

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

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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 investigate how alcohol-related arrest and victimization rates 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, turning 21 increases arrest rates for aggravated and other simple assaults significantly by 5–8%, with the aggravated assault effect for females restricted to the latter half of the sample period. Turning to emergency department (ED) data from the CDC’s Web-based Injury Statistics and Query Reporting System (WISQARS), which spans 2001–2022, we extend our analysis of assault-related violence by assessing victimization outcomes, particularly the effect of the MLDA-21 on nonfatal injury. Notably, we observe that ED visits for “struck by or against” assaults rise significantly by 7–10% at the MLDA-21, suggesting both increased participation in violent altercations and increased risk of victimization upon gaining legal access to alcohol. For both arrest and hospital analyses, analogous effects at slightly older ages are small and insignificant, as well as those for demographic and population characteristics that are expected to trend smoothly across the MLDA-21 threshold. Taken together, these results indicate 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

Across the world, alcohol regulation 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 embedded deeply in many 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 the purchase and consumption of alcohol vary drastically, with countries like Wales setting a minimum legal drinking age of five years under certain conditions and other European nations setting age limits between 16 and 18 years.² At the other end of the spectrum, several countries—including Saudi Arabia, Afghanistan, and Sudan—ban the sale and consumption of alcohol in its entirety.³ Despite the cultural, religious, and political differences, these policies all share an overarching recognition of the dangers of early alcohol access and the need for regulation.

According to the World Health Organization (WHO), alcohol remains a major driver of both morbidity and mortality worldwide. Recent estimates indicate that 2.6 million deaths annually—4.7% of all global deaths—are attributable to alcohol consumption, with approximately 400 million individuals living with alcohol use disorders in 2019 (WHO, 2023). Notably, 13% of these alcohol-attributable deaths were among young adults aged 20–39 years (WHO, 2023). Beyond its chronic health risks, alcohol is strongly associated with a range of social, mental, and psychological consequences, extending even to non-drinkers who can become victims of alcohol-related aggression (WHO, 2002). Thus, a substantial body of the economics and public health literature has studied these effects, linking excessive alcohol consumption to violent crime, impaired judgment, and heightened risk-taking behavior (Cook & Moore, 2000; Carpenter, 2009). Studies have been conducted across diverse settings, from Scandinavia to Australia, and consistently show that more stringent alcohol policies yield public health benefits for society at large and particularly among youth (Carpenter and Dobkin, 2012; Carpenter and Dobkin, 2005; Lindo et al, 2015). The present study contributes to the growing literature by investigating how the U.S. federal minimum legal drinking age of 21 (MLDA-21)—one of the

² In Wales, five-year-olds can drink under supervised conditions in a private setting. 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.

strictest limits among developed nations—affects both criminal arrests and non-fatal injury rates related to assault.

In the United States, sentiment towards alcohol regulation has historically been expressed through various policy levers designed to minimize the substance’s detrimental societal and health-related harms. These include regulations governing where, when, and to what extent alcohol can be sold or consumed. In the 1970s, following the passage of the 26th Amendment, which established a uniform voting age of 18, several states also experimented with reductions in their minimum legal drinking ages (Toomey, 1996). Research soon after showed the repercussions of these changes to be troubling: youth drinking rates and alcohol-related motor vehicle accidents rose sharply after the policy change, especially among the age group newly eligible to drink (Douglass et al. 1974; Wagenaar, 1993; Whitehead, 1977). High-profile “blood border” cases and the ensuing public outcry eventually gave way to legislative intervention.⁴ 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. Congress, House). By 1988, all 50 states had adopted the MLDA-21, marking the start of a nationwide effort to curb the dangers associated with early alcohol access (Bonnie & O’Connell, 2004).

Despite this uniform implementation of the MLDA-21, alcohol consumption and misuse 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). Of all alcohol-related harms, unintentional injuries and deaths are particularly striking: an estimated 1,519 college students aged 18 to 24 years die annually from alcohol-related unintentional injuries, including motor vehicle crashes (NIAAA, 2023). Additionally, alcohol-related emergency department (ED) visits impose a substantial public health burden, generating an estimated \$3.6 billion in healthcare costs annually, with assault-related injuries accounting for a large share (CDC, 2021). Beyond physical health risks, alcohol is frequently implicated in criminal behavior through numerous pathways, including reduced inhibitions, myopia, impulsivity, and increased aggressive behavior. In the U.S., roughly 40–45% of homicides and

⁴ “Blood Border” incidents refer to the highly publicized instances where youth drove to other states with lower MLDA to drink lawfully, only to crash on their way home (FTC).

physical assaults involve alcohol use by the offender (Bureau of Justice Statistics). Young drinkers are particularly susceptible to engaging in risky or harmful behavior: impaired decision-making abilities and peer pressure can quickly escalate into violence, vandalism, and other actions that may carry long-term ramifications.

Given the wide-ranging consequences, the effectiveness of the MLDA-21 has been studied extensively in both the economics and public health literature. Numerous studies suggest that restrictive alcohol policies reduce consumption among young adults and lower rates of subsequent alcohol-related harms, including traffic fatalities, hospitalizations, and mortality (Carpenter and Dobkin 2005, 2009, 2011, 2015, 2017). Still, the federal mandate has not been without controversy. 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 MLDA-21. Their central argument held that lowering the drinking age would encourage safer, regulated consumption rather than fostering an illicit culture of excess (Choose Responsibility, 2008). Critics of the MLDA-21 maintain that the higher drinking age pushes alcohol use into unregulated underground spaces, exacerbating dangerous drinking behaviors such as binge drinking—defined as consuming five or more drinks (men) or four or more drinks (women) on a single occasion. According to the NSDUH (2023), 9.8 million young adults (28.7% of this age group), including 29.3% of full-time college students reported binge drinking in the past month.

Although prior work has documented increases in arrests for criminal offenses at age 21 (Carpenter and Dobkin, 2015), relatively few studies have evaluated outcomes related to morbidity and victimization. Overlooking these common outcomes can result in costly consequences in terms of the labor market, healthcare systems, and individuals (Chalfin et. al, 2023). To address this gap, the present study investigates how turning 21 impacts both assault perpetration and assault-related injuries, presenting a more comprehensive analysis of alcohol's role in violent altercations. Utilizing a regression discontinuity design, we identify the causal effects of the MLDA-21 on violent outcomes in the U.S. context. This approach extends to examine both the criminal and offender side of violent interactions, revealing that alcohol-related violence is driven primarily by mutual participation in physical altercations rather than premeditated crimes. Our findings show that violence related assaults and hospital visits increase significantly at age 21, suggesting that legal alcohol access at this threshold increases the likelihood of both committing and being a victim of assault.

The remainder of the paper is organized as follows: Section 2 reviews the existing literature on alcohol regulations and their impact on crime and public health. Section 3 describes our data sources and the construction of our unique national level sample. Section 4 outlines our empirical methodology, expanding on our quasi-experimental approach, the regression discontinuity framework. Section 5 presents the main results, contextualized by relevant descriptive statistics of our data. Lastly, Section 6 concludes with a discussion of our findings, an acknowledgement of limitations, and suggested directions for future research and policy.

2 Background and Literature Review

2.1 Empirical Evidence

Previous literature has documented a strong association between alcohol use and criminal behavior. For instance, Greenfeld (1998) reported that over one-third of convicted offenders in the U.S. had consumed alcohol at the time of their crime, demonstrating the substance's widespread role in crime commission. Still, the mechanisms and causal pathways underlying the association remain a subject of ongoing debate.

One body of research examines the most direct pathway through which alcohol consumption contributes to crime, namely, its pharmacological effects. Alcohol is known to heighten aggression in individuals by reducing inhibitions and impairing judgement, thereby increasing the likelihood of impulsive or emotionally charged responses (Fagan, 1990; Pernanen, 1981; Carpenter and Dobkin, 2012). These physiological effects are particularly pronounced among younger individuals who already exhibit lower levels of self-control and a greater propensity for risk-taking behavior (Lipsey et al., 2002). This helps explain why legal access to alcohol may lead to a greater risk of both perpetration and victimization. Unsurprisingly, interpersonal violence is especially common in environments where alcohol is readily available, including venues such as bars and nightclubs, which tend to facilitate risky social interactions (Carpenter and Dobkin, 2012). As such, this line of work has provided the foundation for arguments that alcohol access, particularly at legal thresholds such as the MLDA-21, may directly contribute to increased rates of violent incidents.

Early empirical work sought to identify causality by exploiting state-level variation in policies—namely, differences in alcohol taxes and prices, as well as the staggered adoption of the MLDA-21 following the passage of the federal mandate. With the high prevalence of risky drinking among youth (DHHS, 2001; Johnston et al., 2002), these studies consistently found that higher alcohol taxes or prices tend to reduce both the frequency and intensity of youth drinking, with the strongest effects observed among heavier drinkers (Grossman et. al, 1987; Coate & Grossman, 1988; Laixuthai & Chaloupka, 1993). In one such study on youth zero tolerance laws, Carpenter and Dobkin (2007) leveraged variation in state rollout to assess how stricter underage drinking enforcement reduced heavy drinking and crime among 18 to 20-year-olds. The findings were that zero tolerance law adoption resulted in a 13% reduction in binge drinking among

young men and a 5% decline in property and nuisance crimes. Interestingly, there was no significant impact on violent crimes.

Expanding on this work, Carpenter and Dobkin (2009) showed that heavy episodic drinking (binge drinking) increases sharply at age 21, with corresponding rises in alcohol consumption on both the extensive and intensive margin. They further linked this behavioral shift to a rise in alcohol-related traffic fatalities, emphasizing the public health implications of legal alcohol access. Their findings contributed to a growing broader consensus supporting the MLDA-21 as an effective policy tool for reducing alcohol-related harms.

However, some critics questioned the generalizability of the evidence, raising endogeneity concerns about state-level adoption of drinking-age laws (Miron and Tetelbaum, 2009). Using three decades of state-level panel data, the researchers found that the MLDA of 18 years—or a lack thereof a 21 mandate—to be associated with a 4% increase in drinking and a 3% increase in heavy episodic drinking. This applied only in states that raised the drinking age before the federal mandate. In states that adopted MLDA-21 under federal pressure, the effects were smaller and statistically insignificant. Rather than the declines in traffic fatalities being explained by drinking age laws, their work suggested that it was vehicle safety, technology, and emergency response improvements driving this change. Thus, their findings raised concerns about potential over-attribution of public health improvements to the MLDA, calling for more rigorous account of confounding factors. Similarly, other scholars expressed concern about the relatively weak influence of alcohol tax instruments used in the literature (Laixuthai and Chaloupka, 1998; Carpenter and Dobkin, 2012).

As the temporal and cross-state variation in alcohol policies exhausted itself, researchers increasingly turned towards regression discontinuity methods as an alternative source of causality. This methodological shift made use of the quasi-experimental setting set up by the sharp age-based policy cutoff at 21. Carpenter and Dobkin (2009) were among the first to apply RD in this context, exploiting the MLDA-21 threshold to examine its effect on consumption and mortality. Their results showed substantial increases in heavy drinking and a 9% rise in deaths, largely driven by alcohol-related causes. Similarly, Lindo et al. (2016) employed an RD approach in New South Wales, Australia, to evaluate the effects of the country's 18-year-old MLDA on motor vehicle accidents and hospitalizations. Drawing on the nation's strict zero

blood-alcohol content (BAC) limit for drivers, they found that legal access to alcohol significantly increased traffic accidents, particularly during nights and weekends.

Subsequent studies extended this framework to broader crime outcomes. Building on their earlier work, Carpenter and Dobkin (2015) applied the RD design to California's universe of administrative crime data, finding a 5.9% increase in total arrests at age 21. Notably, disproportionate jumps occurred in assaults, DUIs, and disorderly conduct. Disaggregated by crime type, they observed that violent crime (such as aggravated assault and robbery), alcohol-related offenses (including drunkenness and DUIs), and nuisance violations accounted for the majority of the spike. In contrast, property and drug crimes saw much smaller increases. Later, Hansen and Waddell (2018) provided further supporting evidence of this pattern. Analyzing judicial records from Oregon, they identified significant increases in assault and drunk driving upon gaining legal access to alcohol, but no corresponding rise in higher grade offenses like robbery or rape. Together, these studies point to a plausible behavioral mechanism: alcohol availability tends to facilitate impulsive and aggression-driven offenses rather than calculated criminal acts. While the mediation and intent behind criminal behavior were not always the primary focus of earlier work, this emerging result has motivated the present study's emphasis on both assault perpetration and victimization.

Despite the growing body of RD-based evidence on crime perpetration, relatively little attention has been paid to the victimization side of alcohol-related harm. Until recently, much of the economics literature on alcohol control has focused disproportionately on offenders—those who commit crimes—while largely overlooking the experiences and outcomes of victims (Carpenter and Dobkin, 2012; Chalfin et al., 2023). This gap may be partly attributable to the pharmacological effects of alcohol, which makes separating the effects of crime commission from its effect on criminal victimization particularly difficult. Individuals under the influence may become easier targets of crime and may place themselves in riskier situations due to impaired judgment and reduced ability to protect themselves or flee from danger. At the same time, alcohol may heighten impulsivity and lower cognitive functioning, contributing to escalating encounters. Earlier research lacked the data to isolate these mechanisms. However, the recent availability of more granular data regarding mechanism, situational context, and direct outcomes has paved the way for more detailed analyses (Carpenter and Dobkin, 2012).

Chalfin et al. (2023) sought to address this gap by examining the impact of alcohol access at age 21 on crime victimization. They found that both violent and property crime victimization increased at the MLDA-21 threshold, with their estimates showing particularly pronounced effects observed for sexual assault and public space violence. They successfully ruled out “birthday celebration” effects, instead concluding that legal access to alcohol elevates both the likelihood of perpetrating and being subject to crime. Their findings suggest that alcohol consumption not only spurs aggressive behavior but also weakens self-defense mechanisms and increases exposure to high-risk environments. Overall, this suggests that the observed rise in victimizations reflects structural, rather than situational, increases tied to legal access to alcohol.

Another understudied area in the alcohol regulation literature involves nonfatal injuries. While alcohol-related fatalities—especially drunk driving and weapon induced violence—have received the bulk of academic attention and are often the center of policy discourse, morbidity represents a far more common and costly consequence. Alcohol-related injuries can have serious effects in both the short and long term, imposing grave burdens on individuals, healthcare systems, and society at large. In 2021, U.S. hospital expenditures rose by 4.4%, even as inpatient and emergency department (ED) volumes declined below pre-pandemic levels (Kaiser Family Foundation, 2023). This increase reflects rising care intensity, longer lengths of stay, and the ongoing strain on hospital infrastructure. Nearly 60% of inpatient hospitalizations are preceded by an ED visit, and 18% of U.S. adults visited the ED that year (CDC, 2021). Among young adults aged 18–24, alcohol-related injuries are a significant driver of ED utilization and, in many cases, lead to hospital admission), these incidents represent a major externality of alcohol consumption, borne not only by the consumer but also the broader healthcare infrastructure.

Despite its public health relevance, few studies in the economics literature have applied quasi-experimental methods to isolate the causal effect of alcohol access on nonfatal injury. Lindo, Siminski, Yerokhin (2016) and Carpenter and Dobkin (2017) were among the few to provide such estimates. In the U.S., Carpenter and Dobkin (2017) found that emergency department visits in the U.S. rose by 71.3 per 10,000 person-years at age 21, while inpatient hospital admissions increased by 8.4 per 10,000. These effects were driven largely by accidental injuries, overdoses, and physical harm inflicted by others—including assaults. Their findings suggest that the MLDA threshold is associated with a wide spectrum of nonfatal harms that further strain the nation’s healthcare system. In Australia, Lindo et al. (2016) applied a similar

RD approach, studying morbidity using hospital admissions data in New South Wales (NSW) from the National Hospital Morbidity Database (NHMD). Their design exploited the MLDA of 18 in NSW, finding that drinking behavior rose sharply at age 18, yet there was no corresponding increase in the likelihood of motor vehicle accidents (MVAs), accidents with injury, or accidents involving death. Instead, the main rise in alcohol-related harm appeared in hospitalizations for assault and alcohol poisoning, showing that increased morbidity occurs even without a jump in traffic-related fatalities. Lindo et al. attribute the differences of this study from U.S. findings to the strict anti-drunk driving policies in Australia, including random breath testing, zero-tolerance BAC limits for youth, and rigorous enforcement practices.

2.2 Contribution of the Present Study

This study builds on a growing body of economic and public health research that employs age-based RD designs to isolate the causal effects of legal alcohol access. We advance the literature by jointly evaluating both perpetration and victimization outcomes related to alcohol-induced violence, combining national administrative data on arrests with ED visits to capture both sides of such altercations. While previous work has typically focused on either crime or health outcomes in isolation, our approach seeks to integrate both perspectives. In particular, the inclusion of nonfatal ED visits allows us to assess the causal impact of alcohol access on both criminalization and victimization—an important gap in existing work.

We leverage nationally reported datasets from the FBI for arrest data and the CDC Web-based Injury Statistics Query and Reporting System (WISQARS for ED injuries). Unlike prior research, which has 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.

We link data on criminal arrests for assault from the FBI with national estimates of ED visits for assault-related injuries from the CDC's WISQARS system. This allows us to examine the symmetry, or asymmetry, between the onset of criminal behavior and the occurrence of alcohol-related physical harms, providing novel insight into whether violent altercations are mutually aggressive or more one-sided in nature. To our knowledge, this is one of the first studies to leverage national-level data for both crime and morbidity outcomes in an RD framework centered on the MLDA-21 threshold.

We also contribute to the literature on nonfatal injury, which remains underdeveloped despite its substantial public health and economic burden. Our inclusion of emergency department (ED) data addresses an important gap noted in prior studies. While Carpenter and Dobkin (2017) and Lindo et al. (2016) have shown increases in alcohol-related morbidity following legal access, there has been minimal work connecting these patterns with trends in crime. Moreover, much of the existing research is limited to specific U.S. states or within countries like Australia, where policy enforcement and baseline risks differ. Our use of nationally representative data in the U.S. strengthens the external validity of our findings and allows us to speak more directly to federal policy implications.

Taken together, this study offers a more holistic evaluation of alcohol's role in violence and extends the policy discussion on whether—and how—the MLDA-21 effectively prevents societal harm. In doing so, we move beyond the typical dichotomy of public safety versus personal health to show how the two interact under a common policy framework. By demonstrating that legal access at age 21 increases assault and victimization, our findings suggest that lowering the MLDA could generate significant costs on and harmful consequences for emergency departments, law enforcement, and public health systems.

3 Data Description

3.1 Institutional Setting and Context

In the United States, legal access to alcohol begins at age 21 under the federal 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 commission of a variety of criminal offenses (Carpenter and Dobkin, 2009, 2011). While the connection between alcohol and crime has been discussed extensively, much of the existing research has focused on the perpetration of crime and neglected victimization, which omits key components of the substance's full social consequences. Additionally, many analyses prioritize discussions on mortality, especially from motor vehicle accidents and homicides, rather than morbidity. This represents another gap in our understanding of alcohol's societal impact.

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. Below, we describe our key variables, sample construction, and the relevance of each of our datasets.

3.2 FBI Uniform Crime Reporting (UCR) Program Arrest Data

We obtain arrest data from the FBI's Uniform Crime Reporting (UCR) Program, which collects detailed, incident-level reports from over 16,000 participating agencies, including state, county, city, university/college, and tribal agencies. As of 2023, the UCR records over 14 million criminal offenses annually, covering a combined 94.3% of the U.S. population (FBI, 2024). Historically, these data were submitted via the Summary Reporting System (SRS), which aggregates incidents by offense category and basic demographic characteristics, such as age, sex, and race. From 2021 onward, the FBI began phasing out the SRS in favor of the National Incident-Based Reporting System (NIBRS), a more detailed, incident-level framework that collects information about victims, offenders, and circumstances surrounding the crime. To date, not all agencies have transitioned to NIBRS reporting, so our data sample, from 1988-2023, includes submissions from both systems.

We restrict our analysis to the period after all states adopted the MLDA-21, post-1988, thereby allowing for a consistent policy environment. Our primary crime of interest is assault, but we also analyze several other “index crimes,” which are those categorized by the FBI as the most serious that can be committed. Offenses that fall under this include murder and non-negligent homicide, forcible rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, and most recently, arson (EBSCO, 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 of our select offenses, displaying average arrest rates for individuals just under the MLDA threshold, prior to turning 21. Although our analysis focuses on assault related cases, we report the average arrest rates for major offense categories from the FBI Uniform Crime report in Table 1 in order to maintain comparability with prior studies and to contextualize the frequency of assault within the crime landscape. We also

disaggregate arrest rates by gender to account for the differential crime behavior patterns between the two. As expected, 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 comparable results for male rates of property crime commission. Even with female arrest rates being lower on average for these offenses, they are still non-negligible.

3.2.1 Reporting and Sample Construction

Several important considerations arise in our dataset that could impact our results. The UCR Program documents arrest data for single year ages between ages 15 and 24, after which they are grouped into five-year intervals, from 25–29, 30–34, etc. This means that we only have individual year level data up to age 24. To account for counts beyond 24, we convert the data for the 25-29 bin into single year proxies by dividing the estimates by five. Since the grouping of arrest counts occurs far enough away from the cutoff of 21, we do not expect this approximation to meaningfully affect our estimates.

To ensure consistency, we assign a midpoint to each single year's age bin under the rationale that this is the average age of everyone at that age in years—for example, age 18 is coded as 18.5 because at any point in time, the average age of all 18-year-olds is (almost exactly) 18.5. For our RD analysis, we focus on individuals aged 18 and up.

It is also important to account for incomplete or inconsistent reporting. 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, those that do not submit data for at least six months in a calendar year are excluded from our sample. We further address non-reporting concerns by standardizing counts using 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 concerns, the UCR remains the most comprehensive national repository for data on arrests.

3.3 CDC WISQARS Nonfatal Injury Data

To measure 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 the years 2001 to 2022. These data originate from the National Electronic Injury Surveillance System–All Injury Program (NEISS-AIP), which tracks ED visits at approximately 100 hospitals nationwide.⁵ The stratified sample provided by the program is constructed 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 seen 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. This helps address concerns about under-reporting or varying enforcement. However, a concern regarding our hospital 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.

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 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 are unintentional. We use PCAUSE codes to exclude unintentional injuries and self-harm, focusing on violence related injuries, which includes confirmed and suspected assaults, for our assault-related outcomes.

Within assault related injuries, we distinguish between several types based on mechanism: assaults involving physical contact are classified under the mechanism “struck by or

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.

against” (hitting, punching, or fighting). We examine these separately from other assault mechanisms such as stabbing or cutting. Additionally, we examine sexual assaults apart from other assaults, which are classified based on intent rather than mechanism and cover all assault-related ED visits involving sexual violence, regardless of how the injury occurred. We focus on cases where sexual assaults involved physical striking—labeled as “sexual struck by/against”—to consider the overlap between physical altercation and sexual violence.

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. Importantly, ED data captures 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 only the incidence of treated assault victimization, as it understates the true rate of victimization.

Building off the existing literature, our study examines whether the observed shifts in violence reflect real changes in criminal behavior or simply just changes in monitoring and reporting. By combining UCR arrest data with WISQARS nonfatal injury data, our approach addresses both offender and victim outcomes. If both data show a significant jump at age 21 and we observe consistent RD patterns, we are more assured that the relationship reflects a behavioral response to alcohol access rather than a measurement error. 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

We begin by briefly explaining the regression discontinuity (RD) design for readers unfamiliar with the quasi-experimental method. In its simplest form, the RD makes use of a threshold or cutoff in a continuous “running” variable, such as test scores, income, or age, where treatment assignment is determined based on a specific value or range of the variable. The framework estimates the causal effect of the treatment or intervention by comparing individuals just below and just above the cutoff. For these comparisons to be internally valid, individuals just below and just above this threshold must be nearly identical in unobservable characteristics. Given that they are, the local average treatment effect (LATE) can be estimated, as the effect is strongest within the narrow range of the cutoff.

In the present study, the threshold is age 21, the legal drinking age in the U.S. Since individuals are unable to manipulate their age, those just younger and just older than 21 are compared in terms of our outcomes of interest. If we observe a significant discontinuity, or spike in an outcome such as arrests or ED visits, at this cutoff, we can attribute that change to the causal effect of legal alcohol access. Thus, the RD methodology is a powerful tool that is widely used in applied microeconomics to infer causality, allowing for credible identification using observational data, especially when randomized trials are not possible (Hahn, Todd, and Van der Klaauw, 2002; Lee & Lemieux, 2010).

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 legal 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 to produce internally valid estimates. One of these is the absence of running variable manipulation (McCrary, 2008). 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 those just below 21 and just above 21 do not differ with respect to both observable and unobservable characteristics besides the gain in

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, and more. If none of the determinants of arrest or an ED visit change discretely at age 21, variation in alcohol access can be considered as good as random near the MLDA-21.

To empirically check whether the second identifying assumption holds, we begin by conducting falsification tests on observable characteristics using a local regression framework on either side of the cutoff. These serve as diagnostic checks for covariate balance—replacing our outcome of interest (arrests or ED visits) with various demographic and structural covariates to test for smoothness across the MLDA-21 threshold. We estimate these models using symmetric bandwidths of three years on either side of the threshold ($h = 3$) and report results under both linear ($p = 1$) and quadratic ($p = 2$) specifications to ensure robustness. These results, shown in Table 2 and Table 3 respectively, are presented before our main estimation to demonstrate the plausibility of local randomization and are confirmed graphically (Appendix Figure A1.1). We formally introduce our RD estimation framework, including functional form and bandwidth specification, in Section 4.2.

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 reports the results of covariate balance tests around the MLDA-21 threshold using arrest data. Panel A presents our population counts, and Panel B includes results for demographic shares. Across all specifications, we find no evidence of a discontinuity at the cutoff. Point estimates are consistently 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 in Table 3 using the WISQARS dataset on ED visits, estimating trends in population and demographic traits using both linear and quadratic specifications. Again, we find no evidence of discontinuity at the threshold. Across all genders and polynomial specifications, estimates are consistently small, statistically insignificant, and stable. Further visual inspection verifies little evidence of a jump around the cutoff, supporting that individuals near age 21 do not differ in demographic composition or population scale.

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 jumps 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.

After 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 in this setting. We use primarily second-order polynomials ($p=2$) and visually verify that results are robust to alternative specifications.

Our baseline regression model is estimated as follows:

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

Here, Y_{it} represents the outcome of interest (arrest or ED visit 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 could affect arrest rates independently of the MLDA-21, including variables like demographic characteristics, population traits, 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 reducing noise. In subsequent specifications in which we pool regressions over specific time periods, fixed effects are included for each period. In subsample analyses, we adjust these accordingly: sample restrictions for ED assault related

instances from 2001–2008 and 2009–2022 indicate that we 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 , captures the discrete change in the arrest or ED visit rates at age 21, which can be interpreted as the causal effect of alcohol access on the outcome of interest. We implement the conventional weighting method of 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. Importantly, we do not include income or employment, to avoid mediator bias. 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 pre-determined 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 we do not directly test the first-stage relationship between the MLDA-21 and alcohol consumption, this decision is grounded in the empirical literature that establishes this link. A large body of work, including Carpenter and Dobkin (2009, 2011), Hansen and Waddell (2018), and Cook and Moore (2001), demonstrates that alcohol use increases sharply and discontinuously at age 21. Moreover, although underage drinking is prevalent, it is likely to bias our results downward. That is, if some individuals under 21 already consume alcohol, then observed increases in arrests and ED visits at the legal threshold likely understate the true effect of alcohol access on violent outcomes. Thus, our estimates represent a lower bound on the behavioral consequences of legal alcohol access.

Given this well-documented and replicated finding, we treat the increase in alcohol consumption at MLDA-21 as a well established behavioral occurrence. Our analysis thus focuses on the downstream effects of that increase, notably, its effects on violent behavior and injury. We can credibly interpret observed changes in arrests and emergency department visits as causal effects of legal alcohol access. We are interested in the reduced-form effects of this policy rather than the mechanics of the consumption response, so our empirical methodology assumes a first-stage effect that is consistently observed in prior work and focuses on its implications.

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 age years on each side of the cutoff ($h=3$) using a quadratic age profile: this is the minimum bandwidth that supports a second order estimation and avoids pre-existing discontinuities at age 18. From Equation (1), we estimate the impact of reaching the MLDA-21 on rates of both aggravated and other (simple) assaults, disaggregated 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 the baseline regression discontinuity estimates for assault related arrests at age 21 during the full sample period, from 1988–2023. For males, reaching age 21 increases arrests for aggravated assault by 0.44 per 1,000, representing a 7–8% increase relative to the baseline mean of 6.24. Other (simple) assault arrests for males rise similarly by 0.94 per 1,000, still approximately 7–8% above its baseline. Among females, from 1988–2023, reaching the MLDA-21 leads to a significant increase of 0.22 per 1,000 ($SE \approx 0.083$) for other assaults, a 4–5% increase above the mean. However, the increase for aggravated assault arrests for females is statistically insignificant over the full 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

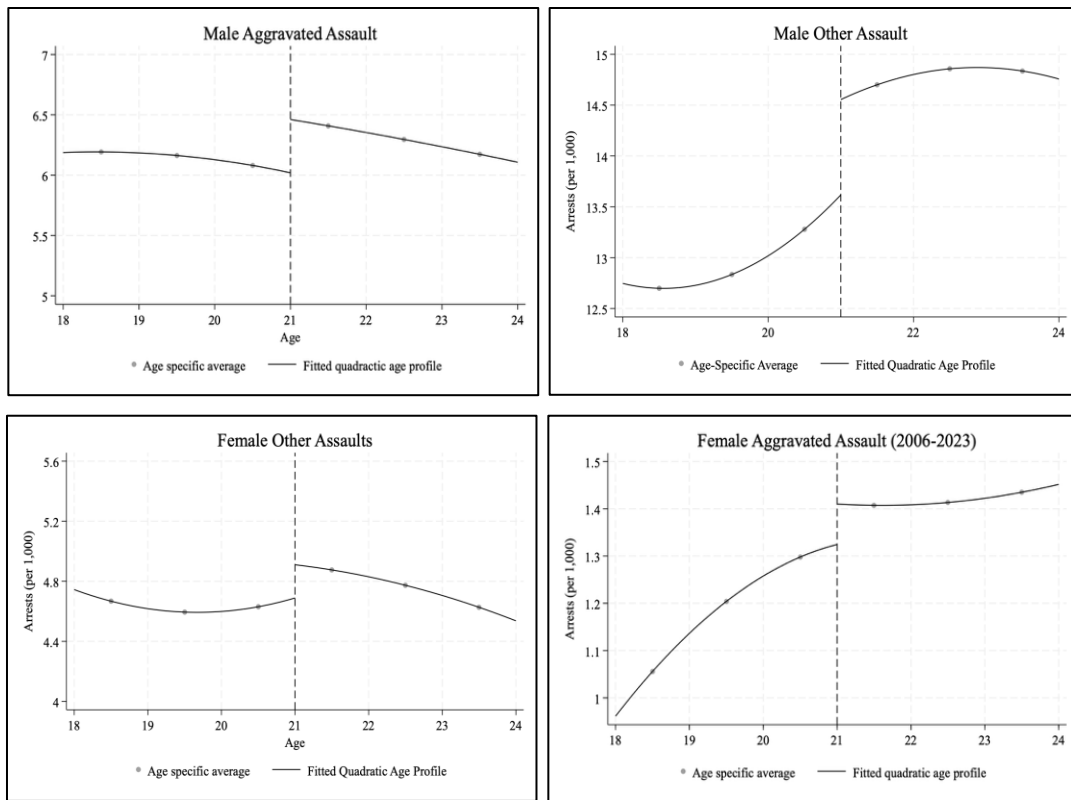
Notes: Each coefficient is the local RD jump in *female* aggravated assault arrests (per 1,000 population) at age 21. Standard errors in parentheses come from `rdrobust` with a quadratic polynomial ($p = 2$), bandwidth = 3, no covariates/weights. “Mean” is the baseline arrest rate (ages 20–21) for each 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 assess whether our effects are driven by recent years, rather than artifacts of the past, we split our 36 years of arrest data in half, dividing them into two sample periods: 1988–2005 and 2006–2023. Among males, we observe that aggravated and other assault arrest effects

remain statistically significant in both periods, though the absolute effect sizes are slightly larger in the earlier sample (0.52 per 1,000 pre-2006 vs. 0.37 post-2006), consistent with higher baseline arrest rates during that period. These correspond to roughly 8% and 7% increases, respectively, relative to their period specific baselines. The similarity in percentage terms suggests that the underlying behavioral response to alcohol access may have remained stable over time, even as overall crime rates declined.

Across the assault types, patterns of aggravated and simple assaults remain relatively consistent across the periods, experiencing a general decline in crime rates in line 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 the prevalence of assault-related crime for women.

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



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

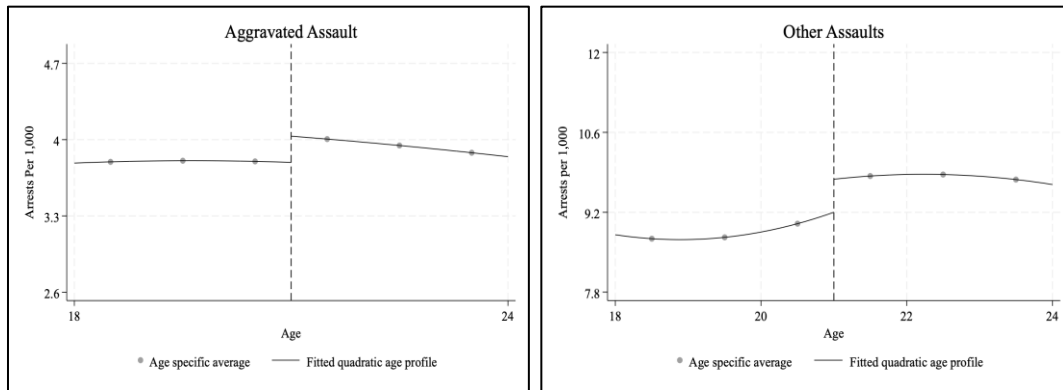


Figure 1 provides visual evidence of the significant discontinuities in assault arrests at age 21 disaggregated by gender as well as for all. Both males and females (the latter for other assault, and aggravated assault post-2006) show clear discontinuities at the MLDA threshold, consistent with a smooth 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 assess the robustness of our preferred estimates, we evaluate alternate linear and quadratic polynomials using varying bandwidths. Since age is discretely measured, we cannot employ the traditional local linear nonparametric approach (Imbens and 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 estimated effects are similar in size. 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 ^{***} (0.100)	0.448 ^{***} (0.104)
Females (2006–2023)	0.072 ^{**} (0.032)	0.066 ^{**} (0.032)
Panel B: Other Assault		
Males	0.952 ^{***} (0.113)	0.981 ^{***} (0.113)
Females	0.225 ^{**} (0.076)	0.251 ^{**} (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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 shows 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. Estimates remain consistent, indicating our results are not driven by bandwidth selection.

5.1.3 Robustness Checks

To further validate our main estimates, we conduct a series of robustness checks using alternate specifications to assess the sensitivity of our results to different modeling and data assumptions. These checks address concerns related to skewed distributions in arrest rates, unobserved time-variant confounders, and inconsistencies in data coverage across agencies. Specifically, we estimate results for aggravated and other assault arrest rates, disaggregated by gender, using log-transformed outcomes, covariate-adjusted regressions (which include race shares and year fixed effects), and population-weighted models.

Table 7: Robustness Checks – Log, Covariate, Weights

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

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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

In Table 7, the main point estimate results remain consistent in direction, magnitude, and significance using narrower and wider age bandwidths. Log-transformed models yield elasticity estimates of approximately 7–8% for males and 4–5% for females, like our baseline percentage effects and suggesting the results are not driven by outcome skewness. Covariate adjustment and population weighting also produce effect sizes that are substantively similar to the baseline, particularly for males.

5.1.3 Effects on Other Crimes

Having considered the robustness of our main results, we turn to estimating the effect of gaining access to alcohol on different crime types. Namely, we focus on property crimes (burglary, larceny, and robbery), as these are similarly classified as serious FBI “index” crimes. Estimating both linear and quadratic order specifications in Table 8, we find negligible effects of reaching the MLDA-21 on crime commission.

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

	Linear	Quadratic	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 and its standard error. The first specification estimates a linear polynomial ($p = 1$) with $h = 2$, while the second estimates a quadratic polynomial ($p = 2$) with $h = 3$. “Mean” indicates the baseline arrests per 1,000 individuals aged 20–21. Significance stars (e.g. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$)

The arrest rates for crimes less likely to be impulsively committed exhibit no significant discontinuities at the threshold. Crimes such as larceny, or the burning down of buildings, typically require greater planning, effort, and strong material motives. As described earlier, the pharmacological effects of a substance like alcohol impairs finer judgements, especially for younger individuals. As shown in Table 8, rates for these crimes do not exhibit significant discontinuities at age 21, despite being classified as some of the most severe by the FBI. We graphically check this analysis and observe no discontinuities at the threshold for both males and females, as shown Figures 2.1 and 2.2.

Figure 2.1: Placebo Discontinuities at Age 21 – Males

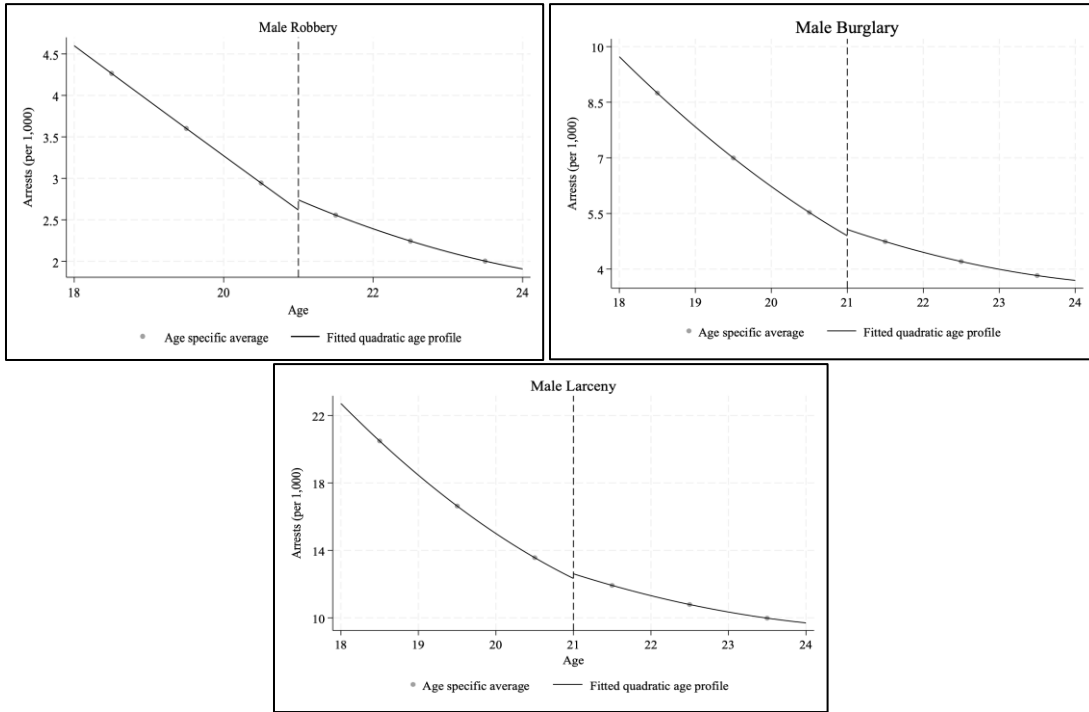
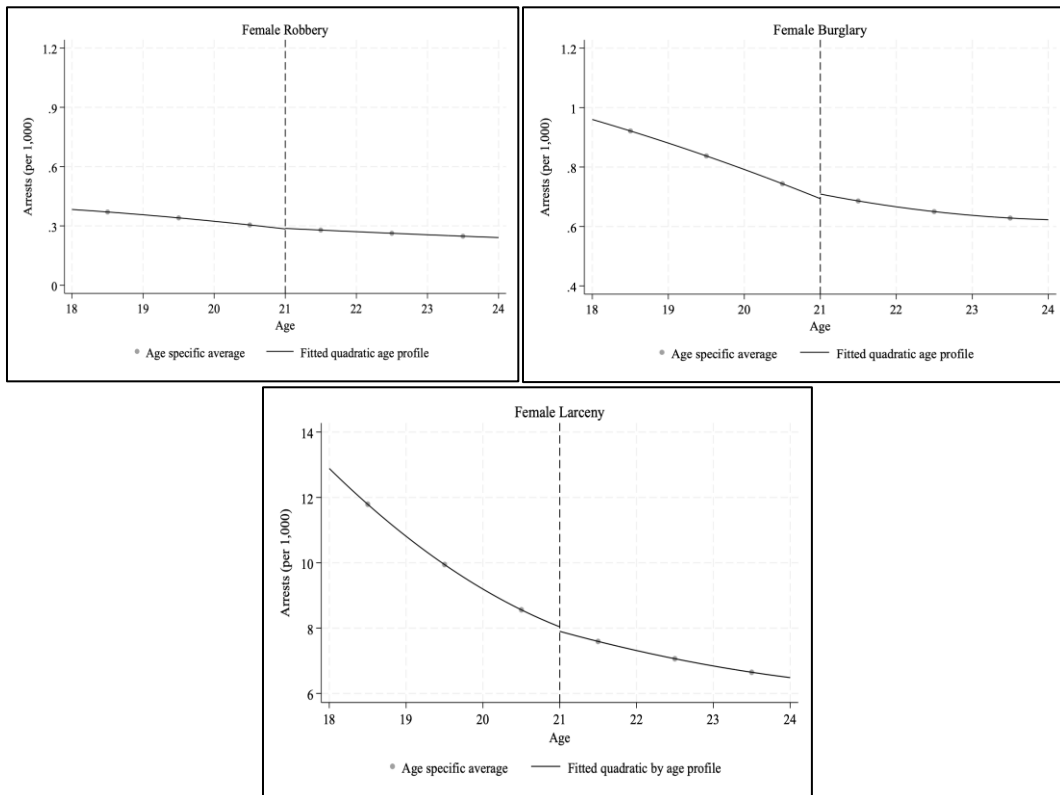


Figure 2.2: Placebo Discontinuities at Age 21 – Females



In these models, the three placebo crimes exhibit no significant jumps at 21, consistent with the idea that impulsive, alcohol-linked aggression, manifesting as assault, could be the collective response to legal access to alcohol.

5.1.4 Placebo Age Tests

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

	Baseline	Log	Covariates	Weights
Panel A: Aggravated Assault				
Males				
<i>Age 21</i>	0.426*** (0.045)	0.069*** (0.010)	0.392*** (0.039)	0.411*** (0.043)
<i>Age 23</i>	0.032 (0.051)	0.004 (0.008)	0.023 (0.041)	0.030 (0.050)
<i>Age 24</i>	-0.044 (0.049)	-0.003 (0.008)	-0.027 (0.039)	-0.035 (0.077)
Females (2006–2023)				
<i>Age 21</i>	0.059*** (0.018)	0.042*** (0.014)	0.059*** (0.012)	0.059*** (0.018)
<i>Age 23</i>	0.028 (0.019)	0.022 (0.015)	0.023* (0.013)	0.026 (0.018)
<i>Age 24</i>	-0.013 (0.016)	-0.010 (0.012)	-0.013 (0.016)	-0.010 (0.024)
Panel B: Other Assault				
Males				
<i>Age 21</i>	1.12*** (0.069)	0.078*** (0.007)	1.08*** (0.034)	1.10*** (0.072)
<i>Age 23</i>	-0.032 (0.073)	-0.002 (0.007)	-0.065 (0.050)	-0.0234 (0.072)
<i>Age 24</i>	0.061 (0.070)	0.005 (0.006)	0.122** (0.059)	0.063 (0.140)
Females				
<i>Age 21</i>	0.279*** (0.041)	0.060*** (0.010)	0.255*** (0.028)	0.277*** (0.042)
<i>Age 23</i>	-0.008 (0.040)	0.001 (0.008)	-0.022 (0.026)	-0.009 (0.041)
<i>Age 24</i>	-0.022 (0.026)	-0.003 (0.006)	0.034 (0.023)	-0.013 (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, our results align with the premise that something behavioral about aggravated and other assault is occurring at the threshold that does not occur at other ages. Appendix Figures A3.1 and A3.2 graphically confirm that conducting our analysis at ages 23 and 24 produces negligible, statistically insignificant discontinuities in assault arrests.

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

Having established a relationship on the offender side of violence, we now turn to the victim perspective to see whether parallel evidence is present in nonfatal injury data. If the MLDA-21 does lead to more frequent or more severe altercations, we would expect an increased risk of being harmed as well. As Chalfin acknowledged, 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 victimization data, addressing this gap by analysis of 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

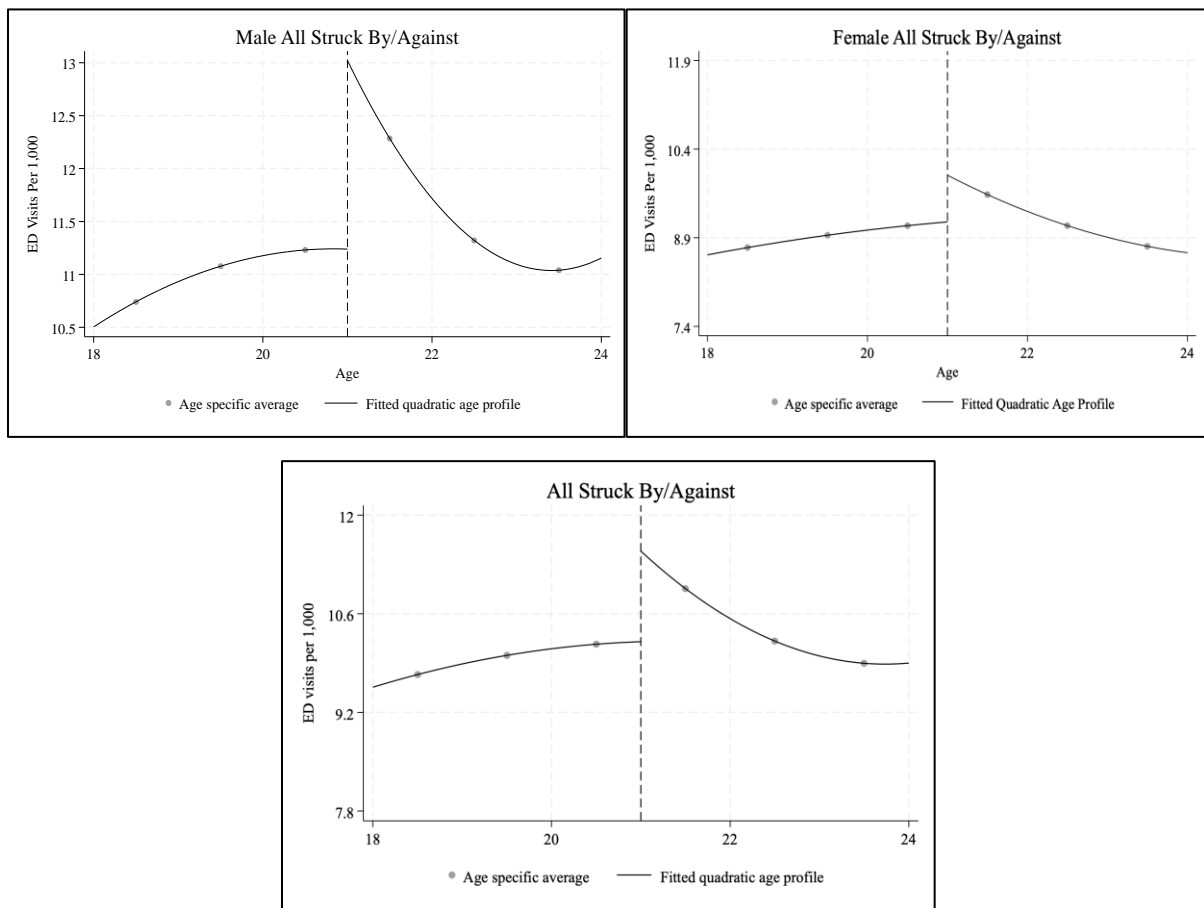
Notes: RD estimates measure the discontinuity in ED visit rates per 1,000 individuals at the MLDA-21 threshold. All models use triangular kernels with $h = 2$, $p = 0$, and include year fixed effects. Means refer to average ED visit rates for individuals aged 20–21. Significance of assaults come primarily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

Table 10 shows that for both female and male ED visits, total assault rates 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 (10.98), and for males, the effect is larger, 1.626 per 1,000 (SE = 0.336), or 10.3% above baseline. 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 the significance of this effect is driven primarily 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 A4.3).

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 visits. For males, there is an increase of 1.458 per 1,000 ED visit cases, raising the rate by ~12.5% given a baseline of 11.75 per 1,000 visits. 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 finding is 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.

5.2.1 Alternative Bandwidth Sensitivity and Robustness Checks

Across alternate specifications, the discontinuity in all struck by/against ED visits at age 21 is large and robust: 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
<i>h(2), p(1)</i>					
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
<i>h(3), p(1)</i>					
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
<i>h(3, 6), p(2)</i>					
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
<i>h(3, 9), p(2)</i>					
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 a 7–10% increase in risk. Covariate-adjusted models that include race/ethnicity shares, as well as population-weighted models, produce nearly identical estimates as the baseline.

5.2.3 Gender Differences and Time-Period Splits

Separating mechanism and halving our sample (pre and post 2009 periods), we observe that struck by/against victimization outcomes reveal differences by gender.

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** (0.097)	0.765*** (0.121)	−0.064 (0.112)	1.26
	Non-Sexual Struck-by	0.470** (0.210)	0.178 (0.418)	0.636*** (0.212)	8.10
Male					
	Sexual Struck-by	0.015 (0.019)	−0.003 (0.022)	0.025 (0.028)	0.06
	Non-Sexual Struck-by	1.444*** (0.263)	2.157*** (0.527)	1.036*** (0.280)	11.70

All models use local constant regressions ($p = 0$) with bandwidth $h = 2$, a triangular kernel, and include year fixed effects: y_2 – y_8 for 2001–2008, y_{10} – y_{22} for 2009–2022. Mean values reflect average ED visit rates per 1,000 individuals aged 20–22. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

As seen in Table 12, the estimated effect for male non-sexual struck by assaults falls by more than half between the earlier (2001–2008) and later (2009–2022) periods. This corresponds with a steady decline in baseline ED visit rates over time, particularly during the 2010s (Figure A4.2). Average annual ED visit rates for male non-sexual struck-by assaults fell from approximately 13.1 per 1,000 in the early 2000s to under 10 per 1,000 in the most recent years, a decline that follows the halving of the estimated discontinuity. It is probable that changes in effect size reflect declining base rates, rather than shifts in the underlying behavioral response. Appendix Figure A4.2 illustrates this overall downward trend in assault-related ED visits during this period.

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 approximately a 67% relative increase. However, the 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 observed to be negligible across both time periods, while non-sexual assaults consistently explain the bulk of the MLDA-21 discontinuity in ED visits, as estimates exceed 1.0 throughout. The findings suggest that young males are more likely to engage in mutually aggressive altercations, reflecting a generalized view of gender differences in risk exposure and behavior post-access to alcohol.

5.2.4 Placebo Age Checks

To ensure this is a unique effect at the legal threshold of 21, rather than one across all birthdays, we estimate a series of placebo regressions using the same main specification, applying the cutoff at alternative ages from 23 to 28 years. Unlike FBI UCR data, our WISQARS ED data is available on a yearly basis for the entire sample, so we are able to extend the windows of our analysis further. The selected placebo thresholds occur at a post-treatment range, as no policy change or institutional shock is expected beyond 21. 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 disaggregation by sex, we find no statistically significant discontinuities at any alternative age cutoffs. While point estimates fluctuate in sign and magnitude, they remain consistently negligible across the board. For example, 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 near age 27, the estimates are statistically indistinguishable from zero and exhibit wide confidence intervals.

Taken together, these results provide evidence that the increase in ED visits for assault victimization at age 21 is driven almost entirely by struck-by/against mechanisms. These cases are defined as ones involving direct physical contact such as punching, pushing, or fighting, and represent the most common form of assault related ED visits. As such, they serve as a useful

proxy for mutual altercations rather than premediated or weapon-based ones. Although our dataset also includes sexual and other types of assault, the discontinuities at the MLDA-21 threshold are concentrated in physical contact cases. As described earlier, these interactions are plausibly heightened by alcohol access through pharmacological links, such as impaired judgement and short-sightedness in younger individuals.

The alignment of our findings across arrest and ED data at age 21 supports the interpretation that legal alcohol access generates a sharp increase in both assault perpetration and victimization, primarily through spontaneous, mutually aggressive encounters. The patterns are robust across sexes, bandwidths, model specifications, and they are also not present at placebo age thresholds. By incorporating both offender and victim outcomes and distinguishing among assault types and mechanisms, the present study offers compelling evidence that legal access to alcohol meaningfully increases real-world violence and its associated harms.

6 Discussion and Concluding Remarks

In this study, we provide new evidence that the U.S. minimum legal drinking age policy has a serious and measurable impact on violent outcomes. Upon reaching the legal drinking age, young adults experience sharp increases in both the perpetration and victimization of assault. As captured by national arrest data, we find that arrest rates for aggravated and other (simple) assaults rise by 5–8% at the MLDA-21, with particularly strong and consistent effects among males across the full 36-year sample. For females, simple assault effects increase significantly across the entire period, while the aggravated assault effect becomes significant only in the later years.

Similar trends are observed using nationally representative ED visit data. We find substantial increases in victimization rates for assault related injuries, especially physical injuries requiring ED visits. While female victimization patterns have shifted over time, from sexual struck by/against assault injuries prior to 2009 to more general physical aggression injuries afterwards, the data indicate that legal alcohol access intensifies physical violence across both genders. In some estimates, male ED admissions rise by as much as 12–13%. Ultimately, our findings on assault related morbidity incidents show that the rise in assault related injuries corresponds to a parallel rise in assault related arrests—a convergence of offender and victim data points that supports an alcohol-induced increase in violent altercations upon legal access.

6.1 Limitations and Future Directions

While our findings are robust, some divergence from prior studies is worth noting. Earlier RD analyses (Chalfin et al., 2023) reported increases in robbery victimization or property crime victimization among certain subgroups (12% increase for male burglary and larceny), for which we do not replicate. There are a few plausible reasons for these discrepancies. First, we use nationally aggregated data spanning more than three decades, whereas other studies rely on more administrative or state-specific data for 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 periods. Second, across states, there are differing definitions of “youth” or restricted geographies, which could yield different patterns for lower frequency crimes, including robbery. Additionally, the distinction between aggressive theft and planned property crime can be blurry; slight differences in classification or underreporting could obscure effects. Finally, it is worth

noting that differences in geographic scope or population subgroup may explain why certain subpopulations respond more strongly in one setting than another.

Another longstanding concern in MLDA research is whether observed spikes in violence or other outcomes at 21 simply reflect a brief “birthday celebration” phenomenon rather than legitimate, sustained increases in alcohol availability and consumption. The bulk of the literature (Carpenter & Dobkin, 2011; Hansen & Waddell, 2018; Chalfin et al., 2023) has largely disproved this explanation, explicitly testing for short-term “partying surges” around the 21st birthday. They find that while some heightened incidents do occur near the exact birthday date, longer term discontinuities tend to persist for months or longer. Our results, though we do not directly evaluate the days and weeks following the birthday, show persistent discontinuities across multiple bandwidths and age bins around the cutoff. These are consistent with the previous consensus of the literature, indicating that the legal alcohol access effect extends beyond an acute “turning 21” celebration.

We also acknowledge that turning 21 in the U.S, like turning 18, is considered the eligibility threshold for more than just alcohol. Other notable adult activities, such as gambling, license to bear arms (in some states), cannabis (where legal), and more, enter the picture. While these in theory could serve as confounding co-treatments, the literature provides substantial evidence otherwise, finding that the dominant behavioral change at age 21 is the jump in legal alcohol consumption. The additional exposure to these activities appears less widespread and relatively minor in comparison to alcohol, making them unlikely to explain the observed discontinuity in assault and injury outcomes.

6.2 Future Directions for Policy

Given recent initiatives and successfully enacted policies of states to lower the legal drinking age, our findings are especially timely. They hold implications for both policymakers and public health officials. Here, we show that reaching the MLDA-21 is much more than a celebration of adulthood, but potentially a societal inflection point. It sets off an enduring escalation in both alcohol-driven aggression and vulnerability to violent assaults. Legal access appears to increase both the opportunity and social acceptability of drinking, which in turn facilitates riskier behavior, especially violent ones. For young adults, this event is associated with a rise in risk-taking, impulsivity, and interpersonal conflict that could lead to criminal charges, physical injury, and increased strain on health and justice systems.

The parallels we observe between arrest data (offenders) and ED visit data (victims) reinforce the argument that the MLDA-21 drastically changes the social environment in which young adults consume alcohol. This shift manifests into tangible harm for the participants of the encounters as well as for the broader healthcare system bearing the economic burden. Critically, these effects remain robust across model specifications, bandwidths, placebo checks, and weighting schemes; however, we recognize that they reflect one institutional context—legal alcohol access at age 21 in the U.S.—and are not definitive evidence that any alternative threshold would necessarily be better or worse.

Some critics have argued that lowering the drinking age would promote safer, more supervised alcohol consumption, especially under the watchful eye and within the household bounds of parental figures. However, our results provide little support for this hypothesis, aligning with the broader empirical literature that documents sharp and immediate increases in alcohol-related harm following legal access. Lowering the MLDA may not mitigate these risks and would likely shift the effects to a younger, more developmentally vulnerable group and potentially even extend the window of harm.

Importantly, any empirical test of a policy change with the MLDA in the U.S. necessitates an extensive time horizon and careful causal inference. Changes in drinking behavior, societal norms, enforcement, and health outcomes take place gradually and vary by region. In the short term, if one were to occur, we would most likely observe the impact of policy itself—an immediate jump in alcohol availability and its harms at the onset—rather than seeing the benefit of increased supervision or cultural or institutional adjustment.

As such, any decision to change the MLDA should be guided by a rigorous and comprehensive cost-benefit analysis. While some argue for the theorized benefits to lowering the drinking age, most of the empirical evidence points to the risks in the context of current U.S. enforcement and norms. Our study contributes to this debate by demonstrating that legal access at 21 produces significant increases in violent behavior and physical harm. Lowering the threshold could worsen outcomes for both individuals and institutions.

Future research should conduct cross-national comparisons for more insight into factors contributing to alcohol-related harms. It is often argued, in popular media and public discourse, that European countries with lower legal drinking ages experience comparatively lower rates alcohol-related harms. However, such comparisons should be made cautiously and consider key

contextual differences that influence these outcomes, such as stricter enforcement of alcohol regulations, differing cultural norms around moderation, and more robust public transit systems that discourage alcohol-impaired driving. Studies should continue to explore these factors and assess whether beneficial components of other models could be transferable to the U.S. landscape. It is likely that without establishing institutional supports, lowering the MLDA in the U.S. would simply replicate, or even exacerbate, the harms documented from existing studies of the MLDA-21.

In addition, our findings raise two further avenues for future study. First, heterogeneity in alcohol enforcement or access may impact the strength of the MLDA-21 effect. In jurisdictions where pre-21 access to alcohol is already common, the behavioral impact of reaching the legal threshold may be less prevalent or muted entirely. Conversely, in settings where control is more stringent, the discontinuity may be more pronounced. Second, our findings may interact with geographic factors, such as urbanicity. If increased alcohol access correlates with greater driving exposure in low-density areas, the consequences of impulsive behavior may be more severe. Stratified analyses by region, enforcement strength, or driving prevalence could yield additional insight into these mechanisms.

At present, the evidence suggests that legal access to alcohol imposes large and measurable costs, significantly impacting those who drink as well as those around them. Moving forward, research should continue to investigate how alcohol policy interacts with gender, geography, enforcement intensity, and societal norms to shift behavior. In the meantime, policymakers should proceed with caution before relaxing the MLDA, as the consequences of such an action could be immediate, costly, and difficult to reverse.

7 Bibliography

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8 Appendix

Figure A1.1: Covariate Smoothness Controls

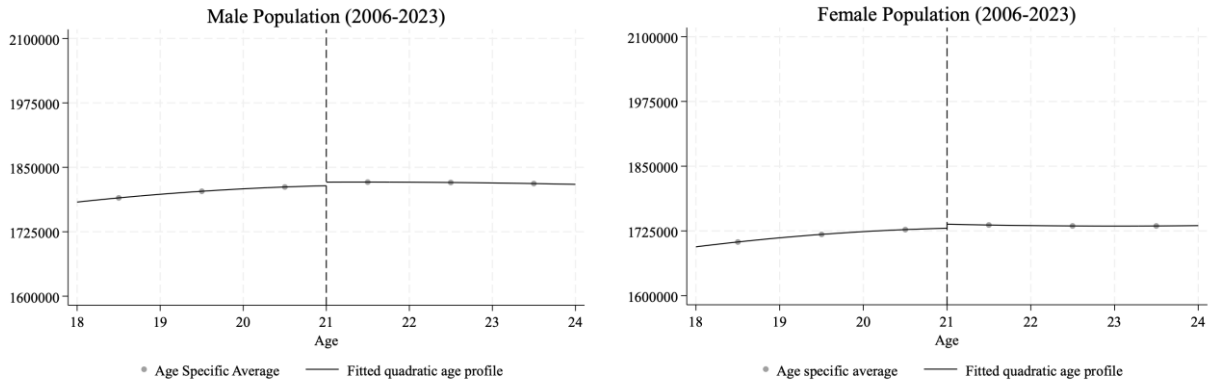
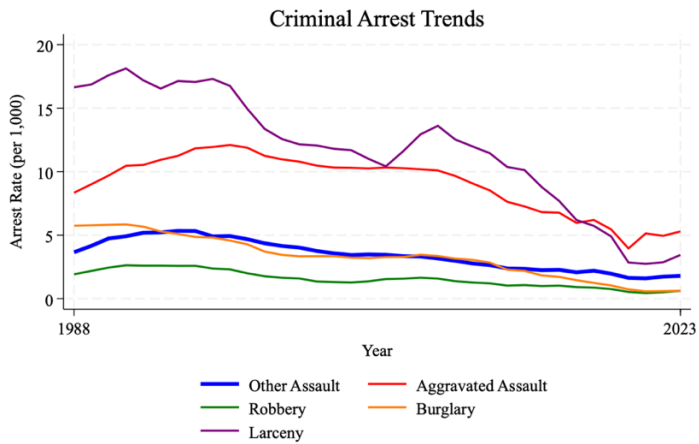


Figure A1.2: U.S FBI UCR. Crime Trends



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)
<i>Full Sample (1988-2023)</i>		
Aggravated Assault	0.442*** (0.108)	6.24
Other Assault	0.937*** (0.120)	14.00
<i>Pre-2006 Period (1988-2005)</i>		
Aggravated Assault	0.517*** (0.157)	8.18
Other Assault	1.070*** (0.156)	17.50
<i>Post-2006 Period (2006-2023)</i>		
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)
<i>Full Sample (1988-2023)</i>		
Aggravated Assault	0.042 (0.025)	1.51
Other Assault	0.221** (0.083)	4.75
<i>Pre-2006 Period (1988-2005)</i>		
Aggravated Assault	-0.001 (0.026)	1.66
Other Assault	0.195** (0.069)	4.69
<i>Post-2006 Period (2006-2023)</i>		
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 A3.1: Placebo Assault Arrest Discontinuities at Age 23

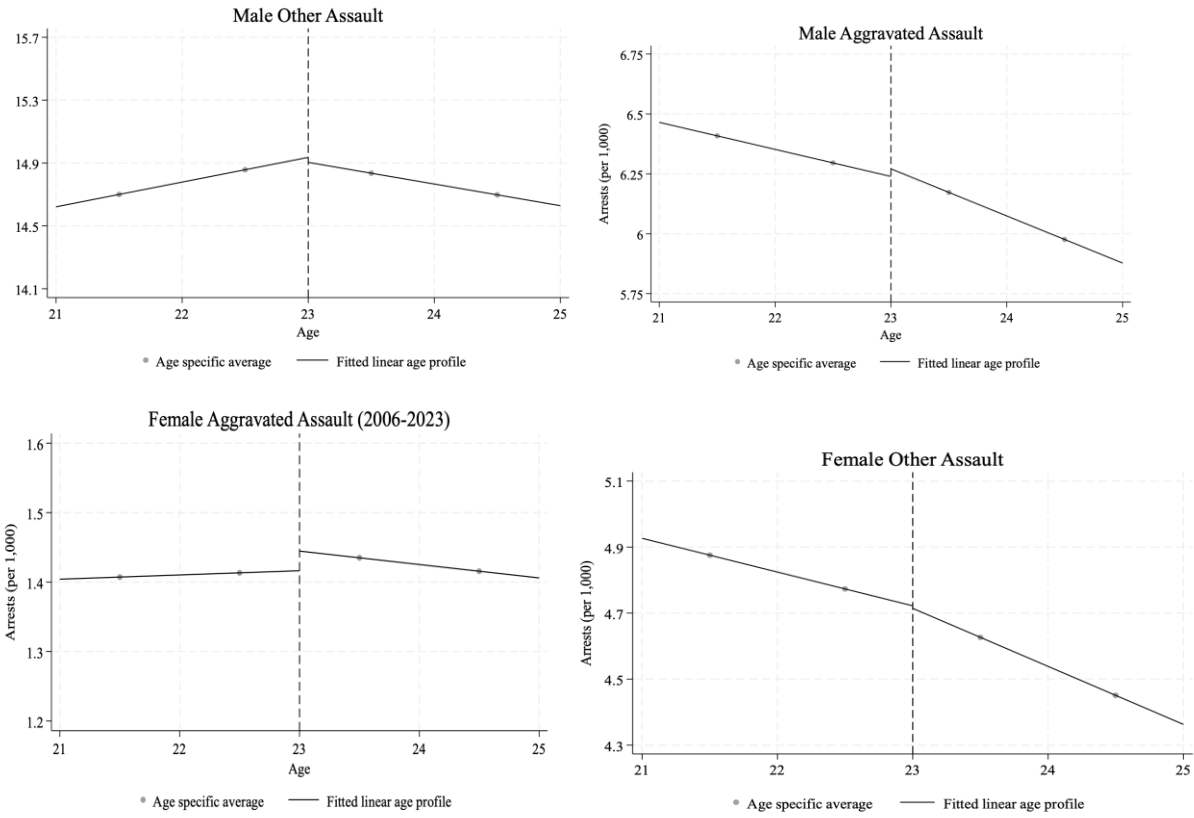


Figure A3.2: Placebo Assault Arrest Discontinuities at Age 24

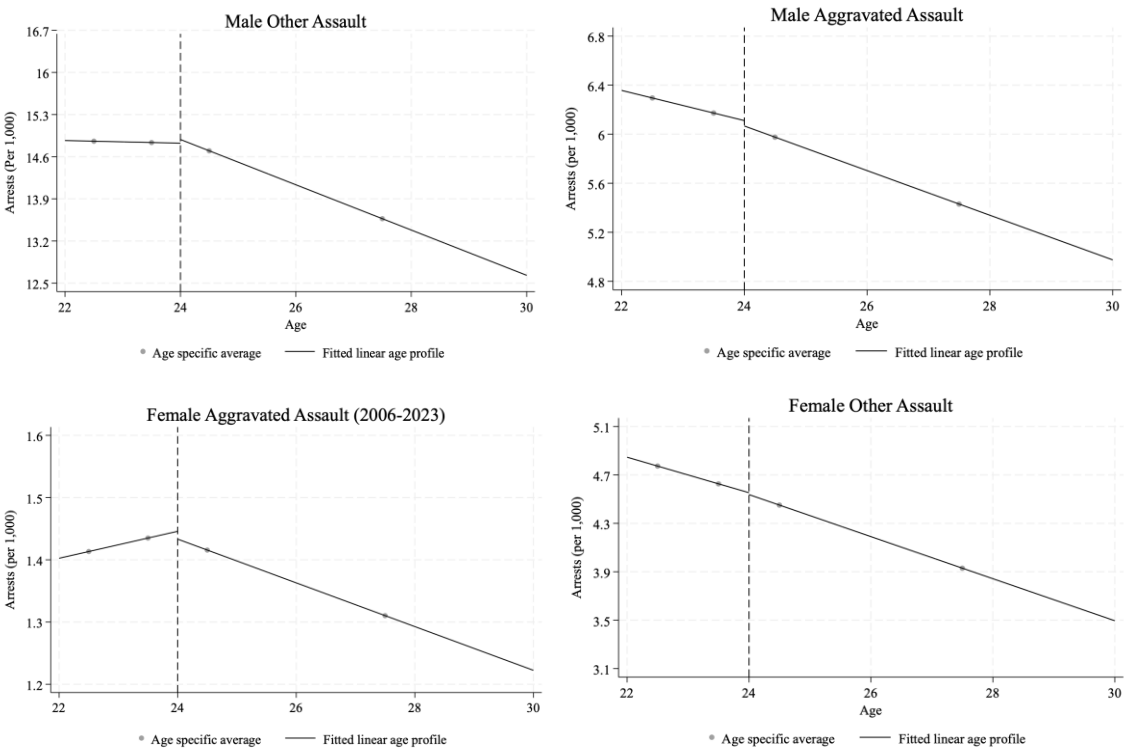
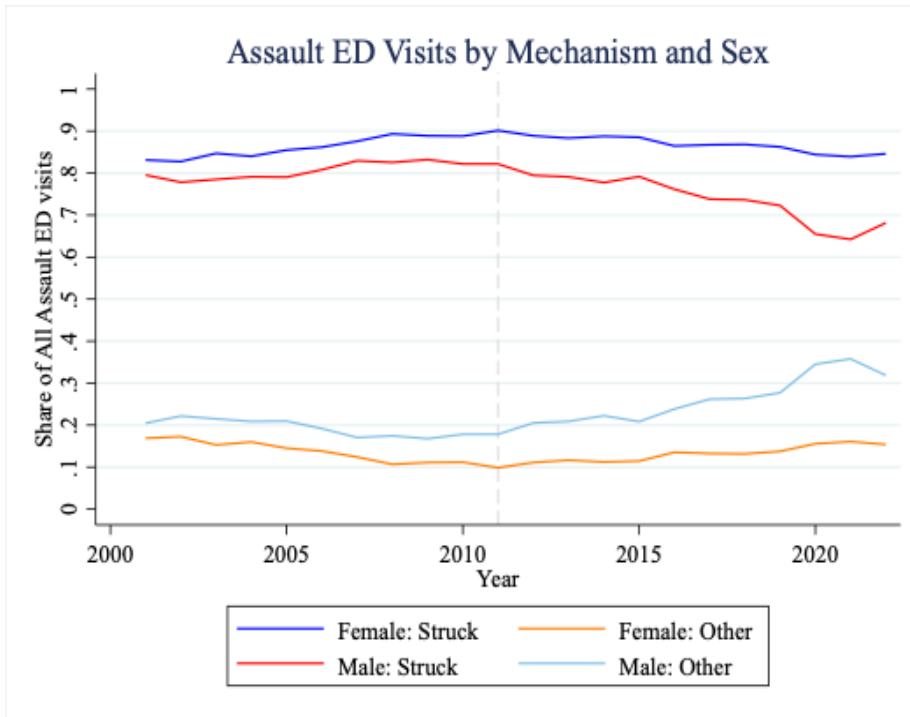


Figure A4.1: Share of All Assaults in ED Visits from 2001-2022



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.

Figure A4.2: Time Series of Assault Related ED Visits in the U.S.

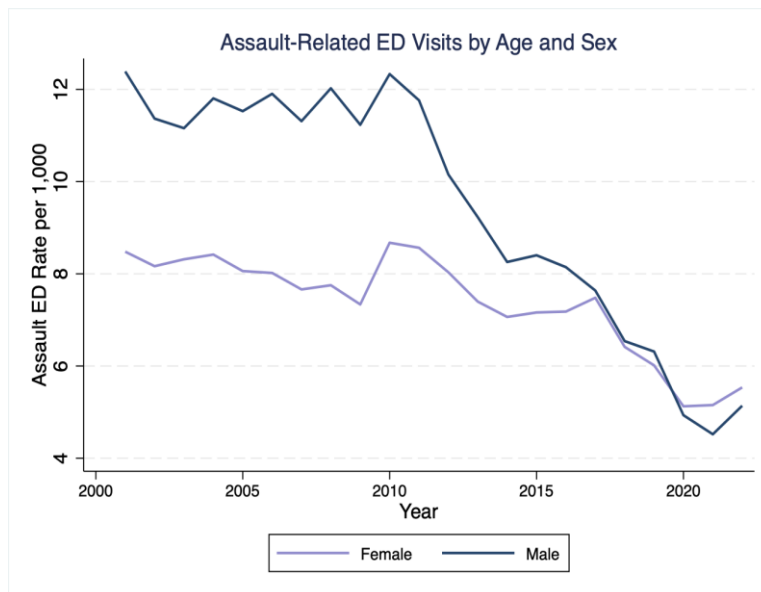


Figure A4.3: Share of Struck By/Against Related ED Visits

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 A.4.5: Unintentional Struck-by ED Visit Rates (per 1,000)

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).