Alcohol Sales to College Football General Seating Sections Reduces Crime: Evidence from NIBRS, Sales Legalizations, and the Minimum Legal Drinking Age

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Abstract

Alcohol sales to general seating at college football games has been broadly adopted despite results suggesting greater alcohol availability increases crime and college alcohol abuse. Crime reductions are found after sales are allowed. To address omitted variables, age-based variation in post-sales, stadium alcohol availability is employed, finding legally aged individuals are less likely to commit crimes and be victims after sales are allowed. Evidence shows age variation controls for time-varying differences in clearance rates. Results suggest substitution effects matter for the relationship between alcohol access and criminal behavior, identifying a new empirical nuance to be heeded when making alcohol policies.[1]

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1 Introduction

One factor in growing college cost is growing expenses of athletic programs; many college teams subsidize their athletic programs from general funds, student fees, and government appropriations (Sackstein, 2019). Average college football attendance fell from 46,328 in 2007 to 43,070 in 2016, negatively affecting revenue (Stankiewicz, 2017).[2] Attempting to raise additional revenue, many universities have recently removed restrictions on selling alcohol at college football games. From 34 teams allowing alcohol sales in 2015, now at least 78 teams allow sales (Hayes, 2019). West Virginia University (WVU) reported $500,000 dollars in alcohol concession revenue in the first season of sales (Tracy, 2015) and the University of Texas, earned $1,100,000 from alcohol sales in its first two home games of 2016 (Knox, 2016).[3]

Allowing alcohol sales at sporting events is controversial. Selling alcohol draws the ire of the National Collegiate Athletic Assocation (NCAA), which bans alcohol sales and advertisements at championship events, and Mothers Against Drunk Driving (MADD) which is opposed to allowing in-game alcohol sales (MADD, 2019).[4] The National Institute of Alcoholism and Alcohol Abuse (NIAAA) recommends banning alcohol sales at college sporting events, rating bans as moderately effective for reducing student alcohol consumption (NIAAA, 2015, pg. 19). Alcohol access reduces academic performance on university exams (Carrell, Hoekstra, & West, 2011; Lindo, Swensen, & Waddell, 2013) and increases crime (Carpenter & Dobkin, 2015; Heaton, 2012). In 2010, alcohol abuse cost $293.4 billion (Sacks, Gonzales, Bouchery, Tomedi, & Brewer, 2015).

Prior work finds college football home games are associated with higher amounts of crime and suggests a mechanism is additional alcohol consumption due to gameday festivities (Lindo, Siminski, & Swensen, 2018; Rees & Schnepel, 2009). A review on alcohol control and crime argues college football gamedays and the National Incident-Based Reporting System (NIBRS) dataset are effective tools for understanding the link between alcohol access and crime (Carpenter & Dobkin, 2010).[5] This study uses multiple stadium policy changes to investigate how allowing in-stadium alcohol sales affects home gameday crime.

[2] Also, litigation is being pursued by student-athletes to receive compensation (Sanderson & Siegfried, 2015).

[3] Note that raising additional revenue could be consistent with universities being revenue or profit maximizers. Bowen's revenue theory hypothesizes that colleges are revenue-maximizers. (Bowen, 1980). An alternative model for the growing cost of college is Baumol's Cost Disease (Archibald & Feldman, 2008).

[4] Part D of MADD's "Alcohol Policies for the Colleges."

[5] College gamedays and NIBRS are used in Rees and Schnepel (2009) and Lindo et al. (2018).

Causal studies, outside of college football contexts, overwhelmingly find increased access to alcohol increases crime. Using temporal variation has shown that increased alcohol access increases crime (Heaton, 2012) and reduced alcohol access reduces late-night hospitalizations (Marcus & Siedler, 2015). Similar findings are reported in Biderman, De Mello, and Schneider (2010); Carpenter and Dobkin (2015, 2017); Grönqvist and Niknami (2014); Nakaguma and Restrepo (2018).

Internally valid causal estimates of the effect alcohol access on consumption and crime are environment specific. Carpenter and Dobkin (2015) finds alcohol access increases DUI in California, but Lindo, Siminski, and Yerokhin (2016) finds barely exceeding the minimum legal drinking age (MLDA) in New South Wales does not cause increases in DUI. An interesting dimension of using college football alcohol policy changes as variation to measure alcohol access on crime is the change affects a few hours in 1 day as opposed to using weekend-length or age-based alcohol access restrictions.[6] Long time periods affected by changes to sales policy make it difficult to understand substitution effects of easing/restricting access to alcohol (Carpenter & Dobkin, 2010).

Studies focused on college football-alcohol sales find mixed results. Due to substitution across time and types of liquor, the three-period, pre-game, game, and post-game, utility maximization model of Boyes and Faith (1993) is unable to predict the expected sign of college football alcohol sales on alcohol consumption.[7] Some research finds a positive association between college football alcohol sales and crime (Bormann & Stone, 2001; Menaker & Chaney, 2014). Other research finds banning alcohol sales increases the blood alcohol content (BAC) of drivers suspected of driving under the influence (DUI) (Boyes & Faith, 1993). Mixed results call into question the generalizability of prior studies on alcohol access and crime to the college football context and are a dissonance in prior college-football specific research to re-examine with multiple policy changes.

In this paper, parsimonious differences-in-differences (DD) model estimates suggest alcohol sales to general seating reduces total crime.[8] The parsimonious DD estimate represents alcohol ac-

[6] The length of the intervention is emphasized in non-economics literature, see the review in Hahn et al. (2010).

[7] The reason the sign is ambiguous in Boyes and Faith (1993) is due to substitution across time and types of alcohol. This substitution across time arises due to bans increasing the cost of in-game alcohol consumption. The substitution across types of alcohol arises from imperfect ban enforcement. It is easier to conceal smaller volumes of higher-proof alcohol which allow an equivalent amount of alcohol consumption as higher volumes of lower-proof alcohol.

[8] Like Rees and Schnepel (2009), Heaton (2012), and Lindo et al. (2018), the treated group are home games and the non-treated group are non-home games. Like Heaton (2012) the post-period is after the change, so the approach compares home to non-home games before and after the change to sales.

cess, plus time-varying, correlated, omitted factors.[9] To investigate whether the negatively signed DD coefficient is a result of bias from unobservable factors, age-based variation from minimum legal drinking age laws in post-period legal access to in-stadium alcohol is used.[10,11]

To illustrate the usefulness of age-based variation in this context, consider a potentially worrisome confounder: law enforcement strategy/deployment numbers. It is possible, even likely that agencies adjust their enforcement strategy (only on home gamedays) after the change to sales.[12] However, law enforcement does not know the age of a suspect a priori, so it is unlikely that enforcement changes discontinuously across the legal alcohol access age cutoff. Other potential confounders, like game scheduling differences and other stadium policies, are also difficult/impossible to precisely age discriminate at the age cutoff.[13]

To combine age-based variation and the temporal change for a control group less vulnerable to omitted confounders, above legal age is used in triple difference and in a fixed effects RD (Pettersson-Lidbom, 2012). The negative sign is not changed by using this strategy to adjust for bias. Individuals who can legally purchase alcohol are less likely to commit or be victims of certain types of crime on home gamedays. After the change to sales, legally aged individuals are less likely to commit or be victims of simple assault. Results suggest length of time affected by alcohol control policies are important due to substitution effects. The following sections outline the data and methods, establish robustness of the difference in difference results and plausible policy exogeneity, then addresses time-varying unobservables with age-based variation.

[9] Potential confounders must be differently affect home game days. It does not matter if omitted variables change at the same time as sales are allowed, as long as the change equally affects home and non-home games.

[10] This variation is extensively used in age-based regression discontinuity estimates of the effect of alcohol access on crime (Carpenter & Dobkin, 2015; Lindo et al., 2016) and other outcomes (Carpenter & Dobkin, 2017; Carrell et al., 2011).

[11] Figure A.1 shows the age-based variation over time that is leveraged, showing strong similarity to Figure 1 in Lemieux and Milligan (2008). Lemieux and Milligan (2008) studies the effect of unemployment benefits after a law change that makes them available to individuals under age 30.

[12] WVU athletic director during 2011, Oliver Luck, noted he planned to hire more event staff. He said, “It would be irresponsible not to bump up our security.” (Penn Live, 2011)

[13] For example, WVU removed halftime re-entry at the same time as alcohol was allowed to be sold inside. The policy ended for all ages.

2 Data

Three primary sources of data are used. Crime data comes from NIBRS, college football game data is scraped from cfbstats.com, and alcohol sales data is manually collected. Each of these sources is discussed in turn. The research question and design requires a panel of agencies covering college teams over time, both before and after the change to allowing sales.

2.1 College Football Games

The data source for college football data is cfbstats.com which includes attendance. The data was available back to 2008, so that is used as the first year of the sample. For a robustness check, additional information about game times is web-scraped from ESPN.com.

2.2 Crime

NIBRS provides crime reports by incident. For some crimes, designated by the FBI as Type A, there is data by incident including age, time of offense, whether or not alcohol was suspected of being used at time of offense, etc.[14,15] Each incident is counted as one, ignoring the number of offenders per incident.[16,17]

Because the question relates to university policy and college football, the sample is only agencies covering the county or university of a D-1A team in the NIBRS dataset. Not all college cam-

[14]Type B crimes do not include whether the offender was suspected of using alcohol or time of day that a crime occurred, and generally provide less accompanying information than Type A crimes.

[15]Since the research question is measuring the impact of alcohol sales on criminal behavior, following a suggestion in (Carpenter & Dobkin, 2010) and dependent variable selection approach of Rees and Schnepel (2009) and Heaton (2012), several different types of crime that alcohol availability could affect are used. Definitions of each crime type is provided in Table A.2.

[16]Most incidents involve 1 offender.

[17]Incidents which record only the date of recording and not the date of the incident occurring are dropped.

puses are covered under NIBRS so the sample results in a panel of 44 college football teams.[18,19,20] The list of teams covered by NIBRS was self-compiled and Lindo et al. (2018) was cross-checked for updates.

2.3 Alcohol Sales

Data for alcohol sales at all NIBRS-covered teams was collected by first searching the college's athletic website. If nothing was found, websites of local news outlets were referenced. Seventeen agencies across 8 teams report crime in the same city as a college that changed to allowing alcohol sales to general seating.[21] Figure 1 shows the spatial distribution of colleges covered by NIBRS and color codes for which years teams changed to allowing sales, if they ever did. Teams that allowed sales or a designated beer garden for general admission fans are counted as starting alcohol sales.

2.3.1 Dates and Imbalance From Variation in Treatment Timing

The dates that are used are the length of the available college football data, 2008-2015. The data is daily and months May-July are dropped like Lindo et al. (2018), because days in these months are not during the typical college school year.[22] For teams that change in 2009, there is only 1 season before the change in the data. For teams that change in 2014, there are 6 seasons before the change. There are up to 6 seasons before and up to 7 seasons after, but only 1 pre-season observed for all changing teams and only 2 post-seasons observed for all changing teams. Data is sometimes setup using event weeks as the panel time variable to address imbalance of treatment in the balanced panel. An event-study framework is used, because Goodman-Bacon (2018) says it can be a good

[18] Table A.1 presents these teams, their number of agencies, and how many years each college had sales.

[19] Other teams that have entered D-IA, FBS, since then are omitted. The omitted teams are Old Dominion, Coastal Carolina, and University of Massachusetts-Amherst. Additionally Eastern Carolina and Miami University (OH) are supposedly covered, but offense data is missing for these two schools. None of these schools changed alcohol policy over the length of the sample. Connecticut is not included because they play many games in Rentschler Field, a professional stadium and are not in the crime data.

[20] There are 13 agencies that are identified as being connected to a university, but have no offense data in any year are omitted. After dropping these, there is still at least 1 reporting agency for all the teams that change alcohol policy over the length of the sample. That is there is at least 1 reporting agency for 44 teams, resulting in crime data for 79 individual police agencies.

[21] Data on changes in alcohol sales have been cross-checked with one other researcher's list.

[22] Lindo et al. (2018) uses more years- 1992-2012, which may not be obviously beneficial for this research question, since the research design leverages a policy change.

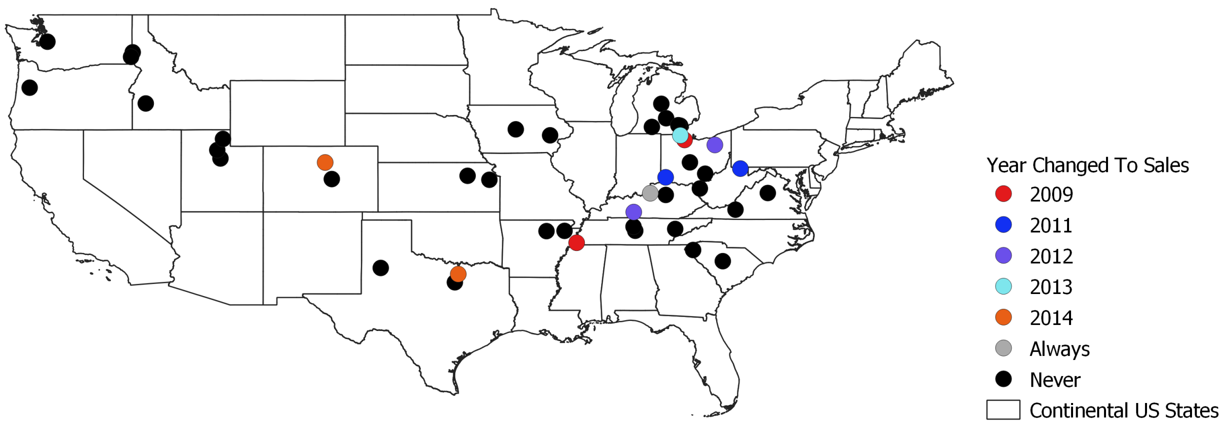


Figure 1: Years Teams in Sample Changed to Sales

Notes: Only includes D-1A colleges represented in National Incident-Based Reporting System (NIBRS) Data.

way to address variation in timing of treatment and it is a good way to check for telltale signs of endogenous policy adoption.

2.4 Descriptive Statistics

Combining these data sources results in balanced panel of agency-by-day level observations including crime counts, college football game information, and the beginning of alcohol sales. Table 1 shows descriptive statistics. Panel A shows the aggregate sample. 2 percent of the sample are home games and 14 percent of days are after sales have been legalized at games. Game And/Or Alcohol-Related Crimes stands for crimes associated with college football or alcohol and include assaults, rape, vandalism, DUI, disorderly conduct, liquor law violations, and drunkenness.[23]

Panels B-G subsample to only teams that changed their policy and split the data by pre-period/post-period and home, away, and non-game days. Comparing Panels B and C shows mean

[23]The selection of crime types is based on these types being used in Rees and Schnepel (2009) or Lindo et al. (2018).

Panel A: Aggregate						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	29.35	5	66.30	0	613	162898
Game And/Or Alcohol-Related Crimes	10.85	2	25.42	0	254	162898
Home	0.02	0	0.15	0	1	162898
Sales	0.14	0	0.35	0	1	162898
Panel B: Home, Pre-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	33.09	16	63.50	0	429	313
Game And/Or Alcohol-Related Crimes	14.95	9	25.76	0	185	313
Panel C: Home, Post-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	44.51	7	92.24	0	424	548
Game And/Or Alcohol-Related Crimes	18.61	3	39.32	0	205	548
Panel D: Away, Pre-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	28.02	9	59.62	0	453	297
Game And/Or Alcohol-Related Crimes	11.74	4	23.49	0	186	297
Panel E: Away, Post-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	41.47	5	90.09	0	420	534
Game And/Or Alcohol-Related Crimes	17.23	2	39.41	0	203	534
Panel F: Non-Game, Pre-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	25.18	5	58.64	0	613	13559
Game And/Or Alcohol-Related Crimes	9.53	2	22.62	0	214	13559
Panel G: Non-Game, Post-Sales						
	Mean	Median	Std Dev	Min	Max	Count
Total Crime	40.79	4	89.94	0	470	21758
Game And/Or Alcohol-Related Crimes	15.91	1	37.18	0	221	21758

Table 1: Summary Statistics by Period and Type of Gameday

Note: Panel A includes all game and non-gameday observations, including teams that do not change policy. Panels B-G only include teams that change their policy over the length of the sample. Home is a dummy variable = 1 if there is home game on that day. Sales is a dummy variable = 1 if it is a team that changed to sales and in the post-period. Total crime is all crime types. Game And/Or Alcohol-Related Crimes are: rape, simple assault, aggravated assault, intimidation, vandalism, driving under the influence (DUI), disorderly conduct, liquor law violations, and drunkenness.

game-related crimes increased in the post period, by about 3 crimes, and total crime increased, by about 11 crimes, for home games. In contrast, there was a decline in median crime, total and game-alcohol related, after changing to allowing sales on home game days. The reduction in median total crime is from 16 to 7, a 56% reduction, and the reduction in median game-alcohol related crime is a 67% reduction on home game days. The difference in how crime changes after allowing sales in mean and median is likely due to outliers affecting the mean more severely.

Panels D and E show there was a reduction in median crime on away games also, however, the reduction is smaller in magnitude than the reduction on home game days. Particularly, there was a 50% reduction in median game-alcohol related crimes for away games after the change (4 before, Panel D; 2 after, Panel E). After changing to allowing sales, there is also a 50% reduction in median game-alcohol related on non-gamedays. The magnitude of the reduction in crime is the exact same on away games as it is on non-gamedays. After the change to allowing sales, the reduction in median crime on away and non-gamedays is smaller than the reduction in median crime on home gamedays.[24]

2.5 Home and Away Game Premiums in Pre and Post Periods

Figure 2 presents stylized versions of parallel trends. It leverages that there are non-treated days in the post-period for treated teams. Only teams whose change sales policy are included.[25]

One particular day, away games, could be a good control group. Away games could be a desirable control group, because the sales policy does not affect days when there is no game played in that location, but away games are played at the same time of the year on the same day of the week. To leverage away games, Figure 2a keeps gamedays (home and away) and non-game days which are 2 days to either side of a home or away game.[26]

[24]In Figure A.2, two t-tests for each crime are compared for teams that change at some point over the length of the sample. The first t-test compares home and non-home games, before the change in sales. The second t-test compares home and non-home games after the change in sales. The t-tests show decreases in most types of crime, the largest decrease is in liquor law violations.

[25]This approach closely follows the scatterplot graphical evidence presented in Heaton (2012).

[26]Figure 2b uses only home and away games.

Next, the non away game day crime median (from 2 days on either side of the away game) is subtracted from the away game crime median. Also, the non home game day crime median (from 2 days on either side of the home game) is subtracted from the home game crime average. Call this variable the gameday crime premium. For example,

$$(1) \qquad Home\ Gameday\ Crime\ Premium_w = Home\ Game\ \overline{C}_w - NonHome\ Game\ \overline{C}_w$$

, where the subscript w stands for event week and $\overline{C}$ stands for "crime-median".[27] The y-axis plots the extra crimes that occurred on a gameday compared to a non gameday.

The x-axis is centered around the number of games away from the first season/game of alcohol sales. Using seasons away would not produce a useful comparison, because there is only 1 pre-period and 2 post-periods for all changing teams. The black dashed vertical line at 0 represents the change to allowing sales.[28]

Figure 2a shows the home gameday crime premium decreases from 12.1 to 5.7 after stadium sales begin. The average away gameday crime premium only slightly changes from 2.3 to 1.85 after sales are introduced. The home gameday crime premium falls and the away gameday crime premium does not, suggesting allowing alcohol sales is not correlated with an overall decrease in weekend crime. Figure 2b shows crime on home games compared to away games decreases after sales begin.

3 Method

Differences-in-differences estimates how stadium alcohol sales affect crime. The equation is

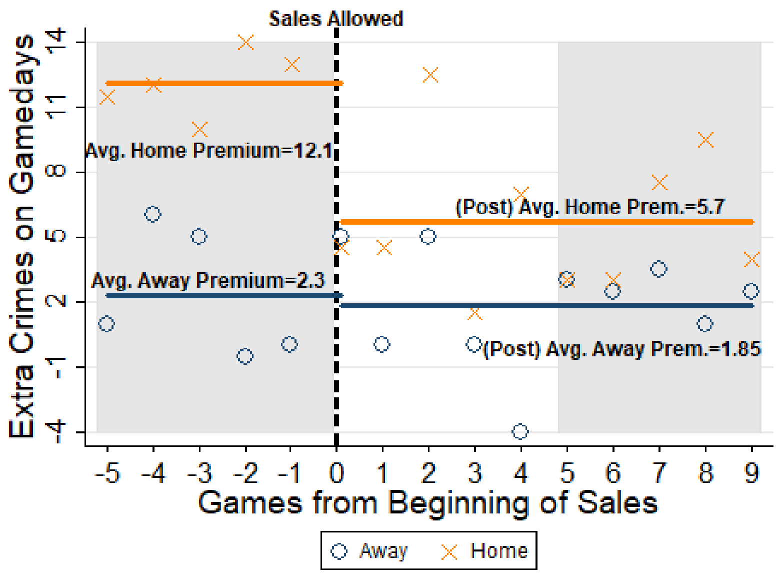
$$(2) \qquad Crime_{pcd} = exp(\beta_1 Home_{cd} + \beta_2 Sales_{cs} + \beta_3 Home * Sales_{cds} + \mu_P + \lambda_T + \phi_O),$$

in which P represents police agency, c represents colleges, s represents college football seasons, d represents days, T represents time, and O represents day of week order. Sales is a dummy variable

[27] Median is chosen over mean to reduce the impact of outliers on the results. Table 1 shows the median and mean are different suggesting outliers influence the mean as a measure of central tendency.

[28] Going from a shaded to non-shaded background (and vice-versa) in Figure 2 represents beginning a new season.

Panel A: Gameday Crime - Non-Gameday Crime



Panel B: Home Game Crime - Away Game Crime

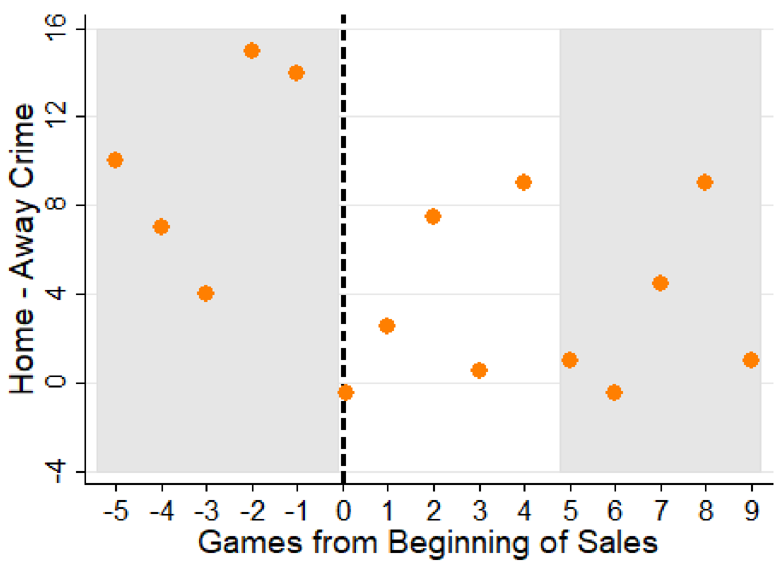


Figure 2: Gameday Crime Premiums for Home Game Versus Away Game Weeks

Note: In Panel A, there are N = 2,500 agency-date level observations. The gameday and 2 days immediately after and before. The x-axis is game-weeks away from the change to allowing sales, the first home game each season is matched to that seasons first away game for 5 games. The Home and Away series are the difference in median gameday crime between the day of the game and the two days after/before. $HomePremium_w = (HomeGame\overline{C}_w - NonHomeGame\overline{C}_{ew})$. In Panel A, the x's are for home games and o's are for away games. In Panel B, there are 1,100 gameday observations. The y-variable is $home_w - away_w$ crime Black dashed vertical line represents beginning of alcohol sales. Changing background shade indicates beginning of new season. 3 seasons of data reported, because it is balanced for all teams/agencies.

equalling 1 if it is August or after of the year the college football team began selling alcohol.[29] Home is a dummy variable equal to 1 if a college football game was played within the jurisdiction of a reporting agency.[30]

The standard errors in the preferred specification are clustered at the city level to adjust for the possibility that errors are correlated over time within groups. The dependent variable is a count variable, so Poisson models are primarily used.[31] At first, the dependent variable is total crime to limit discretion, then robustness checks subset to college-football associated crime types.

Agency fixed effects, μ_P, control for time-invariant characteristics of agencies over the length of the sample, for example climate or region. One could argue some things such as agency culture are relatively constant over the 8 year sample period. Time fixed effects, λ_T, control for aggregate variation in incidents that could potentially be related to game scheduling trends over time.

Day of week indicators, ϕ_O, are important, because college gamedays only occur on certain days, usually Saturday. Although most college games occur on Saturdays, around half of a teams schedule each season is played in another team's stadium outside of the home agencies' jurisdictions, leaving some Saturdays with no home game during the fall semester. Day of week is also correlated with crime, specifically because weekdays are typically associated with greater amounts of crime. Additionally, if universities adjust their schedule at the same time sales become legal, for instance by moving Saturday games to Thursday, then including day of week adjusts for this observable difference.

Although not shown in Equation 2, the model also includes indicator variables for holidays like Lindo et al. (2018). The holiday indicators control for changes in activities that are associated with holidays.[32] In addition to reasons in Lindo et al. (2018), colleges could be less likely to schedule games on holidays associated with drinking after alcohol sales are allowed.[33]

[29] Sales beginning is the beginning of the post period.

[30] Equation 2 applies the (Sunday) alcohol sales legalization main equation from Heaton (2012) to the college football home game crime question estimated in Rees and Schnepel (2009) and Lindo et al. (2018).

[31] Poisson estimation is favored in Heaton (2012) and Lindo et al. (2018) and has relatively favorable theoretical results compared to negative binomial models. (Allison & Waterman, 2002; Cameron & Trivedi, 2005; Wooldridge, 1999). In particular, Cameron and Trivedi (2005) shows that cluster-robust standard errors in Poisson models correct for over-dispersion which defeats the main purpose of including an overdispersion parameter (i.e. using negative binomial models.) Results are not qualitatively different when using of negative binomial or OLS.

[32] The holiday controls included are: Columbus Day, Halloween, Labor Day, Thanksgiving, and Veterans' Day.

[33] Lindo et al. (2018) notes a "direct" bias from the association between holidays and the days which games are played on. Another way is through an "indirect" bias through the day of week fixed effects, because certain holidays always fall on certain days of the week.

3.1 From Alcohol Sales to Consumption and Alcohol Consumption to Crime

A useful theoretical model for considering stadium alcohol access and alcohol consumption is a three-period utility maximization problem, in which the periods are pre-game, game, and post-game (Boyes & Faith, 1993). Goldstein (1985) describes 3 mechanisms from alcohol use to crime. Mechanisms are the direct pharmacological effect, the excuse mechanism, and social interaction.[34] ,[35]

3.2 Selecting a Control Group

Equation 2 estimates a treatment effect of alcohol sales by comparing home games to non-home games in the same agency, before and after the change. The approach uses home game days, where a college played a game in the jurisdiction of an agency in the same city as a college in a given day, of teams that changed to allowing alcohol sales as a treated group. This design takes advantage of days where no home game was played, meaning no college football game at all or the team was playing but not in the same city or stadium, as a control group.[36]

In addition to the home/treatment day versus non-treatment days used by previous papers (Heaton, 2012; Lindo et al., 2018; Rees & Schnepel, 2009), this strategy is appropriate because teams that do not change may be systematically different than teams that do change.[37] For example, teams that change are mostly geographically similar by being from the midwest (see Figure 1) and they are not from the most competitive conference (the SEC) due to the SEC banning alcohol sales over the sample period. These reasons make non-changing teams a less attractive counterfactual, using non-home game days is not vulnerable to these criticisms. Conditional on away games

[34]The first mechanism is a direct pharmacological affect, where increased blood alcohol content (BAC) reduces the cognitive function of individuals which can lead to poor decisions. The second mechanism is the excuse effect, where an individual believes being intoxicated will lead to less harsh sentencing if convicted. The third mechanism is the social interaction effect, which is essentially that alcohol use is often positively correlated with social activity, so increasing alcohol use also increases the number of social interactions one has which independently could lead to additional crime. It is empirically challenging to differentiate which channel alcohol use affects outcomes through (Carpenter & Dobkin, 2010).

[35]The effect of alcohol consumption on a wide variety outcomes have been examined in the literature (Chaloupka, Grossman, Saffer, et al., 2002).

[36]In contrast to typical DD applications, one can drop all teams that did not change and still estimate Equation 2 due to the presence of the untreated non-home games. Leveraging non-treated days of treated teams means non-changing teams can be completely removed and there is still identifying variation.

[37]If changing and non-changing teams are dramatically different, then teams that do not change would be an undesirable counterfactual, because it would not be clear that the policy change is driving the estimated effect.

not changing after sales, away games could make an attractive control group, because it is on a similar day at the same time of the year.

3.3 Identification

The identification assumption is that in the absence of the change to alcohol sales, the home gamedays and non-home game days would not diverge from their common crime trends. The high variance, relative to the mean, of crime data makes this difficult to observe in typical DD parallel trends. It is reasonable to believe that changing to sales was done with the main purpose of raising additional revenue, not in response to home game crime trends.

Equation 2 omits several variables which may be related to crime. Some variables correlated with crime are unemployment and number of law enforcement officers deployed. The reason unemployment and number of law enforcement officers are omitted is because daily data does not exist at the agency level.

The DD coefficient, β_3 from Equation 2, is unlikely to be biased by omitting daily unemployment. For excluding unemployment to bias the parsimonious DD coefficient, unemployment would have to change at the same time as the policy. Furthermore, because the control group is non home game days, the change in unemployment would have to affect home game days differently from all other days. Law enforcement is more likely to bias the parsimonious DD coefficient, because it conceivably changes only on home gamedays.[38]

4 Results

First, a general difference-in-differences model is estimated, followed by models establishing robustness by using specific sets of crime in the dependent variable, falsification of an effect on away games, using away games as the control group, and heterogeneous effects by university agency. Second, randomization inference suggests statistical significance is not due to few treated (8) and overall number of clusters (44). Third, event-study coefficients are not consistent with endogenous policy adoption. Fourth, the sensitivity of the results to omitting attendance and enforcement from

[38]Enforcement being time-varying, correlated, omitted variable leads to the necessity of leveraging age-based variation after the parsimonious DD.

parsimonious Equation 2 is examined. Section 5 proposes a research design to leverage age based variation for a different control group feasibly less vulnerable to potential omitted variables.

4.1 Increasingly Saturated Fixed Effects

Table 2, Panel A presents the main results of estimating Equation 2 using models increasingly saturated with fixed effects. In each column, the estimated effect of home games is positive and the estimated effect of allowing sales at home games is to reduce the total amount of crime on home games relative to non-home games. Both the coefficients on home and home X sales are statistically different from 0 at greater than 95% confidence in all specifications, although they have opposite signs. When the day of week and holiday indicators are added (column 4), home and home X sales coefficients have the exact same magnitude in opposite directions.[39] The negative sign suggests allowing alcohol sales reduces overall crime, which departs from most of the general results on alcohol access and crime (Carpenter & Dobkin, 2015; Heaton, 2012), but is similar to Boyes and Faith (1993). Four competing hypotheses for the un-anticipated, negative sign exist: substitution across time and/or types of liquor, time-varying omitted variables which bias the coefficient estimate, large increases in crime in the pre-period which the policy was designed to address, or poor sample/dependent variable choice. Further analyses investigate the latter three, seeking to rule them out leaving the first reason as the only explanation.[40]

4.2 Specific Crime Types

Panel A of Table 2 uses total crime; however, unrelated crime types could reduce precision and add bias to the estimates. Another potential issue is liquor law violations are no longer possible, for old enough individuals, during the game.[41] Panel B of Table 2 ascertains whether the unexpected negative sign is due to unrelated crime types being in the total crime dependent variable.

[39] The coefficient magnitude is calculated: $(e^{0.19} - 1) * 100 \approx 20$ percent.

[40] Table A.3 shows that the DD coefficient is negative and statistically significant and also when using OLS or Negative Binomial models. This suggests the results are also not driven by type of regression model used. Results are not affected by changing to clustering by agency.

[41] If crime types besides liquor laws do not decrease also, then it would be indicative that the mechanical relationship is driving the entire effect on crime.

Panel A: Various FE's	(1)	(2)	(3)	(4)	(5)
Home	0.31***	0.28***	0.24***	0.19***	0.19***
	(0.08)	(0.08)	(0.08)	(0.06)	(0.06)
HomeXSales	-0.21**	-0.19**	-0.19**	-0.19**	-0.19**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Observations	162898	162898	162898	162898	162898
Number of Agencies	79	79	79	79	79
Agency FE	-	X	X	X	X
Years	-	-	X	X	X
Months	-	-	X	X	X
Week of Yr	-	-	-	-	X
Day of Week	-	-	-	X	X
Holidays	-	-	-	X	X

Panel B: Alternative Dependent Variables	Game-Associated Crime			Alcohol Crimes	
Home	0.12***	0.20***	0.35***	0.65***	1.18***
	(0.04)	(0.06)	(0.10)	(0.16)	(0.17)
HomeXSales	-0.11**	-0.24***	-0.39***	-0.63***	-0.49*
	(0.06)	(0.09)	(0.13)	(0.23)	(0.29)
Observations	162898	162898	162898	162898	162898
Number of Agencies	79	79	79	79	79
Drop Liquor Law Viol.	X	X	-	-	X
Game-Crime Subset	-	X	X	-	-
Alc. Suspected	-	-	-	X	X
Crimes Within 3 Hours of Game	-	-	-	-	X

Panel C: Alternative Subsamples/Control Groups	Falsification	Gamedays		Agency Type	
Away	-0.04**				
	(0.02)				
AwayXSales	-0.02				
	(0.03)				
Home		0.28***	0.45***	1.29***	0.09**
		(0.09)	(0.12)	(0.14)	(0.04)
HomeXSales		-0.24**	-0.38***	-0.90***	-0.10*
		(0.09)	(0.13)	(0.26)	(0.06)
Observations	162898	7442	7442	65984	96914
Number of Agencies	79	79	79	32	47
Gamedays Only	-	X	X	-	-
Drop Liquor Law Viol.	-	X	-	-	-
University Agencies Only	-	-	-	X	-
City Agencies Only	-	-	-	-	X

Table 2: Alcohol Sales Legalizations Effect on Total Crime at Home Games

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors cluster-robust at the city-level in parentheses. All models estimated using Poisson conditional fixed effects models. In Panel B and C, all models include agency, year, and month fixed effects, day of week indicators, and holiday indicators. Holidays are: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Panel B, column 1 drops liquor law violations. Panel B, column 2 includes only crimes in the following types: assault, rape, vandalism, drunkenness, dui. Panel B, column 3 adds liquor law violations back. Panel B, column 4 only includes crimes where alcohol was suspected For Type A crimes, this means an alcohol suspected indicator is present; for Type B crimes alcohol must be in the name (liquor law violation, drunkenness, or DUI) of being used by the offender. Panel B, column 5 only includes crimes where alcohol was suspected and the crime occurred within 3 hours of the game beginning or ending. Panel C columns 2 and 3 include only home and away games. Panel C, column 4 is only university agencies. Panel C, column 5 is only city/county agencies.

4.2.1 Game-Associated Crimes

Panel B, Table 2 column 1 drops all liquor law violations from the total dependent variable in Panel A. The effect remains statistically significant above 95% confidence, but drops in magnitude by nearly half. Column 2 only uses game-associated crimes, which have been used in studies by Rees and Schnepel (2009) and Lindo et al. (2018). The effect returns to a more similar magnitude as Panel A and is statistically significant above 99%, suggesting there may be some bias and imprecision from using unrelated crimes. Column 3 adds liquor law violations to the crime types used in Column 2 and the effect size becomes larger. While liquor laws drive some of the effect, crime types associated with high alcohol consumption are also observed to decrease with access to in-stadium alcohol. The negative effect of alcohol sales on crime does not change to positive by using the most game-related crime types.

4.2.2 Alcohol Crimes

Table 2, Panel B, columns 4 and 5 makes use of alcohol-related crimes, because stadium alcohol sales presumably would affect crimes in which alcohol was suspected of being used.[42] Panel B, column 4 uses only alcohol-related crimes. The magnitude of the coefficient is much larger and statistically significant above 99% confidence. This magnitude increase is in line with expectations that alcohol sales should affect alcohol-suspected crimes. Panel B, column 5 uses alcohol-related crimes within 3 hours of the game beginning or ending. This time window is important, because if the gametime alcohol policy is driving the results, alcohol crime occurring around the same time should be affected most noticeably to be consistent with a substitution type explanation for the unexpected negative sign. Stadium sales reduce alcohol suspected crimes near game time.

4.3 Alternative Control Groups

Panel C of Table 2 aims to detect any potential bias of away games as a control group after there is no effect of home stadium sales on away game day crime. Away games are an appealing control group, because they occur on the same days and are during the same season of the year as home

[42] Alcohol-related crimes are those where alcohol was suspected of being used, as noted by the NIBRS dataset (for Type A crimes), or the crime type was alcohol-related if it is Type B. Type B alcohol-related crimes are DUI, drunkenness, and liquor law violations.

games, while the estimates above also include spring days and non-gamedays and no dummy variable for away games. Furthermore, no effect should be found on away gamedays, because the stadium sales only change on home gamedays.

4.3.1 Falsification On Away

Table 2, Panel C, column 1 shows no effect of home sales on away game crime. No effect is evidence against unrelated time trends which may happen to be correlated with the change to alcohol sales. No effect also suggests away games are a viable control group, since they will not introduce bias.

4.3.2 Away Games as Control Group

Table 2, Panel C, Columns 2 and 3 changes the sample to only away games, because these are similar days of the week as home games at the same time of year. This also rules out the results being driven by variation in crime on non-home gameday which take place during the Spring semester when there is no college football. The DD point estimates in Panel C, columns 2 and 3, are exactly the same as columns 2 and 3 of Panel B. Regardless of control group, the effects of alcohol sales on crime is negative, statistically significant, and of similar magnitude.

4.3.3 Agency Types

Table 2, Panel C, Columns 4 and 5 the sample is split into university and city agencies. Splitting the sample by agency type is intuitive, because the policy change is for university policy. Given this, one would expect a larger effect for university police agencies. Furthermore, it could be that there is some type of geographic displacement of crime. If city/county and university police had differently signed responses of crime to stadium alcohol sales, then it would be preliminary evidence of geographic displacement.[43] The same-signed findings by agency type are inconsistent with the unexpected negative effect being due to crime displacement from city to university agencies. Ad-

[43] Related way to look at location is by only using crimes at certain places. Table A.4 subsamples to locations that are related and finds effects in locations which are related to college football.

ditionally, the magnitude is greater for university agencies, consistent with stadium alcohol sales being plausibly more related to university agency crime.[44]

4.4 Ruling Out Inappropriately Narrow Confidence Intervals

The standard errors are clustered by college, which accounts for within college correlation of the standard errors; but there are two reasons to be suspicious that the DD estimates have misleadingly narrow standard errors. The issues are there are only 44 colleges by which to cluster and coefficient is estimated from only 9 teams changing. These issues mean confidence intervals could be inappropriately narrow (Cameron & Miller, 2015). To address the potentially narrow standard errors, a permutation test is used which is less vulnerable to erroneous inference resulting from few clusters (Fisher, 1935).[45] This procedure is based on Fisher's exact p-value.

For the procedure, any number of teams are randomly assigned a year of beginning sales.[46] The test statistics from each of the 1,000 permutations are displayed in a histogram in Figure **??**. The actual test statistic of the coefficient on HomeXSales is outside the 1st percentile (The vertical green line with dots) of the placebo distribution of test statistics. The number of times one of the z-statistics on β_3 from Equation 2 from falsely randomly assigning treatment was less than the actual z-statistic of home X sales coefficient from Equation 2 with realistically assigned treatment was only 8/1000 repititions, which implies an exact Fisher p-value of 8/1000 = .008. No evidence is found that strong statistical significance is solely due to few overall clusters or few treated clusters.

4.5 Ruling Out Pre-Policy Changes in Crime Outcomes

An event-study framework is adopted to provide evidence related to endogenous adoption of alcohol sales. The objective is comparing home game crime changes in the post-period to home game crime in the pre-period. The equation used is

$$(3) \qquad Home\ Crime_{pw} - Away\ Crime_{pw} = \sum_{w=-30}^{-2} \delta_w Week_w + \sum_{w=0}^{34} \delta_w Week_w + \mu_p + e_{pw},$$

[44] Some stadiums, such as Memphis, are professional stadiums so the university has less influence on alcohol-sales.

[45] This is a similar approach to deal with narrow confidence intervals from few clusters as Cunningham and Shah (2018).

[46] Specifically, all teams are randomly assigned season/year for beginning sales. The date is August 1, random year. Whether random assignment of any number of teams is appropriate depends on the true data generating process.

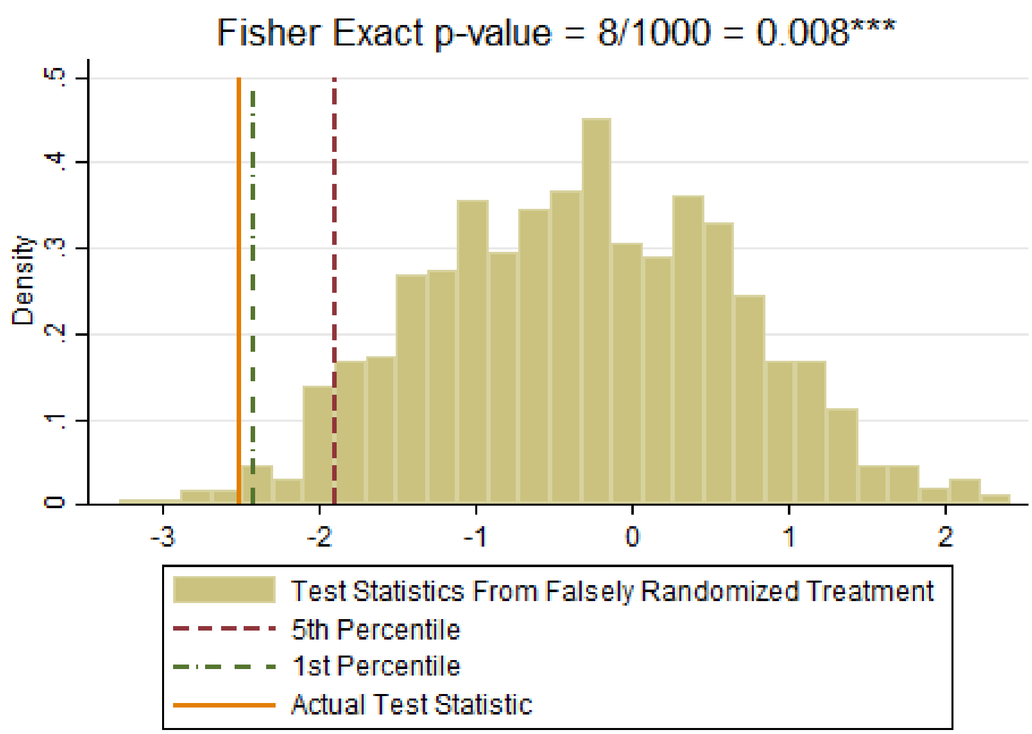


Figure 3: Randomization Inference, Distribution of Z-Statistics From Home X Sales From Equation 2 Using Randomized Treatment Across Seasons and All Teams in Sample

Notes: Histogram displays the Z-statistic on the Home X Sales coefficient from Equation 2, specifically the coefficient, β_3 from Equation 2, with the same fixed effects as column 4 of Table 2. No restrictions are imposed on the number of teams who receive treatment (i.e. Bernuolli sampling.) The treatment is randomized across time. The actual z-statistic is only greater than 8/1000 random samples, implying an exact Fisher p-value of 8/1000 = 0.008.

in which *w* stands for event week and all other subscripts are similar. Only gamedays and changing teams are included in the estimation sample. The $Week_w$'s are binary variables equal to 1 if it is a specific event week and δ_w is the associated coefficient estimate.[47] As is standard, the event week of -1 games from the allowance of sales is omitted so coefficients compare crime in other event weeks to this period. The dependent variable $Crime_{act}$ is adjusted for away games by subtracting the away game crime to leverage non-treated days in treated groups .[48,49] It is preferred to include fixed effects for agencies, because it is possible for teams to drop out of the sample if the agency stops reporting data.[50]

Figure 4 presents coefficients from each period with heteroskedasticity-robust 95 percent confidence intervals.[51] There are no significant effects ($p<0.05$) of sales on crime before sales are allowed. The joint F-statistic that all pre-period coefficients are 0 is 1.14 (p=0.339), meaning the null hypothesis of no pre-trends cannot be rejected. The event study approach finds no evidence endogenous policy adoption is responsible for the negative effect of alcohol sales on crime.

4.6 Investigating Omitted Variable Bias

4.6.1 Attendance

Table 3, Panel A includes home game attendance in the regression. Attendance could be correlated with sales, because more people might come to games if alcohol is available. Attendance could also affect crime through a social interaction effect (Carpenter & Dobkin, 2010; Goldstein, 1985). By being related to sales and crime and being omitted, attendance is a possible confounder would could causes the unexpected negative effect. Columns (1) and (2) assume non-home game attendance is 0, which creates artificial positive correlation of attendance with home games.

[47] As discussed in Section 2.3.1, the balanced panel and variation in treatment timing lead to uneven temporal coverage. All lags and leads are included, but there is only 1 pre-season and 2 post-seasons where all colleges are included, so these coefficients are presented. A different approach is pooling all years outside of these years. Some discussion of pros and cons of pooling data in event studies can be found in McCrary (2007).

[48] This also necessitates using OLS, because there can be negatively-valued observations of the dependent variable.

[49] Furthermore, there are not many observations, so using away game crime increases the amount of data, potentially offering efficiency gains. The drawback is a trend in away game crime could bias the estimate, this is not concerning given the insignificant effect the change has on away game crime (shown in Table 2, Panel C, column 1).

[50] Only teams that change to allowing stadium sales are kept, which facilitates construction of the appropriate comparison group, reduces variables in the model, and eases interpretation. There are only a few teams who change the policy meaning that dropping teams leaves less statistical power in the model. Due to lack of power, month, year, and sales variables are not included. Including fixed effects leaves too little variation to yield any precise results.

[51] City-clustered standard errors produce similar results, despite there are being only 8 city clusters.

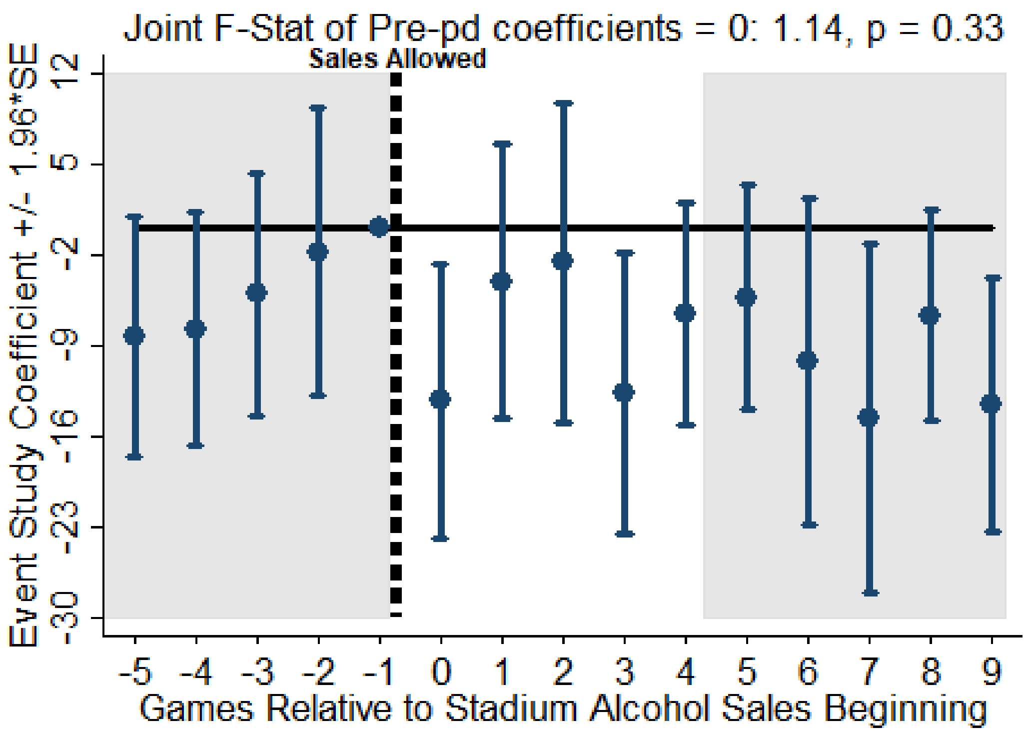


Figure 4: Time-Varying Treatment Effects of Sales on Home Game Crime Compared to Away Game Crime

Notes: Coefficients are from the event study in Equation 3. Dependent variable is home crime - away crime. N = 551 event week-agency observations. Only teams that change included. Only gamedays included. Heteroskedasticity-robust standard errors used. Alternating between gray and white background shows the beginning of a new season.

In column 1, attendance is included as a covariate in the parsimonious DD after dividing by ten thousand to facilitate interpretability. The home and the DD coefficient are much closer to 0 and statistically insignificant. In column 2, attendance is transformed to a measure of attendance deviation from the teams' sample-length attendance average. Specifically,

$$HomeAttendanceDeviation_{cg} = HomeAttendance_{cg} - \overline{HomeAttendance}_{c},$$

where c stands for college and g game. This is done, because it makes home games with average attendance be equal to 0 attendance, like non-home games.[52,53]

When using attendance deviations, the estimated DD coefficient regains statistical significance above 95 percent confidence (Panel A, column 2). The magnitude of the DD coefficient is similar to estimates reported in Table 2. Panel B repeats the two columns of Panel A using the gameday/alcohol-related crimes that have been used earlier. In both columns the DD coefficient is statistically significant above 95 percent confidence and have reasonable magnitudes. Panels A and B show omitted home game attendance is not the reason for the unexpected negative sign of allowing alcohol sales on crime.

4.6.2 Law Enforcement Differences

Table 3, Panels C and D investigate the effects of the change to sales on law enforcement strategy. Law enforcement strategy is almost certainly related to the amount of recorded crime. Also, there are several examples of statements made about law enforcement changing, on home gamedays only, after the change to sales (e.g. (Penn Live, 2011)). It is less straightforward to sign the bias for law enforcement, because law enforcement could deploy more or less officers after the change and more officers could lead to more crime (Levitt, 1995) or less crime (MacDonald, Klick, & Grunwald, 2016).

Controlling for law enforcement strategy is also not straightforward, because the daily count of deployed officers is not observed.[54] Prior studies using NIBRS use clearance rates as the best daily,

[52] Attendance deviation is also divided by 10000 for easier interpretation.

[53] To my knowledge, no estimates exist in a causal framework which can claim to include a measure of social interaction in research studying the effect of variation in alcohol availability. This weakness to identify mechanisms is noted by Carpenter and Dobkin (2010).

[54] Strictness of enforcement, areas, or times of deployment also likely matter, but are less likely to be observed.

Panel A: Dependent Variable: Total Crime	(1)	(2)
Home	-0.01	0.19***
	(0.14)	(0.06)
HomeXSales	-0.08	-0.19**
	(0.08)	(0.08)
Home Attendance (10000s)	0.04	
	(0.03)	
Home Attend. Deviation (10000s)		0.03
		(0.04)
Observations	162898	162898

Panel B: Dependent Variable: Game-Alcohol Related Crimes	(1)	(2)
Home	0.13	0.35***
	(0.22)	(0.10)
HomeXSales	-0.27**	-0.39***
	(0.13)	(0.14)
Home Attendance (10000s)	0.04	
	(0.05)	
Home Attend. Deviation (10000s)		0.08
		(0.05)
Observations	162898	162898

Panel C: Clearance Rate on Home Games	(1)	(2)
Home	0.09***	0.10***
	(0.02)	(0.02)
HomeXSales	-0.09***	-0.08**
	(0.03)	(0.03)
Observations	131841	162898
OLS	X	X
Drop Missing Ratios	X	-

Panel D: Clearance Rates on Away Games	(1)	(2)
Away	-0.01*	-0.02***
	(0.01)	(0.01)
AwayXSales	-0.03	-0.02
	(0.03)	(0.02)
Observations	131841	162898
OLS	X	X
Drop Missing Ratios	X	-

Table 3: Investigating the Influence of Omitted Attendance and Law Enforcement on Effect of Sales on Crime

Note: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Standard errors cluster-robust at the city-level in parentheses. All models include agency, year, month fixed effects. All models use indicator variables for day of week and holidays including: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Panels A and B are estimated using Poisson conditional fixed effects models. In Panels A and B, column 1 includes attendance in 10,000's. Non-home game attendance is assumed to be 0. In Panels A and B, column 2 home game attendance is subtracted from the average within-sample attendance. In Panel B, the dependent variable is game/alcohol-related crime, including: assault, rape, vandalism, drunkenness, DUI, and liquor law violations. Panels C and D are estimated using OLS. Panel C and D use clearance rates as the dependent variable to measure law enforcement. Clearance rate $= \frac{Number\ of\ Reports}{Number\ of\ Arrests}$. Panels C and D, column 1 does not include observations where there are 0 arrests or 0 reports. Panels C and D, column 2 assumes clearance rate = 0 if there are no arrests.

observable measure of law enforcement strategy (Heaton, 2012; Lindo et al., 2018), because Mas (2006) finds that clearance rates are positively correlated with deployment numbers.[55] Clearance rate is used as the dependent variable, in prior studies, to see if the policy affects the likelihood of being arrested, which would be consistent with changes in enforcement.[56] If the DD coefficient does not change the arrest rate on home games after the change to sales, compared to non-home games, the data is not consistent with estimated effects being driven by changes in enforcement.

Table 3, Panels C and D estimate Equation 2 using clearance rate as the dependent variable.[57,58] Panel C shows that, regardless of how one treats the missing values (where there are no reports), the effect of the DD coefficient is to reduce the clearance rate on home games compared to non-home games. This is a problem, because a reduction in clearance rates implies lower enforcement intensity, which could be responsible for alcohol access unexpectedly reducing crime.[59] Panel D shows clearance rates do not change on away games, meaning it is not some uncorrelated weekend change in enforcement driving Panel C.

5 Using Age-Based Variation to Address Omitted Variables

Table 3 suggests omitting enforcement may bias DD estimates and be responsible for the unexpected negative sign. To address likely omitted variable bias from time-variant, law enforcement, Section 5 uses age-based variation in legal access to in-stadium alcohol, that also varies over time (see Figure A.1). This change in policy only affecting a specific age is similar to Lemieux and Milligan (2008), where individuals of a certain age gain access to benefits after a point in time. Those exceeding the minimum legal drinking age were allowed to purchase alcohol and exposed to the same time-varying confounders as those who did not gain access.[60] Age-based variation

[55] Clearance rate is the number of arrests per report ($\frac{number\ of\ arrests}{number\ of\ reports}$). More deployed officers lead to higher clearance rates, because more officers means officers can respond faster leading to the likelihood that a report generates an arrest.

[56] Prior studies (Heaton, 2012; Lindo et al., 2018) interpret the lack of an effect of the policies under consideration on the clearance rate as evidence that omitting law enforcement variables does not bias the coefficients of interest.

[57] The panels no longer use Poisson models, because the dependent variable, clearance rate, is not a discrete variable.

[58] Results are not different when using models that explicitly account for the dependent variable being bounded by 0 below and 1 above, such as fractional logit or zero-inflated binomial models. These results are available upon request.

[59] It also suggests that the null results of Lindo et al. (2018) and Heaton (2012) are meaningful, because it rejects that there is no change in enforcement when there is anecdotal evidence that this may be the case.

[60] Age has been used as a heterogeneity analysis before (Marcus & Siedler, 2015).

addresses bias from omitting enforcement if enforcement changes equally affect both underage and legally aged individuals, by modifying the treated and control groups according to exogenous federal law.[61]

5.1 Age-Based Variation Mitigates Omitted Enforcement Bias

First, whether clearance rates are continuous at the minimum legal drinking age cutoff is examined in the spirit of a regression discontinuity design. Any discontinuous changes in the conditional mean of the clearance rate at the minimum age cutoff age would mean using age based variation does not mitigate the impact of omitting law enforcement causing the treatment effect to be biased into an unexpected, negative sign.[62,63] Figure 5 tests the continuity of the home game clearance rate at the age cutoff, before and after sales are allowed.[64] There is not a visible discontinuous increase in clearance rate at the age cutoff. If anything the higher clearance rate of underage individuals converges downwards towards legally aged clearance rates after sales, which suggests omitting enforcement will reduce the likelihood of finding less crimes for overage people (i.e. being consistent with the results of Section 4).[65] No different effects of sales on clearance rates at the cutoff supports leveraging age-based variation to adjust for omitted enforcement bias.

[61] Heaton (2012) uses counties that do not change as a triple difference. In contrast with Heaton (2012), at least some unobservable variables which could bias the key DD estimand, due their correlation with the treatment (sales) and outcome (crime) and subsequent omission from the parsimonious specification, are not well controlled for by using unaffected counties/teams. Using non-treated counties as a triple difference theoretically addresses omitted variables such as TV advertisements in Heaton (2012) because it is entirely plausible that TV advertisements are aired for whole regions. The same strategy does not work for stadium alcohol policy, because omitted variables like enforcement only change for treated college teams on home games.

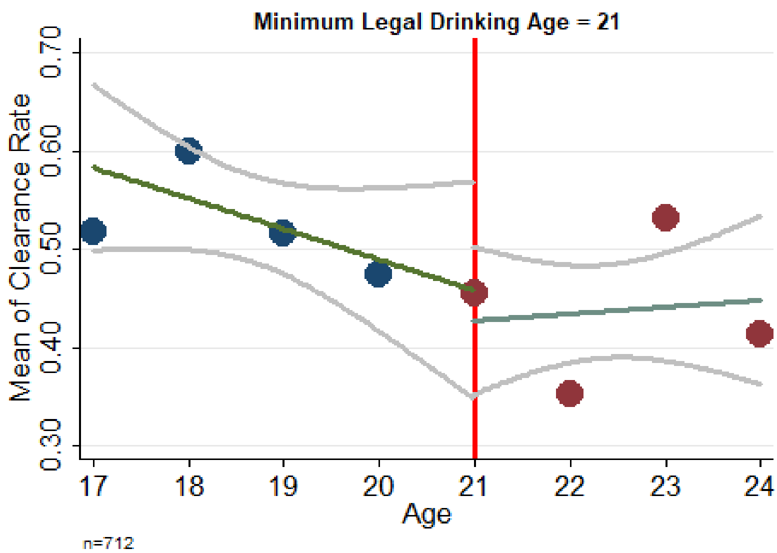
[62] The issues of large, discrete jumps in running variables for regression discontinuity designs are examined by Lee and Card (2008) and Kolesár and Rothe (2018). Essentially the standard errors can be misleadingly narrow, because one has to still assume a functional form for the difference between the last ages to the cutoff, i.e. from age 20 to age (21 - ϵ)

[63] The test may have low power due to not having many observations and not having observations closer than "1 year" away from the cutoff on either side. This is due to NIBRS only recording age of offender to the year.

[64] Clearance rate should be $\in$ (0,1), however Figure A.3 shows that there are some data points that do not conform to these expectations. They are a relatively low number.

[65] Figures A.4a and A.4b show that there is not a noticeable change at the age cutoff of at least 1 report being filed on gameday.

Panel A: Pre-Sales Home Gameday Clearance Rates



Panel B: Post-Sales Home Gameday Clearance Rates

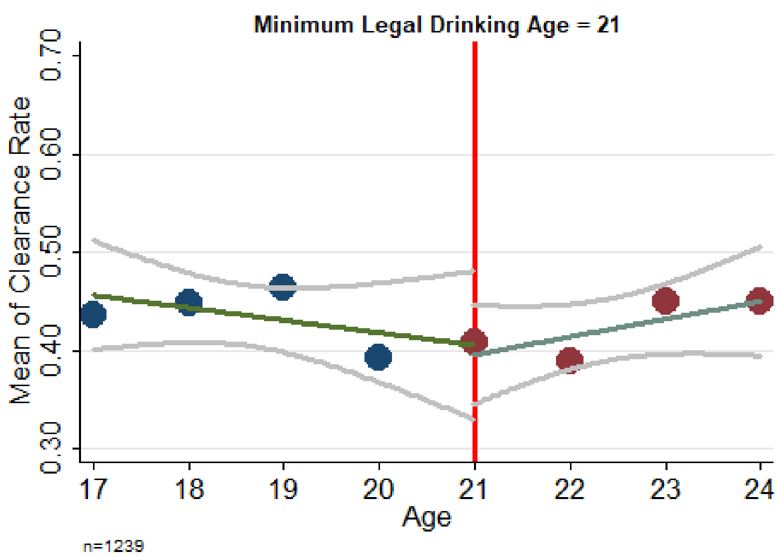


Figure 5: Examining If Age-Based Variation Feasibly Controls for Enforcement Bias

Note: Panel A and B show the likelihood of being arrested, conditional on a report being filed by age. The red line vertical at age 21 represents the minimum legal drinking age cutoff. The likelihood of being arrested, clearance rate, has been used to proxy for enforcement intensity in prior literature (Heaton, 2012; Lindo et al., 2018).

5.2 Home Game Crime Premiums For Legal and Underaged Individuals

Figures A.4a and A.4b show a discontinuity in number of observed clearance rates at age 17, probably due to indivudals becoming a legal adult at age 18. In order to have equal bandwidths and exclude this discontinuity in observations at age, a bandwidth of 3 years on either side of the cutoff is chosen. Figure 6 shows home game crime premiums (compared to nearby non-gamedays) for 18-20 year olds compared to 21-23 year olds. There is no change in the underage crime premium after the change to sales, but there is a decrease in the overage crime premium after the change. The 1.5 median for 21-23 year olds is driven by 3/5 observations being at 1.5 or above. In the post period, there are only 2/10 observations that are at least 1.5.

5.3 Age-Based Variation Regression Methods

To leverage age-based variation, two separate, related regression approaches are used. First, triple differences uses legal age as a third difference. Second, a fixed effects RD estimator, mimicking Pettersson-Lidbom (2012), is used. Both methods have different strengths and weaknesses.

5.3.1 Triple Difference

The triple difference (TD) model uses the same home and sales binary variables as Equation 2; the binary variable for creating the third difference is whether the offender was of legal age (age $\geq$ 21) to buy/consume alcohol. Triple difference models follow

$$\begin{aligned} Crime_{pctl} = exp(&\beta_1 Home_{ct} + \beta_2 Sales_{ct} + \beta_3 Home * Sales_{ct} + \beta_4 Legal_l \\ &+\beta_5 Home * Legal + \beta_6 Sales * Legal + \beta_7 Home * Sales * Legal + \mu_p), \end{aligned} \tag{4}$$

in which β_7 is the coefficient of interest. β_7 estimates the causal effect of sales as the change in crime for legally aged individuals compared to non-legally aged individuals, on home and non-home games, before and after the change to sales. For observations where crimes counted were committed by those who were at least 21, the $Legal_l$ dummy variable is equal to 1.[66,67]

[66] Note this means the number of observations doubles as there are now 2 observations per day-agency combination.

[67] If one is concerned that individuals below the cutoff may be obtaining alcohol in the stadium of the cutoff through friends or false identification, then the 21 age cutoff can thought of as a fuzzy cutoff. Those above the age of 21 are discontinuously more likely to consume legal alcohol in the stadium than those under 21 after the allowance of legal alcohol.

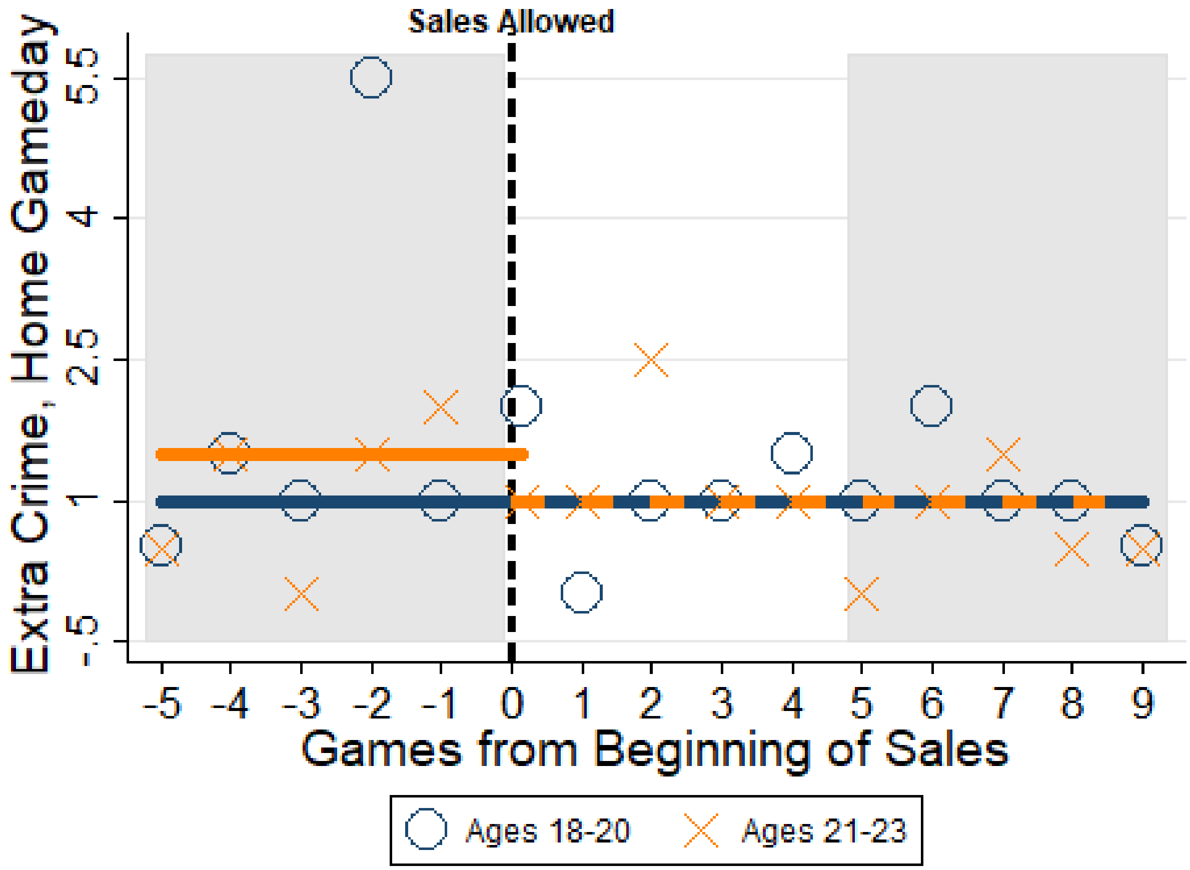


Figure 6: Gameday Home Crime Commission Premiums for Legally Aged Versus Underage

Notes: N = 4,200 agency-date-legally aged level observations. Inidividuals aged ≥ 21 can legally purchase and consume alcohol in the United States. The > 20 and < 21 points are the difference in median gameday crime between the home game and the two days after/before, $HomePremium_{ew} = (HomeGame\overline{C}_{ew} - NonHomeGame\overline{C}_{ew})$. The two series shown are medians of these points. Black dashed vertical line represents beginning of alcohol sales. Changing background shade indicates beginning of new season.

The benefit to triple differences is it essentially pools the data for ages right above and below the cutoff. This helps deal with a sparse dependent variable. A drawback to differencing is that there are many periods and variation in the timing of treatment which leads to unknown weights being placed on different comparisons (Goodman-Bacon, 2018). Using a third difference likely increases the number of comparisons and makes it hard to discern which variation drives the results.[68]

5.3.2 Fixed Effects RD Estimator (Pettersson-Lidbom, 2012)

An alternative way to leverage age-based variation is an age discontinuity approach with a temporal dimension. Several such estimators exist including: first-differenced RD (Lemieux & Milligan, 2008), fixed effects RD (Pettersson-Lidbom, 2012), dynamic RD (Cellini, Ferreira, & Rothstein, 2010), and a difference in discontinuity approach (Grembi, Nannicini, & Troiano, 2016).[69] The data at hand is most appropriate with a fixed effects RD approach, so that is used.[70] The estimating equation follows

$$Home\ Crime_{pwa} = \mu_p + \lambda_w + \beta Stadium\ Alcohol_{csa} + f(Age) + u_{pwa}, \tag{5}$$

in which *p* stands for police agency, *w* for event week, *s* for football season, and *a* for age.[71] μ is an agency fixed effect and λ is an event week fixed effect. Stadium Alcohol is a dummy equalling 1 if the age is 21 or over and it is on or after the team switched to selling alcohol (if event week $\geq$ 0), making β the estimated treatment effect for being right above the age cutoff in the post period. $f(Age)$ is a linear function of age.

A strength of Equation 5 is that the identifying variation is all from the age cutoff. There is no way that β is being driven by confounding non-home gameday variation.[72] A weakness is formatting the data this way leads to many 0's in the dependent variable. Away games can be used

[68] For instance, is it the underage, non-home game individuals whose crime increased dramatically? Figure 6 suggests no, but it is possible.

[69] This is the overview given in (Grembi et al., 2016, pg. 10).

[70] A first difference RD estimator (Lemieux & Milligan, 2008) does not make sense, because individuals are not observed once before and after the change. A dynamic RD is used to study the same cross-sectional unit with multiple opportunities to adopt a school bond tax increase (Cellini et al., 2010), there are not multiple opportunities for treatment to occur in this environment. A difference in discontinuity uses two cross-sections of individuals (Grembi et al., 2016), while this data is a panel and the intra-group correlations can be accounted for with fixed effects.

[71] This just an adaptation of Pettersson-Lidbom (2012) to the current empirical context.

[72] One additional advantage is the there is no variance in group size this way, meaning that alternative differences in differences robustness may be redundant (Ferman & Pinto, 2019).

to take advantage of more data, deal with some of the zeros, and difference out common trends. Figure 6 and Table 2, Panel C, column 1 suggests away game crime is not changing, so using away games as a counterfactual does not add bias by adding a confounding trend. To leverage away games, some models use the difference between home game and away game crime, in the same event week, as the dependent variable

(6) $Home\ Crime_{pwa} - Away\ Crime_{pwa} = \mu_p + \lambda_w + \beta Stadium\ Alcohol_{cwa} + f(Age) + u_{pwa}.$[73]

5.4 Fixed Effects RD- Total Crime

Panel A of Table 4 uses the fixed effect RD approach described in Equations 5 and 6. Column 1 of Table 4 estimates a positive effect of alcohol sales on crime, conflicting with all parameter estimates reported so far. This is likely due to there being no time fixed effects in Column 1.

Column 2 adds time (specifically event-week) fixed effects. The parameter estimate goes back to being negative and is statistically significant at 95% confidence. In column 3, agency clustered standard errors are added. The statistical significance falls below 90%, because there are only 8 clusters.

Columns 4-6 use Equation 6. Defining the dependent variable this way is one way to control for time fixed effects, because away games happen at the same time and any common trend that evenly affects home and away games does not bias the estimate. In column 4 of Panel A, the estimate is 0.06 *less* crimes at the age cutoff for those old enough to legally drink, due to sales being allowed. Unlike column 1, column 4 controls for time periods with the dependent variable, instead of by using dummies for time fixed effects. In this case, the effect remains negative and statistically significant. Column 2 found similar results.

Column 5 uses time fixed effects and differences out away game crime in the dependent variable. The coefficient is still neegative and statistically significant. Column 6 uses cluster-robust standard errors, and despite having few groups, the differencing strategy reduces uncertainty (compared to Column 3) causing the estimate to be statistically significant above 90% significance.

[73] Lemieux and Milligan (2008) differences out the same individual, this approach differences out the same time period.

Panel A: FE-RD Models						
	Home Only			Differencing Out Away		
	(1)	(2)	(3)	(4)	(5)	(6)
Legal In-Stadium Alcohol Sales	0.075**	-0.099**	-0.099	-0.060*	-0.109**	-0.109*
	(0.035)	(0.042)	(0.053)	(0.033)	(0.044)	(0.048)
Observations	1260	1260	1260	1260	1260	1260
R-Squared	0.0937	0.157	0.157	0.0188	0.0283	0.0283
Adj. R-Squared	0.0828	0.137	0.137	0.00696	0.00541	0.00541
Agency Fixed Effects	X	X	X	X	X	X
Event Study Week Dummies		X	X		X	X
Robust SEs	X	X		X	X	
City-Clustered SEs			X			X
Panel B: Triple Diff. Crime Commission						
	Total	Assault	Simp. Ast.	Vandal	Rape	Liq Law
Home*Sales*Age≥21	0.084	-0.093**	-0.099**	-0.221*	-0.821***	-1.226**
	(0.085)	(0.043)	(0.048)	(0.115)	(0.231)	(0.599)
Observations	325796	325796	325796	325796	325796	325796
Panel C: Triple Diff: Victimization						
	Total	Assault	Simp. Ast.	Ag. Ast.	Intim	Rape
Home*Sales*Age≥21	-0.095***	-0.125***	-0.142**	-0.168**	0.033	-0.022
	(0.036)	(0.043)	(0.056)	(0.082)	(0.084)	(0.279)
Observations	325796	325796	325796	325796	325796	325796

Table 4: The Effect of Alcohol Sales on Crime Commission and Victimization at Minimum Legal Drinking Age Cutoff Using Fixed Effects RD and Triple Differences

Note: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. All estimates include data only from 18-23 year old individuals. In Panel A, FE-RD estimates are presented, the unit of analysis is age-event week-agency, and the dependent variable is total crime in all columns. In Panel A, columns 1-3, only home game days included (β from Equation 5). In Panel A, columns 4-6, dependent variable is home game crime - away game crime(β from Equation 6). 8 city clusters are used in Panel A. Panels B and C use triple difference Poisson conditional fixed effects models from Equation 4. Agency, year, and month fixed effects, day of week and holiday indicators are included. Standard errors clustered at the city level. Column 1 uses total crime, column 2 uses assaults- aggravated, simple, and intimidation, column 3 uses simple assault. Panel B, column 4 the dependent variable is vandalism, column 6 is liquor law violations. Panel C, column 4 the dependent variable is aggravated assault, column 5 is intimidation.

These estimates are less likely to be biased from the omission of law enforcement variables, yet remain negative.

5.5 Triple Difference- Crime Commission

To deal with sparse crime data by age and data, an alternative estimation strategy is triple differences. The objective is obtaining an estimate of how crime differentially affected the age groups before and after the change.[74] Non-home games have been the control group so far, so this approach compares legal to underage individuals on gameday to non-gameday, before and after the change to sales.

Panel B of Table 4 presents triple differences estimates of the effect of alcohol sales on crime commission. Column 1 starts by finding no effect of sales on total crime. The reason for the insignificance may be because total crime has many heterogeneous crime types that have stronger influence on regression results when the data is split by age.

Panel B of Table 4, column 2 changes the dependent variable to assault crimes.[75] The treatment effect is now negative and statistically significant at 95% confidence. It can be interpreted as 21-23 year olds commit 0.093% less assaults on gamedays compared to non-home games than 18-20 year olds, after the change to sales. Column 3 changes the dependent variable to only simple assaults, the kind of assault most associated with college gameday and alcohol consumption. The coefficient is 95% significant and almost the exact same magnitude as column 2. This suggests that the effect of alcohol sales on assault works through simple assault which is associated with gameday.

Column 4 uses vandalism as the dependent variable and the effect is statistically significant above 90 percent confidence. Column 5 uses rape as the dependent variable and finds a negative and statistically significant effect at 99% confidence. The parameter estimate is very large, implying a 127% decrease in rapes committed by 21-23 year olds who gain access to alcohol compared to those who are too young, so it should be interpreted with caution.[76]

[74] Heaton (2012) uses untreated places as a third difference. For this study, untreated places would not solve the omitted variable of enforcement, because enforcement only changes on gamedays in treated colleges.

[75] Assault includes aggravated assault, simple assault, and intimidation.

[76] Table A.5 shows that this is due to increase in rape for underage and decrease in rape for legal age of similar magnitudes. Table A.6 shows that the significance and magnitude depends on the ages included. Changing ages sometimes does not change the parameter estimate which could be indicative the results are driven by few observations.

Column 6 shows alcohol sales reduce liquor law violations for those above the cutoff age, because in-stadium alcohol can be bought. The coefficient is very large and should also be interpreted cautiously. Despite some data limitations for either regression method, the negative and statistically significant results in Panel B are also negative and statistically significant like Panel A.

5.6 Triple Difference- Crime Victimization

One possible critique so far is only crime that is caught is included in the dependent variable. Panel C addresses this concern by using victimization, which could be reported even if the offender is not caught.[77] Table 4, Panel C, column 1 reports the triple difference coefficient of alcohol sales on total crime victimization. It is estimated that the ability to purchase alcohol is associated with a 9.5% reduction in victimization for those just old enough compared to those just young enough. The coefficient is statistically significant at 99 percent confidence and is also close to the parameter estimates, in Panel B, on committing assault.

Table 4, Panel C, column 2 shows there is a 12.5 percent reduction in being the victim of any type of assault.[78] There is no statistically significant effect on intimidation, so the assault results are not driven by the relatively less harmful act of intimidation. Also there is no effect found on rape victimization for 18-20 year olds compared to 21-23 year olds.[79] In summary, the victimization results are consistent with crime commission results, suggesting alcohol sales does reduce criminal involvement for those who gain access to in-stadium alcohol sales.

6 Conclusion

The main finding is increased access to in-stadium alcohol at college football games reduces crime. There is no evidence that this negative and statistically significant relationship is driven by overly

[77] Rape victimization is also the main dependent variable in Lindo et al. (2018).

[78] Column 3 shows there is also a statistically significant effect on aggravated assault which is perhaps less associated with college football than simple assault. While it is not impossible that alcohol sales could also influence aggravated assault, it is more likely to affect simple assaults. Table A.7 shows that the parameter estimates on aggravated assault is more sensitive to ages included than the parameter estimates on simple assault.

[79] Table 4 shows there is no effect on rape for most age combinations although there is a reduction in rape victimization for 18-20 year olds compared to 21 year olds which is statistically significant at 90 percent confidence. There are also no significant increases in rape victimization or of any other crime type for any age combination considered.

narrow standard errors, unrelated crimes, crimes far from game time, different control groups, or separately examining different agency types. No measurable pre-trends exist in an event-study.

There is no effect of including attendance, but clearance rates decline after sales are allowed which suggests a reduction in enforcement activities (Mas, 2006). The unexpected negative sign of alcohol sales on crime could therefore be due to reductions in enforcement intensity on gamedays, so age-based variation is used to create estimates from a control group less vulnerable to omitted, confounding dimensions of law enforcement, like strictness, beats, and number of officers. There is no difference in enforcement for those who just gain alcohol access compared to those who are just barely too young to gain access after in-stadium sales are allowed. Estimates using age-based variation are feasibly less vulnerable to bias from omitted dimensions of law enforcement which may change at the same time of the policy, but not be measurable and therefore bias the treatment effects. Two different estimators using age-based variation support alcohol access decreasing crime and triple differences shows victimization decreases among ages old enough to drink.

There are several limitations. One limitation, similar to the RD framework, is that the bias-adjusted, triple difference estimates are local to the age cutoff of 21 which means they may not be generalizable to other age cutoffs. There is no hypothesis testing using unexpected outcomes which is the norm in prior studies (Lindo et al., 2018; Rees & Schnepel, 2009).[80] No data on alcohol sales or alcohol consumption, such as quantity or type, is used which would be more ideal than simply availability. All teams changed to allowing sales, making it impossible to make inferences about whether banning alcohol sales at a place already with alcohol would have a symmetric effect. Finally, the crime data is not a random sample of agency-team combinations and so results may not generalize to under-represented areas of the sample such as the Southeastern Conference (SEC).

This paper makes 3 contributions. The first contribution is explicitly investigating the role of alcohol access on crime at college football games using multiple policy changes in quasi-experimentally minded econometric approaches. This relationship has only been implied before (Carpenter & Dobkin, 2010; Lindo et al., 2018; Rees & Schnepel, 2009), but this paper uses several policy changes which increase access to alcohol on college football home gamedays. The findings that alcohol access reduces crime are more consistent with the theoretical model allowing for sub-

[80] Making inferences based on this small number of teams that change and games with unexpected outcomes makes statistical inference imprecise.

stitution and empirical results of Boyes and Faith (1993) than estimates presented in Bormann and Stone (2001) and Menaker and Chaney (2014). Findings suggest alcohol is important, like Lindo et al. (2018) and Rees and Schnepel (2009) imply, but increased access reduces crime.

The second contribution is investigating a different alcohol access policy on crime, compared to prior literature (Biderman et al., 2010; Carpenter & Dobkin, 2015; Green, Heywood, & Navarro, 2014; Grönqvist & Niknami, 2014; Hansen & Waddell, 2018; Heaton, 2012; Yörük, 2014). Previous work estimating the causal effect of alcohol access on crime uses quasi-experimental methods to be internally valid. However, Lindo et al. (2016) shows that external validity is not a given. The uncertainty of external validity makes investigating outcomes of a different policy causing variation in alcohol access a standalone contribution.

The third contribution relates to the broader study of alcohol and crime. NIBRS and college football can be used to learn about alcohol and crime (Carpenter & Dobkin, 2010). No study of the effects of alcohol access on crime has documented a robust, statistically significant, and negative relationship between increased access to alcohol and criminal behavior.

It is worth considering an important difference between this study and others, the policy only affects a few hours out of the day whereas others use policies that are less likely to induce substitution across time such as the minimum legal drinking age (Carpenter & Dobkin, 2010) or a blanket prohibition (Luca, Owens, & Sharma, 2015). The three-period utility maximization problem detailed in Boyes and Faith (1993) details the ambiguities involved with predicting the effect of college stadium alcohol bans on alcohol consumption. These ambiguities are suspected to cause flipping of the sign of alcohol access on crime from positive, like most prior studies on alcohol access and crime, to negative. This suggests that substitution effects matter as hypothesized by Carpenter and Dobkin (2010) and re-emphasizes how internally valid causal estimates of alcohol access on crime are dependent on the environment (Carpenter & Dobkin, 2015; Lindo et al., 2016). Future research could study other alcohol access policies affecting short-time periods to investigate substitution effects as a reason some college football studies find alcohol accesss reduces crime, contrary to general research (Carpenter & Dobkin, 2015; Heaton, 2012).

Related to establishing internal validity here, age-based variation is also applied like other internally valid studies of alcohol and crime (Carpenter & Dobkin, 2015; Lindo et al., 2016). This strengthens the unexpected conclusion of alcohol access reducing crime. Part of the contribution is noting the similarities of the alcohol access changes at college stadiums to other temporal RD strategies (Lemieux & Milligan, 2008; Pettersson-Lidbom, 2012) to leverage age and temporal variation simultaneously to create a control group equally affected by enforcement differences.[81]

These estimates have policy implications for college sports alcohol sales policy. The estimates suggest sales reduce crime, a negative externality of college football. There are drawbacks to the restriction of alcohol sales at college football games such as foregone concession revenue and reductions of enjoyment to fans who would consume alcohol responsibly. Additional revenue could reduce the growth in the cost of college and allow responsible fans to enjoy goods they find complementary, alcohol and sports. Results imply the net effect of alcohol sales at college football games is higher social welfare.

The second policy implication is how these results to inform alcohol policy more generally. In line with economic theory (Boyes & Faith, 1993) and the hypothesis of a review on alcohol access and crime (Carpenter & Dobkin, 2010), there is evidence that substitution across time and types of products matters. Viewing alcohol access and alcohol consumption problem as a single period maximization problem masks these nuances. Alcohol access policies should therefore carefully consider how individuals will adjust their alcohol consumption over multiple periods and whether this could have unintended consequences. This research suggests the human response to artificially increasing the price of alcohol for short time periods is substitution of consumption which results in worse outcomes due to the pharmacological effects of substituting across types or time periods.

[81] As shown by age continuity at cutoff in Figure 5.

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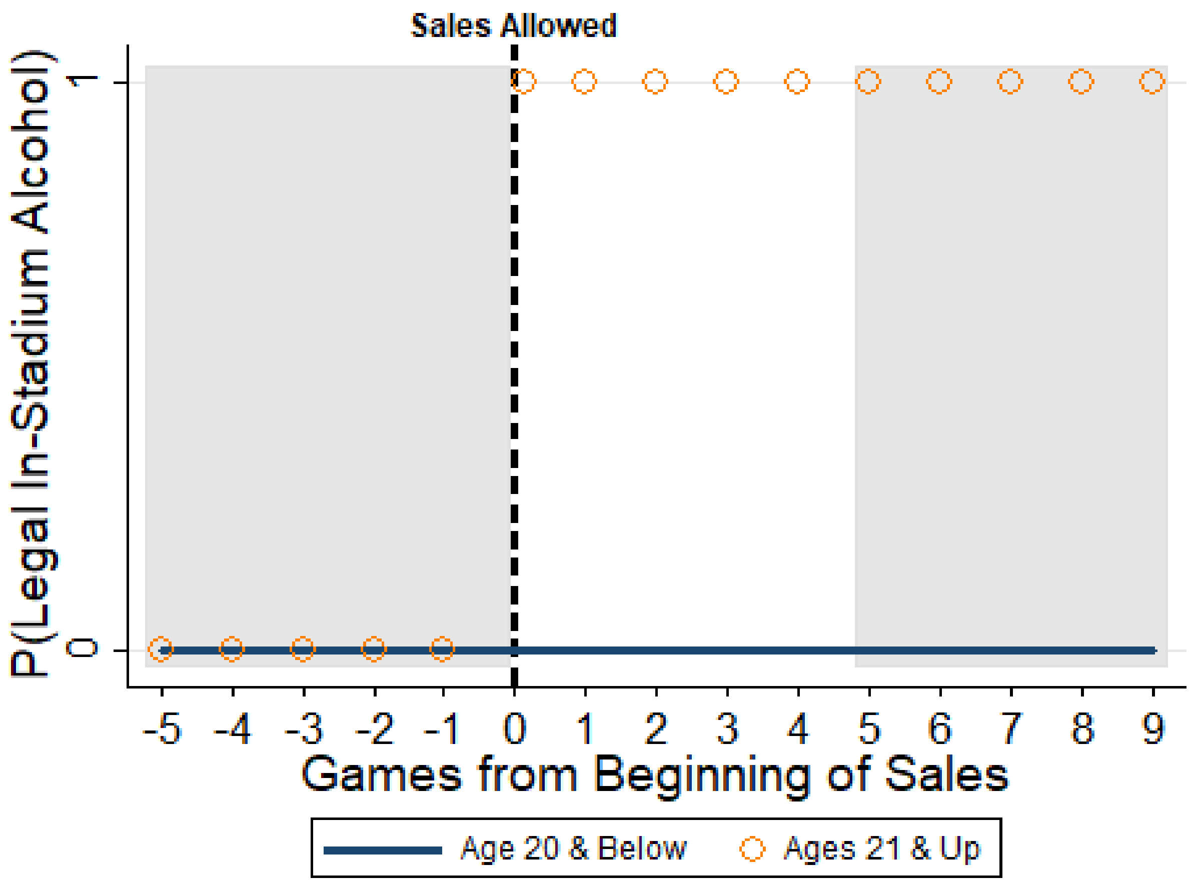
A Online Appendix

Figure A.1: Time-Varying Difference in In-Stadium Legal Alcohol Availability by Age Group



Note: Gray shaded areas to non-shaded indicate beginning of new season. In the United States, the minimum legal drinking age is federally set to age 21, since the 1970's.

Table A.1: Colleges In Sample

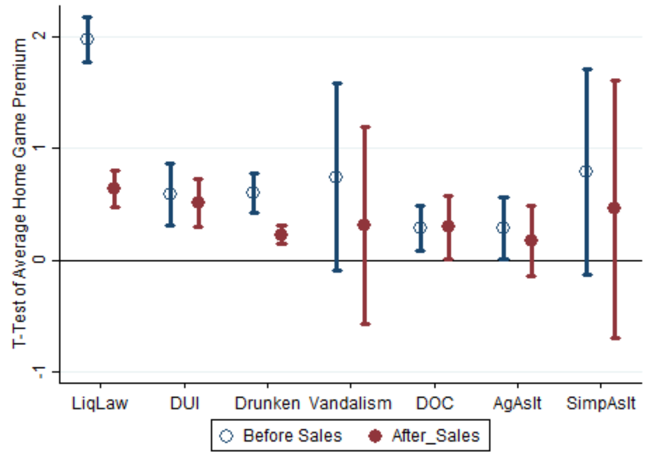
Institution	First Sales Season	Agencies
Akron	2008-2009, 2012-2015	2
Bowling Green University	2009	2
Cincinatti	2011	1
Colorado State University	2008	2
Colorado-Boulder University	2014	3
Louisville University	2008	1
Memphis	2009	2
North Texas	2014	1
Toledo	2013	1
West Virginia University	2011	2
Western Kentucky University	2012	2
Air Force	Never	1
Arkansas	Never	2
Arkansas State University	Never	2
Boise State University	Never	2
Brigham Young University	Never	2
Central Michigan University	Never	2
Clemson	Never	2
Eastern Michigan University	Never*	2
Idaho University	Never	1
Iowa	Never	2
Iowa State University	Never	2
Kansas	Never	2
Kansas State	Never	2
Kentucky	Never	2
Marshall	Never	2
Michigan State University	Never	2

Michigan University	Never	2
Middle Tennessee University	Never	2
Ohio	Never	2
Ohio State University	Never	2
Oregon State University	Never	1
South Carolina University	Never	2
Tennessee	Never	2
Texas Christian University	Never	1
Texas Tech University	Never	1
Utah	Never	1
Utah State University	Never	2
Vanderbilt	Never	2
Virginia	Never	2
Virginia Polytechnic University	Never	2
Washington	Never	1
Washington State University	Never	2
Western Michigan University	Never	3

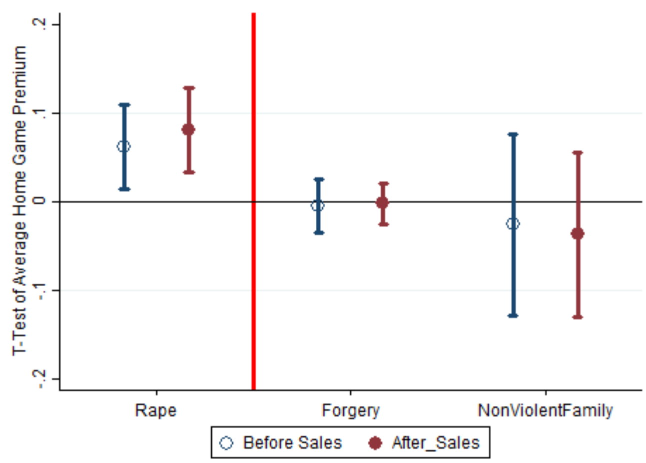
Source: Author reviewed internet for news regarding all teams in the NIBRS datasets in D-1 College Football. First Sales Season column is not necessarily the same year that sales were introduced, it is the years sales begin in the sample, meaning 2008 stands for a team that had sales in the stadium the entire sample period. Eastern Michigan had sales for one game in 2015.

Figure A.2: T-Tests of Gameday Crime Premiums, Before and After Sales Allowed

Panel (A): T-Tests For Differences in Means



Panel (B): Rape T-Tests and Falsification Checks



Notes: Y-axis = Home Gameday Crime - Non-Home Game Days Crime, the home gameday crime premium, or the t-test point estimate. T-tests to the right of the red line in Panel (B) are falsification tests. For Before Sales, blue, empty circles, which are second of each pair, n = 14,203. For After Sales, red, filled in circles, which are second of each pair, n = 22,913. DOC = disorderly conduct. AgAslt = Aggravated Assault. SimpAslt = Simple Assault.

Table A.2: Descriptions of Crime Types Used

Code	Offense	Description
13A	Aggravated Assault	An unlawful attack on one person upon another wherein the offender uses a weapon or displays it in a threatening manner or the victim suffers obvious severe bodily injury involving apparent broken bones, loss of teeth, internal injury, severe laceration, or loss of consciousness
13B	Simple Assault	An unlawful physical attack by on one person upon another where neither the offender displays a weapon, nor the victim suffers obvious severe bodily injury involving apparent broken bones, loss of teeth, internal injury, severe laceration, or loss of consciousness
13C	Intimidation	To unlawfully place another person in reasonable fear of bodily harm through the use of threatening words and/or other conduct, but without displaying a weapon or subjecting the victim to actual physical attack
290	Vandalism	To willfully or maliciously destroy, damage, deface, or otherwise injure real or personal property without the consent of the owner or person having custody or control of it
11A	Forcible Rape	The carnal knowledge of a person, forcibly and/or against that persons will or against the persons will in instances where the victim is incapable of giving consent because of his/her temporary or permanent mental or physical incapacity
90C	Disorderly Conduct	Any behavior that tends to disturb the public or decorum, scandalize the community, or shock the public sense of morality
90D	Driving Under the Influence	Driving or operating a motor vehicle or common carrier while mentally or physically impaired as the result of consuming an alcoholic beverage or using a drug or narcotic
90E	Drunkenness	To drink alcoholic beverages to the extent that ones mental faculties and physical coordination are substantially impaired
90G	Liquor Law Violations	The violation of laws or ordinances prohibiting the manufacture, sale, purchase, transportation, possession, or use of alcoholic beverages

Table A.3: Robustness to Model Type

	(1) Negative Binomial	(2) OLS
Home	0.72***	6.71***
	(0.11)	(1.62)
HomeXSales	-0.52***	-5.82***
	(0.19)	(1.99)
Observations	162898	162898

Notes: * p<0.1, ** p<0.05, *** p<0.01. All models estimated using conditional fixed effects models. Standard errors cluster-robust at the city-level in parentheses. All models include agency, year, and month fixed effects. All models use indicator variables for day of week and holidays including: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Column 1 uses a negative binomial model. Column 2 uses OLS.

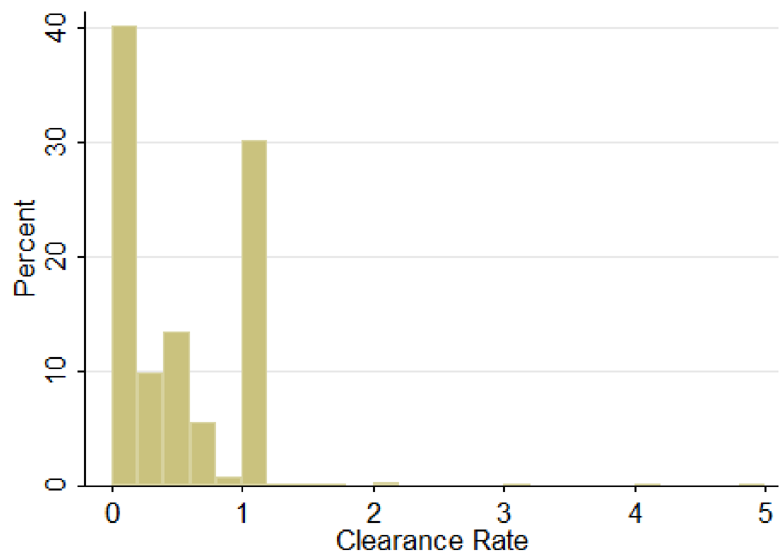
Table A.4: Effect of Alcohol Sales on Specific, Related Crime Types

Panel A: Location-Specific Crimes	(1)	(2)
Home	0.10***	0.06***
	(0.03)	(0.02)
HomeXSales	-0.11*	-0.07**
	(0.07)	(0.03)
Observations	162898	162898
B Crimes Incl.	None	None
Location Types Incl.	Narrow	Broad

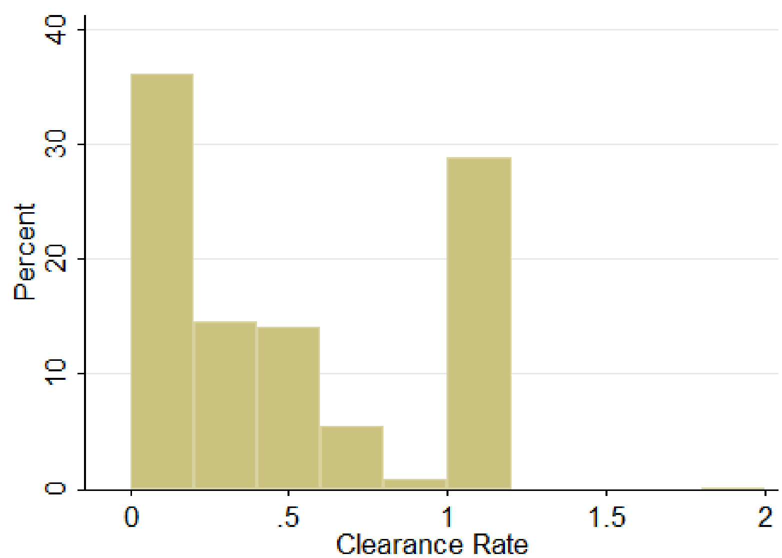
Note: All models estimated using Poisson conditional fixed effects models. Standard errors cluster-robust at the city-level in parentheses. All models include agency, year, and month fixed effects. All models use indicator variables for day of week and holidays including: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Panel A, column 1 only includes crimes that are committed in the following locations: bar/nightclub, street, liquor store, parking lot, school/college, and arena/stadium. Panel A, column 2 uses the same crime locations and adds: convenience stores, field/woods, grocery store, residence, restaurant, gas station, and gambling facility.

Figure A.3: Distribution of Clearance Rate

Panel A: Clearance Rate Distribution- Age-Date Combos, No Report Excluded



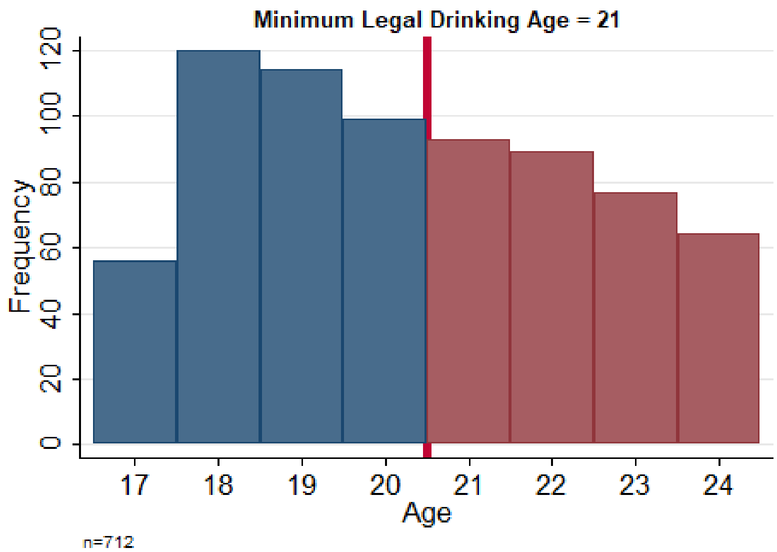
Panel B: Clearance Rate Distribution- Age-Date Combos, No Report Excluded, Home Games



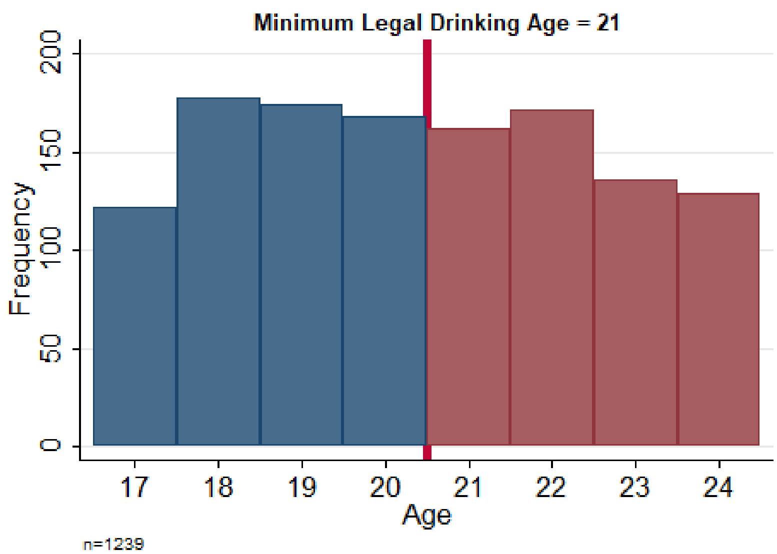
Note: Displays clearance rates distribution. It can be seen that few clearance rates lie outside (0,1). Binned at intervals of 0.2.

Figure A.4: Examining If Age-Based Variation Feasibly Controls for Enforcement Bias; Density of Having a Report

Panel A: Pre. Home Gameday Clear. Rate Density



Panel B: Post Home Gameday Clear. Rate Density



Note: Panel A and B show the likelihood of having at least 1 report on home gameday by age. At age 21, individuals above age 21 are able to drink during the game in the post period. At age 18, individuals become a legal adult.

Table A.5: Triple Difference, Max. Age 24, Min. Age 18

Panel A: Type B Crimes	Liquor Law			DUI			Disorderly Conduct		
	(1) ≥21	(2) <21	(3) Both	(4) ≥21	(5) <21	(6) Both	(7) ≥21	(8) <21	(9) Both
Home*Sales	-1.87	-0.79	-0.78	-0.12	-0.24	-0.25	-0.84	-0.75	-0.75
	(0.59)	(0.16)	(0.16)	(0.09)	(0.13)	(0.12)	(0.31)	(0.40)	(0.41)
H*S*21Plus			-1.17			0.14			-0.09
			(0.55)			(0.14)			(0.20)
Panel B: Type A Crimes	Rape			Vandalism			Aggrevated Assault		
	≥21	<21	Both	≥21	<21	Both	≥21	<21	Both
Home*Sales	-0.39	0.52	0.53	-0.32	-0.12	-0.11	-0.30	-0.10	-0.10
	(0.16)	(0.26)	(0.26)	(0.09)	(0.16)	(0.16)	(0.07)	(0.10)	(0.09)
H*S*21Plus			-0.93			-0.22			-0.20
			(0.27)			(0.12)			(0.10)

Notes: * p<.10, ** p<.05, *** p<.01. All models estimated using Poisson conditional fixed effects models. All models include month, year fixed effects, reporting agency fixed effects and day of week fixed effects, and holiday indicator variables. Agg. Assault is aggravated assault. Columns 1, 4, and 7 only include 21-24 year olds. Columns 2, 5, 8 only include 18-20 year olds. Columns 3, 6, and 9 include 18-24 year olds.

Table A.6: Table Varying Min/Max Ages, Crime Comission

Panel A: Liquor Laws	Min. Age = 18				Min. Age = 20			
	(1) 18/21	(2) 18/22	(3) 18/23	(4) 18/24	(5) 20/21	(6) 20/22	(7) 20/23	(8) 20/24
H*S*21Plus	-0.88	-1.02	-1.23	-1.17	-0.54	-0.68	-0.89	-0.84
	(0.56)	(0.52)	(0.60)	(0.55)	(0.53)	(0.52)	(0.60)	(0.56)
Panel B: Rape	Min. Age = 18				Min. Age = 20			
	(1) 18/21	(2) 18/22	(3) 18/23	(4) 18/24	(5) 20/21	(6) 20/22	(7) 20/23	(8) 20/24
H*S*21Plus	-0.56	-0.83	-0.82	-0.93	-0.29	-0.68	-0.68	-0.68
	(0.56)	(0.31)	(0.23)	(0.27)	(0.57)	(0.40)	(0.27)	(0.32)
Panel C: DUI	Min. Age = 18				Min. Age = 20			
	(1) 18/21	(2) 18/22	(3) 18/23	(4) 18/24	(5) 20/21	(6) 20/22	(7) 20/23	(8) 20/24
H*S*21Plus	0.07	0.14	0.09	0.14	-0.08	-0.01	-0.06	-0.02
	(0.14)	(0.12)	(0.13)	(0.14)	(0.12)	(0.11)	(0.14)	(0.15)
Panel D: Vandalism	Min. Age = 18				Min. Age = 20			
	(1) 18/21	(2) 18/22	(3) 18/23	(4) 18/24	(5) 20/21	(6) 20/22	(7) 20/23	(8) 20/24
H*S*21Plus	-0.21	-0.10	-0.22	-0.22	-0.19	-0.10	-0.17	-0.17
	(0.17)	(0.12)	(0.12)	(0.12)	(0.24)	(0.18)	(0.19)	(0.20)
Panel E: Alc. Suspected (Type A)	Min. Age = 18				Min. Age = 20			
	(1) 18/21	(2) 18/22	(3) 18/23	(4) 18/24	(5) 20/21	(6) 20/22	(7) 20/23	(8) 20/24
H*S*21Plus	-0.35	-0.11	-0.17	-0.27	-0.22	0.03	-0.03	-0.13
	(0.20)	(0.14)	(0.22)	(0.20)	(0.30)	(0.24)	(0.32)	(0.31)

Notes: N = 325,796 in all regressions. Standard errors cluster-robust at the city-level in parentheses. All models estimated using Poisson conditional fixed effects models. The coefficient reported, H*S*21Plus, is the triple difference coefficient from a triple difference model comparing how the policy change affected the home game day crime (compared to non-home game days) for those above the MLDA vs those below the MLDA. All models use reporting agency, month, and year fixed effects. All models use indicator variables for day of week and holidays including: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Columns 1-4 use the minimum age of 18 and add 1 year older individuals with each additional column. Columns 5-8 use the minimum age as 20 in each regression.

Table A.7: Table Varying Min/Max Age, Crime Victimization

	Min. Age = 18				Min. Age = 20			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	18/21	18/22	18/23	18/24	20/21	20/22	20/23	20/24
Panel A: Total Victimization								
H*S*21Plus	-0.12	-0.13	-0.09	-0.06	-0.08	-0.10	-0.06	-0.03
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)
Panel B: Rape								
H*S*21Plus	-0.71	-0.40	-0.02	0.15	-0.59	-0.28	0.10	0.27
	(0.43)	(0.41)	(0.28)	(0.23)	(0.46)	(0.45)	(0.35)	(0.28)
Panel C: Total Assault								
H*S*21Plus	-0.04	-0.15	-0.12	-0.12	-0.05	-0.16	-0.14	-0.13
	(0.07)	(0.06)	(0.04)	(0.04)	(0.08)	(0.08)	(0.05)	(0.05)
Panel D: Intimidation								
H*S*21Plus	-0.05	-0.21	-0.17	-0.12	-0.03	-0.18	-0.14	-0.10
	(0.10)	(0.10)	(0.08)	(0.08)	(0.14)	(0.14)	(0.12)	(0.13)
Panel E: Simple Assault								
H*S*21Plus	-0.00	-0.14	-0.14	-0.15	-0.03	-0.17	-0.17	-0.18
	(0.09)	(0.08)	(0.06)	(0.05)	(0.10)	(0.09)	(0.07)	(0.06)
Panel F: Aggravated Assault								
H*S*21Plus	-0.19	-0.08	0.03	0.04	-0.15	-0.03	0.08	0.08
	(0.11)	(0.10)	(0.08)	(0.07)	(0.13)	(0.10)	(0.09)	(0.09)

Note: N = 325,796 in all regressions. All models estimated using Poisson conditional fixed effects models. The coefficient reported, H*S*21Plus, is the triple difference coefficient from a triple difference model comparing how the policy change affected the home game day crime (compared to non-home game days) for those above the MLDA vs those below the MLDA. Standard errors cluster-robust at the city-level in parentheses. All models use reporting agency, month, and year fixed effects. All models use indicator variables for day of week and holidays including: Thanksgiving, Halloween, Labor Day, Columbus Day, and Veterans Day. Columns 1-4 use 18 year olds as the undergage minimum age cutoff. Columns 5-8 use 20 years olds as the underage minumum age cutoff. Columns 1 and 5 use the maximum age for the overage age cutoff as 21. Columns 2 and 6 use the maximum age for the overage age cutoff as 22. Columns 3 and 7 use the maximum age for the overage age cutoff as 23. Columns 4 and 8 use the maximum age for the overage cutoff as 24.