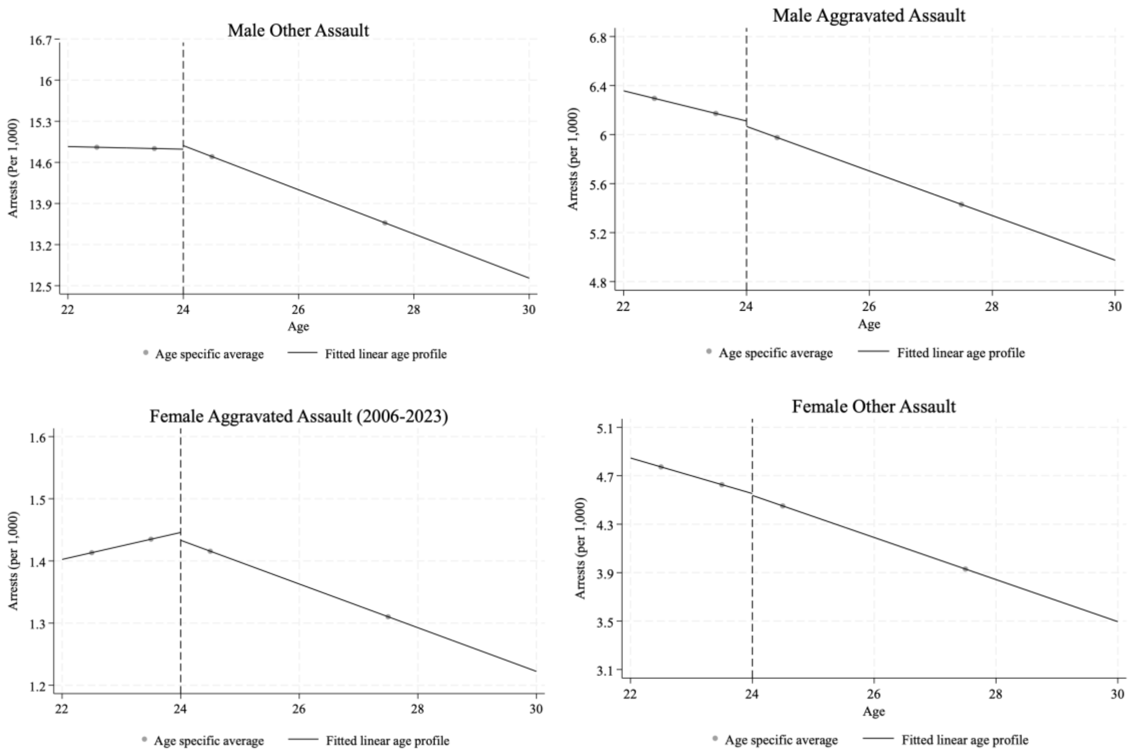
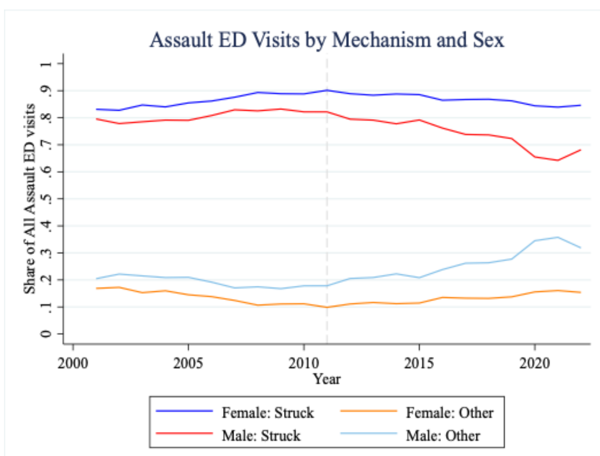


Figure A4.1: Share of All Assaults in ED Visits from 2001-2002



Notes: Figure 2 depicts this discrepancy in the share of struck-by/against assaults versus other assaults – we observe that struck by/against assaults account for ~85% of female and ~75% of male assault ED visits, while other mechanisms (including bites, burns, cuts) remain minimal.

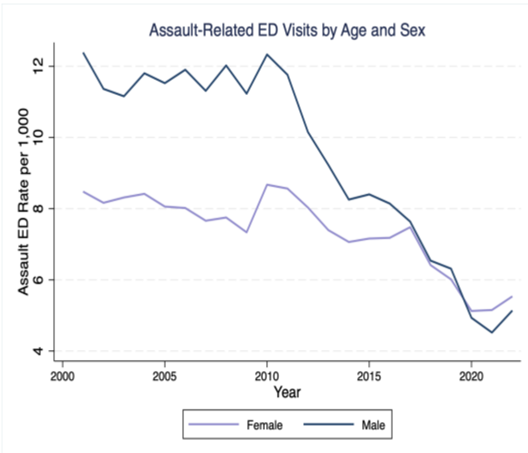


Table 10: Mean Assault ED Visit Rates and Mechanism Shares by Age and Sex (per 1,000)

Age	Sex	Total Assault	Struck by/Against	Other Mechanism	% Struck
20.5	Female	10.700	9.105	1.596	85.1%
21.5	Female	11.266	9.630	1.636	85.5%
20.5	Male	15.118	11.231	3.887	74.3%
21.5	Male	16.373	12.284	4.089	75.0%

Notes: This table presents the mean emergency department visit rates for assault-related injuries at ages 20.5 and 21.5, by sex and mechanism of injury. "Struck by/Against" refers to altercations involving physical blows (e.g., punches, shoves), while "Other Mechanism" includes all other violence-related causes (e.g., bites, burns, cuts). The final column reports the share of total assault visits attributable to Struck by/Against. For both males and females, over 85% and 75% of the MLDA-21 increase in ED assaults is due to this mechanism.

Table 3: Unintentional Struck-by ED Visits at MLDA-21

Group	$h(2), p(1)$	$h(3), p(2)$
Female	0.184 (0.304)	–0.099 (0.505)
Male	0.296 (0.404)	–0.677 (0.659)

Notes: Unintentional struck by injuries (e.g., accidental contact) at the MLDA-21 threshold. None of the coefficients are statistically significant. This supports the interpretation that MLDA-21 affects intentional assaults, not general injury risk or reporting.

As an additional placebo check, we estimate the effect of turning age 21 on “unintentional struck by” injuries in our WISQARS ED data. These injuries include accidental contact, such as being struck by an object or person without intent, which we would not expect to respond to changes in alcohol access. Consistent with this, we find no statistically significant effects. For females, the estimates are 0.184 (SE = 0.304) and –0.099 (SE = 0.505) under bandwidths 2 and 3, respectively; for males, the estimates are 0.296 (SE = 0.404) and –0.677 (SE = 0.659).