DRINKING AND THE BLUES: THE EFFECT OF SUNDAY ALCOHOL SALE BANS ON TEEN DRINKING BEHAVIORS IN GEORGIA

by

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(Under the Direction of Joshua Berning)

ABSTRACT

Current blue laws are primarily concerned with the preclusion of the sale of alcohol on Sunday in a handful of states. In this study, we examine an important potential secular benefit of Sunday sales bans: a reduction in underage drinking, which is linked to a multitude of deleterious risky behaviors. We exploit a pseudo-natural experiment which has arisen from the heterogeneity in Sunday sales status across counties and municipalities in Georgia. To account for potential endogeneity of the decision to repeal, we employ both fixed effects and an instrumental variable approach. Across several different model specifications, we find no effect of repeal on underage drinking. Policy-makers would do well to seek methods other than sale prohibitions to illicit change in underage drinking rates.

INDEX WORDS: Blue Laws, Sunday Sales, Sunday Closing Laws, Underage Drinking, Risky Behavior, Prohibition, Panel Data, Fixed Effects, Instrumental Variable Regression

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DEDICATION

I dedicate this thesis to my parents, Gene and Carol, to my sister and brother-in-law, Liz and James, and to my girlfriend Hannah. Unconditional love is the only thing in life.

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I am thoroughly appreciative of all the guidance and tips given by my committee members Dr. Greg Colson and Dr. Travis Smith. They showed me to think outside of the box when confronting the many speed bumps inherent to research. This wisdom I will take with me on my many endeavors to come. I am especially grateful to Dr. Josh Berning. He served as a mentor to my professional and personal development, and became a friend.

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CHAPTER 1

INTRODUCTION

Underage drinking poses a serious threat to public health; its effects create serious personal, social, and economic consequences. Its use is widespread among youth. Alcohol is consumed by more young people than tobacco or illegal drugs, and the highest level of alcohol dependence in the US population is among 18- to 20-year-olds. 35 percent of high school students report having had at least one drink in the past 30 days. A staggering number of individuals begin drinking at very young ages. Nearly one-third of adolescents begin drinking before age 13 (Office of the Surgeon General 2007). Carpenter and Dobkin (2009) find an increase in mortality rate of 9 % following increased alcohol consumption at age 21. For individuals under 21, drinking contributes to the three leading causes of death among teens: unintentional injury, homicide, and suicide (Miller et al. 2007). Annually, about 5,000 young people die from alcohol-related injuries (Office of the Surgeon General 2007).

Further, alcohol consumption may lower inhibitions and affect ability to assess risks (Ertan Yoruk and Yoruk 2015). For example, underage drinking is linked to increased sexual activity in teens (Sen 2002, Miller et al. 2007, Ertan Yoruk and Yoruk 2015). Grossman and Markowitz (2005) find that alcohol consumption among teens lowers the probability of using birth control and condoms. Carpenter (2005) finds that youth targeted drunk driving laws reduced the gonorrhea rate among 15-19-year-old white males. Chesson et al. (2000) identify a lower sexually transmitted disease rate in the USA after an increase in alcohol taxes. Miller et al.

(2007) link binge drinking, the most common pattern of alcohol consumption among high school youth, to dating violence.

Several studies point to the linkage between drinking and poor academic performance (Carrell et al. 2011, Miller et al. 2007), increased tobacco use, (Bobo and Husten 2000, Grant et al. 2004, Miller et al. 2007), and illicit drug use (Miller et al. 2007).

Furthermore, the negative effects of early drinking may have consequences spanning far beyond adolescence. A portion of individuals who engage in underage consumption of alcohol will experience an alcohol use disorder or engage in heavy drinking later in life. Heavy lifetime use of alcohol is linked to increased risk for medical problems such as cancer, liver cirrhosis, pancreatitis, and stroke. There is also reason to worry that underage consumption can impede the proper functioning of the developing brain, which may reduce overall productivity later in life (Office of the Surgeon General 2007).

One form of regulation aimed at reducing alcohol availability and consumption is the enforcement of sales bans on particular days of the week, most notably Sunday. These are so called Blue laws. In recent decades, however, the USA has seen a dramatic decrease in the number of states which impose alcohol sale restrictions (DISCUS 2015). A relevant question is whether such ban repeals will have an effect on underage drinking and associated risky behaviors.

In this study, we examine an important potential secular benefit of Sunday sales bans: a reduction in underage drinking. Specifically, we estimate how the repeal of Sunday alcohol bans affects the incidence of and ease of access to alcohol for minors. The effect of lifting Sunday sales bans on underage drinking is not immediately clear. Minors cannot legally purchase alcohol, hence it is not certain whether their consumption patterns would change in response to

repealing the bans. At the same time, a greater number of sales days increase the availability and therefore the ability of underage drinkers to obtain alcohol, *ceterus parebus*. Our results, which indicate no effect of repeal on underage drinking behaviors, provide insight into whether Sunday alcohol sales bans do indeed attenuate negative externalities associated with excess drinking, and thus can be used to guide legislatures into formulating laws that benefit society in the best way. More broadly, our results also contribute to the literature examining the efficacy of product sales bans and restrictions.

We make three important contributions to the literature. First, we estimate the effect of Sunday sales ban repeals on underage drinking. Second, to our knowledge, we are the first to account for endogeneity of repeal using an instrumental variable approach. Third, and also to our knowledge, this study is the first to examine within state variation in repeal.

CHAPTER 2

BACKGROUND AND MOTIVATION

Blue laws that restrict commerce on Sundays have been a point of contention since their inception. Although their origin is religious, Sunday closing laws have been in place adherent to the state's objective to preserve the health, safety, and welfare of its citizens (Dillof 1979). Following a Supreme Court decision in 1961, a slew of legal challenges were levied on blue laws contending they did not satisfy this objective of preservation (Theuman 2005). Over the years, the collection of classifications of commerce permissible on Sundays has exceedingly increased. In 2015, while few restrictions on Sunday commerce persist, laws targeting only the sale of alcohol remain intact in certain states. Currently, 38 states permit Sunday retail sales of alcohol products. Since 2002, 16 states have joined the list of states allowing Sunday sales. While 16 of these 38 states have enacted statewide repeals, 22 states allow a local option – cities and unincorporated counties are permitted to hold citizen referenda to allow retail stores to sell packaged beer, wine, and liquor on Sundays.

Across the US, there does not appear to be any pattern in the order of the repeals by state. The results of Price and Yandle (1987), who examine what state characteristics lead to repeal and find no significant explanatory variables, highlight the heterogeneous circumstances which bring about blue law repeal. As such, the individual phenomena that serve as the impetuses to repeal are widely varied and idiosyncratic. For example, Vermont, Minnesota, and Utah repealed blue laws through litigious battles. Others, such as North Dakota, repeal blue laws through legislation following court rulings. Still, other states, such as Texas, changed blue laws after lobbying from

the regulated industries or, as occurred in South Carolina and New Jersey, lobbying from individuals.

Blue Laws in Georgia

In April 2011, Georgia legislation passed a bill, approved by Governor Deal, that would allow a local option in the form of citizen referenda, to permit retail stores to sell beer, wine, and liquor on Sundays. News articles reporting on the repeals cite tax revenue and industry lobbying from retailers as the impetus for holding and passing a referendum. As time progressed, the amount of referenda taking place converged toward zero. For referenda occurring during the years 2013 through 2014, news articles cite pressure from surrounding localities as the main influence to hold referenda. For example, alcohol retailers complained that customers were traveling to nearby cities that had previously repealed to do their Sunday shopping.

The decision to repeal the statewide ban was contentious. Deal's predecessor, Governor Purdue, famously vowed to veto any legislation aimed at allowing Sunday sales, citing an influential paper by Lapham and McMillan (2006), which linked a 40% increase in alcohol-related vehicle accidents with blue law repeal in New Mexico[1]. The general defense of maintaining blue laws on the books has hinged on the claim that they provide secular benefits to society by limiting drinking and thus they reduce alcohol-related issues in public health, e.g. drunk driving fatalities, domestic violence, illicit drug use, and underage drinking. And specifically in Georgia during the course of political battle, two activist groups, the Georgia

[1] Nonetheless, the methodological approach used by Lapham and McMillan has been deemed problematic because they observe data from only one state dealing with only one policy change (Stehr 2010, Lovenheim and Steefel 2011).

Christian Coalition and the Georgia Baptist Commission, which were heavily involved in anti-repeal lobbying, cited increased drunk driving, underage drinking, and domestic violence as their main arguments against repeal of blue laws. If these benefits are found to be untrue, however, Sunday sales bans become hard to justify. They entail costs such as lost tax revenue and consumer deadweight loss (Lovenheim and Steefel 2011). Further, previous authors examining blue laws have pointed to potential health benefits. For example, the extra day of sales might in effect smooth drinking behaviors across more days, thus reducing binge drinking and its deleterious related reckless behavior (Carpenter and Eisenburg 2009). It is also arguable that Sunday sales may curb drunk-driving fatalities if their repeal induces individuals, who would otherwise consume alcohol at a bar, to consume alcohol from the safety of their homes (Lovenheim and Steefel 2011). Still, others question the efficacy of Sunday sales bans. Stehr (2007) asks "why should public policy target traffic fatalities on Sundays and not other days? If the goal is to reduce reckless behavior… a ban on Friday or Saturday sales… would probably be more effective."

Literature Review

There is some evidence of societal effects of comprehensive bans, that is, bans encompassing all goods including alcohol, on commerce on Sundays. For example, some work has considered the impact of blue laws on labor and profitability. Tullock (1975) provides a theoretical framework that predicts firms within communities that repeal blue laws will see their profits fall. Moorhouse (1984), however, tests the Tullock hypothesis empirically and refutes his claim. Similarly, Gradus (1996) shows that, theoretically, the imposition of blue laws may lead to a decrease in employment. This assertion is backed up by empirical work by Goos (2005),

who shows that repealing the blue laws has had a positive effect on employment in sectors that are most effected by the laws.

There has been work quantifying the impacts of comprehensive bans on other aspects of society. Lee (2012) uses a difference-in-difference specification to examine the effect of statewide repeals on educational attainment, and finds a reduction in years of education of about 0.13 years. Gerber et al. (2008) describe a small negative effect on voter turnout with the repeal of comprehensive Sunday sale bans.

Policy-makers stress the importance of policies which reduce the negative effects of alcohol use. To the effect that comprehensive bans impact society, there is little evidence in the literature that sale bans targeting alcohol specifically provide public health benefits. A key concern with policy-makers is that repealing a ban might increase overall alcohol consumption due to higher availability. To the contrary, Carpenter and Eisenburg (2009) find no increase in overall consumption in Ontario Canada following repeal of Sunday alcohol sale bans. Interestingly, their results do point to a shifting effect. That is, they find a 15% increase in consumption on Sunday commensurate with a fall in consumption on Saturday. It is arguable, then, that perhaps the repeals decrease the health hazard of alcohol by smoothing out consumption over more days. Corroborating this work, Stehr (2007) finds a slight increase in alcohol sales following repeal throughout the US, though he observes significant cross-border shopping, which accounts for at least 20% of the increase in sales.

There has been a plethora of studies analyzing alcohol sale ban repeals and drunk driving fatalities, likely one of the most obvious and important potential impacts of the bans. Interestingly, there has been mixed results in the literature. Lapham and McMillan (2006) note a surprising increase in car crashes of 42% in the state of New Mexico following its repeal of

Sunday sale bans in 1990. Doubt has been cast onto the finding of Lapham and McMillan (2006) by more recent work. Stehr (2010) compares changes in alcohol-related vehicle fatalities with changes in non-alcohol related vehicle fatalities and finds they change similarly in all states, including New Mexico. Lovenheim and Steefel (2011) deem the methods of Lapham and McMillan (2006) problematic: the study entails only one state and only one policy change, making it very difficult to disentangle the effects of policy from trends or secular shocks. Lovenheim and Steefel (2011) expand the past work on vehicle fatalities by including fixed effects in their model, which allows them to control for omitted variables that are correlated with repeal and vehicle accidents. They find no effect of repeal on vehicle fatalities.

To our knowledge, this is the extent of the literature on the public health benefits of Sunday alcohol sale bans. No one has yet to examine underage alcohol abuse, an effect that is likely ignored because underage individuals are precluded from purchasing alcohol anyway. That being said, it is clear that adolescents have the ability to procure alcohol by other means, thus an increase in availability of alcohol may increase the access of alcohol to minors. As was clear from the mixed results in the literature, a broader examination of the connection between blue laws and public health outcomes, that is, one that employs different identifying assumptions, is warranted. It may be the case that in studies involving endogenous policies, simply controlling for possible correlated trends may not fully ameliorate the bias. Our use of an instrumental variable approach offers a new identifying assumption to the study of product sale bans.

CHAPTER 3

DATA AND METHODS

Data

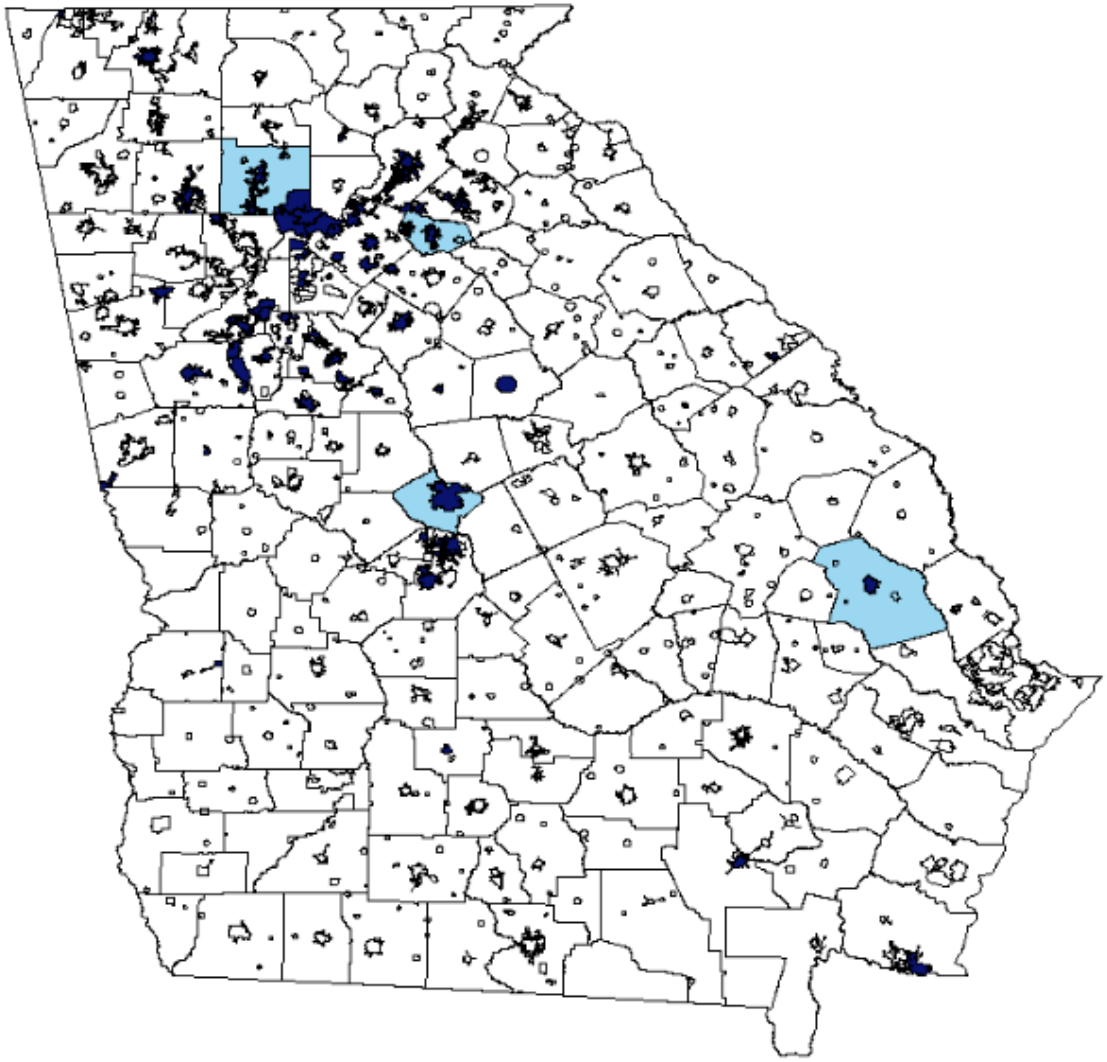
The first of the referenda in Georgia were permitted to occur in November 2011. Roughly 120 counties and municipalities voted, resulting in the first packaged alcohol Sunday sale to happen in Winder, Georgia on November 13th of that year. In the subsequent years, 280 referenda have taken place out of 668 localities in Georgia. Of those 280 referenda, 244 localities now sell alcohol by the package on Sundays. Based on the idiosyncratic vindications for repeal for states across the US mentioned above, a discussion of the ongoing process of repeal in Georgia is warranted. The scattering of referenda and effective dates between the years 2011 and 2014 may help to elucidate different impetuses in cities/counties repealing in Georgia. Table 2.1 presents the number of referenda and effective dates at the city and county level for each year. As stated above, the bulk of the referenda occurred immediately following the repeal of the statewide ban, in late 2011. The lag between the timing of these 2011 referenda and the effective dates can also be seen: 77 out of 110 localities which passed the referendum in 2011 waited until 2012 to officially repeal for various legal and bureaucratic reasons. The changes in laws reflected in Table 2.1 can be visually seen in Figure 2.1 – 2.3, which present maps of Georgia expositing city and county level repeals. Notice that the repealing cities and counties are concentrated around densely populated areas e.g. Atlanta, Savannah, Macon, and Athens.

Table 2.1. Timing of Referenda and Effective Dates

	Referenda Date				Effective Date		
	City		County		City	County	
Year	Referenda	Passed	Referenda	Passed			Total
2011	118	101	10	7	77	4	81
2012	60	58	50	45	77	41	118
2013	32	26	5	4	23	9	32
2014	0	0	3	2	9	3	12
Total	210	185	68	58	186	57	243

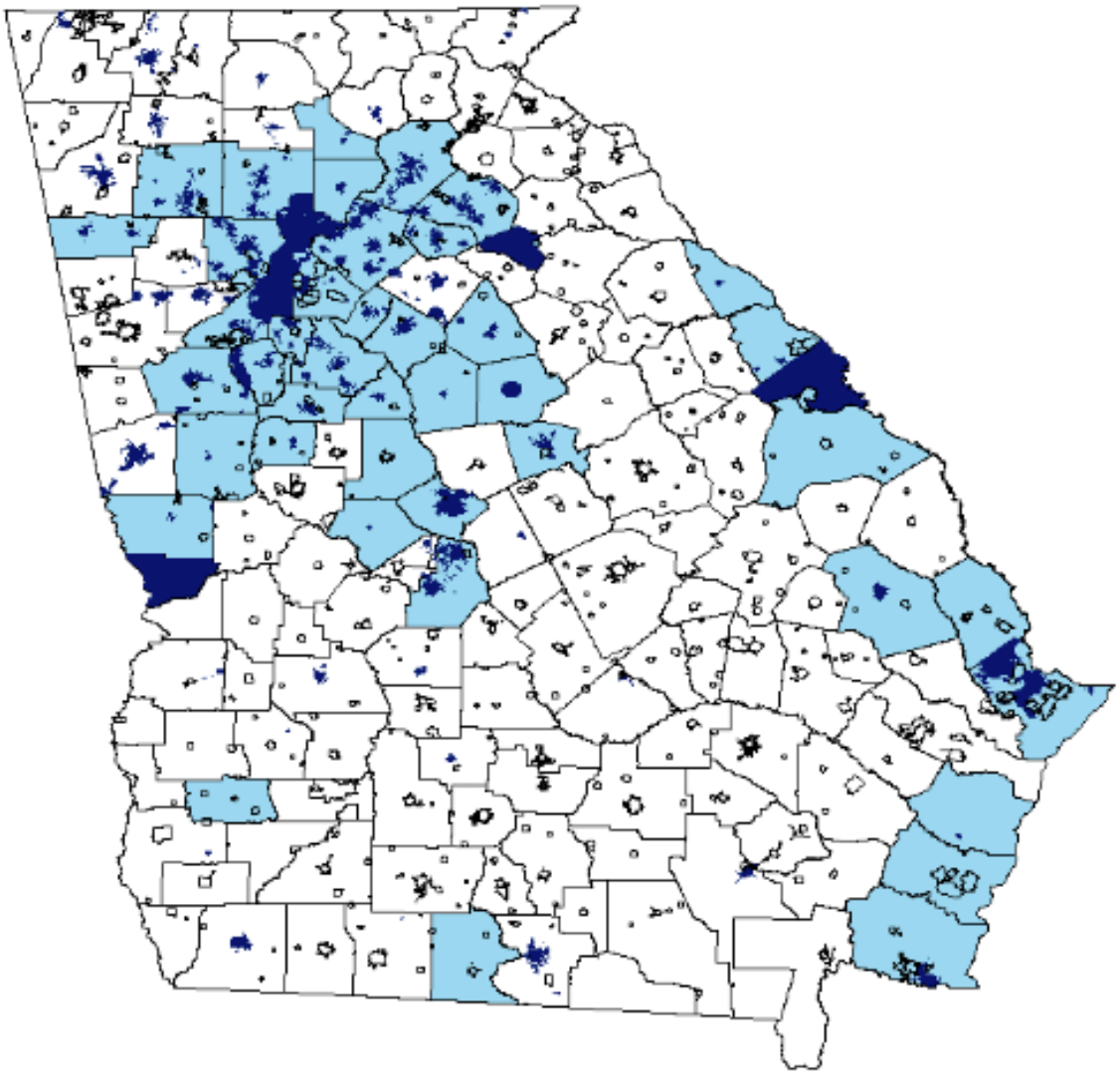
Referenda dates and effective dates of localities across Georgia

Figure 2.1. Georgia repeal municipalities as of 2011



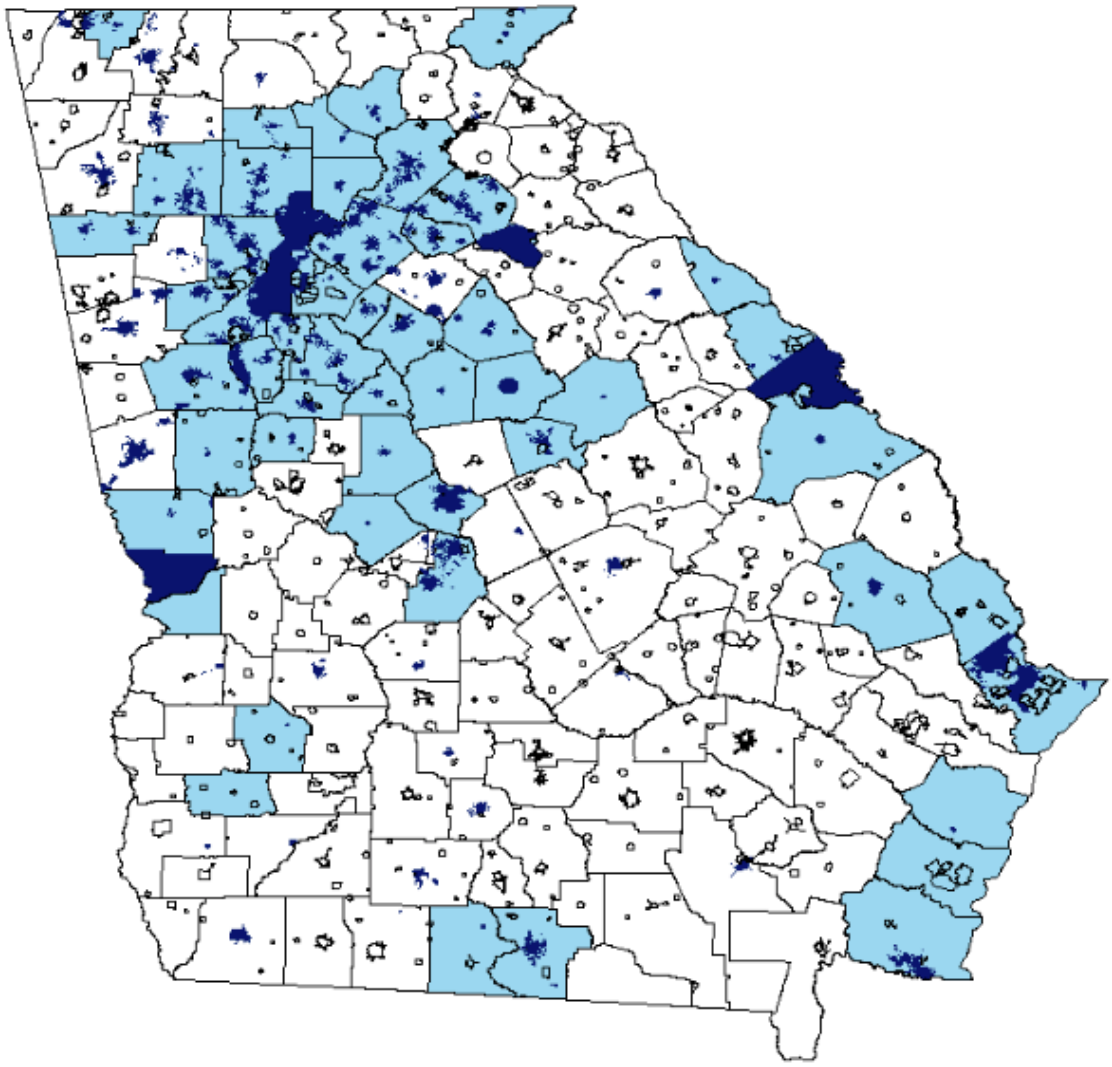
Georgia counties and cities that repealed by the end of 2011. Repeal cities are dark blue, repeal counties are light blue.

Figure 2.2. Georgia repeal municipalities as of 2012



Georgia counties and cities that repealed by the end of 2012. Repeal cities are dark blue, repeal counties are light blue.

Figure 2.3. Georgia repeal municipalities as of 2013



Georgia counties and cities that repealed by the end of 2013. Repeal cities are dark blue, repeal counties are light blue.

Voting referendum results were compiled using two methods. We gathered information concerning the existence of referendum in a particular locality, rationale behind the holding of referendum, date of referendum, outcome of referendum, and date of repeal enactment, via local news articles. When necessary, such as instances in which no local news is provided for particular places, we made in-person phone calls to locality officials to collect the above information.

We obtained data on underage drinking behaviors from 2008-2013 from the Georgia Student Health Survey II which is administered by the Georgia Department of Education. The survey is an anonymous, self-reported statewide survey instrument developed to identify safety and health issues that have a negative impact on student achievement and school climate. The survey is offered commensurate with the requirements of No Child Left Behind which specifies that data must be collected for categories including incidence, prevalence, age of onset, perception of health risks, and perception of social disapproval of drug use and violence. We utilize two responses from this survey: *Past30Use* and *Ease of Access*. The first is a response to the statement: "Within the past 30 days, I have used alcohol…" where the student selects the number of days they have used alcohol. The second variable is a response to the statement "It's easy to get alcohol," where the student selects one of four responses: Strongly Agree (1), Somewhat Agree (2), Somewhat Disagree (3), and Strongly Disagree (4). There is missing data for 9^{th} and 11^{th} graders. Evidently they were exempt from taking the student survey for the years 2008-2010. These data are summarized in Table 2.2. Importantly, these data represent the average values per school district.

After initially exploring the data, we found that certain students responded with seemingly excessive responses to the past use question. Looking at Table 2.2, the maximum past

use is more than 9 days in the past month for a school district. This clearly indicates that certain students are reporting in excess of 9 days. There are certain practical and health reasons to think these numbers are potential outliers. First, underage drinkers may have a hard time acquiring sufficient alcohol to drink so frequently. This could mean the data are misreported. Secondly, students drinking so frequently seem likely to perform poorly in school and face other health issues.

We identified outliers within the full dataset by comparing individual student responses to the Substance Abuse and Mental Health Services Administration definition of heavy drinking, which is characterized as five or more drinking occasions in the past 30 days (NIAAA). We made the decision to delete any responses in the individual student data where the student had reported five or more drinks per month. The rationale is that if the data are accurate, these students are heavy drinkers and likely have steady access to alcohol. It follows then that the relaxing of a Sunday stricture would not influence their drinking patterns. Further, by eliminating these outliers, our analysis focuses on how the Sunday sales laws might impact those students who currently do not drink. The revised data are followed by an asterisk in Table 2.2.

City-level demographic information is from the US Census. We accessed data on general demographic characteristics such as total population, median income, median age, number of females, number of blacks, and the number of senior citizens. We also collect data on vendors of alcohol, such as the number of supermarkets, convenience stores, gas stations and package stores. Presumably, areas with greater availability of alcohol will be more likely to have underage drinkers. These data are all normalized by school district population.

We also gathered data on various "activities," including amusement and recreational facilities, amusement arcades, amusement parks, marinas, bowling centers, amusement parks,

golf courses, fitness centers, and theatres. We construct an "activities" variable, which is a composite of all listed activities normalized by dividing by total population of a school district. These facilities provide substitute leisure opportunities for underage drinkers.

Before 2010, the demographic data was made available once every five years. Similarly, demographic and industry data is not available for the year 2014, and in some instances, 2013. For this reason, we linearly interpolate such variables for the years missing.

To conduct our analysis, we aggregate our data to the school district level for the following several reasons. First, the repeals occur at the city and county level. This means that, for example, it is possible that two contiguous cities have different Sunday sales laws, or that an entire county has repealed the law, but cities within the county have not repealed. Because of this, it may be difficult to classify one entire city or county as dry on Sunday. Second, the student surveys are anonymous, hence individuals are not indexed in the survey data and we are precluded from panel analysis at the individual level. Third, in some cases one city feeds into multiple schools or multiple cities feed into a single school, making it hard to match a city or county with particular school. Therefore, the repeal variable in our main specification is not an indicator variable at the city level but rather it represents the proportion of individuals within the boundaries of a school district that have experienced repeal.

This particular data limitation may not be a shortcoming, however. In aggregating to school district level, we avoid issues of cross city/county boundary effects. Further, our dependent variables, *Past 30 Day Use* and *Ease of Access*, which are originally discrete numbers, become continuous once averaged. This makes them more amenable to linear regression. The final panel dataset reports student drinking behaviors for the majority of school districts in Georgia, before and after repeal effective dates.

Table 2.2.Summary Statistics for Key Variables

Variable	Obs	Mean	Std. Dev.	Min	Max
Past30Use	935	2.359686	0.941532	0	9.891231
Log of Past30Use1	925	0.79494	0.411945	-2.07944	2.291649
Ease of Access	935	2.210743	0.276385	1	4
Past30Use*	935	0.365997	0.180767	0	1.92684
Log of Past30Use*	924	-1.10128	0.482447	-3.68888	0.655881
Ease of Access*	935	2.284355	0.278313	1	4
Repeal	1014	0.10964	0.290989	0	1
Total Population	1014	60887.25	112661.3	786	914464.4
Median Income	1014	38992.33	10910.71	17225.67	91591.08
Median Age	1014	37.97854	3.803177	28.3	56.25
Fraction Female	1014	0.505904	0.046883	0.229124	0.791647
Fraction Black	1014	0.265437	0.175331	0.001291	0.774126
Fraction Seniors	1014	0.130947	0.035141	0.050662	0.288388
Fraction Educated	1014	0.109098	0.059496	0.02368	0.456797
Supermarkets	1014	11.50552	22.36019	0	189
Conv Stores	1014	5.756608	11.21878	0	114
Gas Stations	1014	30.56489	44.86106	0	364
Package	1014	5.477909	12.3249	0	104.8
nReligiosity	1014	0.000776	0.000285	0.000191	0.002048
Activities	1014	15.73965	38.31041	0	292
Tourist Lodges	1014	12.62367	28.73258	0	272

* signifies that the variable has been rid of outliers.

Methods

The basic fixed effects linear model is as follows:

$$Y_{it} = \alpha_i + \beta Z_{it} + \gamma \%Repeal_{it} + \delta_t + t + \varepsilon_{it} \qquad (1)$$

Where Y_{it} is the log of average Past30Use responses for school district *i* in year *t*, or Ease of Access responses for school district *i* in year *t*; α_i is a school district fixed effect, Z_{it} is a vector of time varying control variables, $\%Repeal_{it}$ is the fraction of school district *i* that has experienced repeal in year *t*, t is a time trend, δ_t is a year fixed effect, and ε_{it} is an error term.

We exploit the pseudo-natural experiment which has arisen from the heterogeneity in packaged Sunday alcohol sales[2] status across localities in Georgia using a fixed effect approach. Variation in the timing of the repeal allows us to disentangle policy effects from spurious trends, e.g. the rollout in timing allows us to control for unobserved shocks to underage drinking that may be correlated with repeal (Hoynes and Schanzenbach 2012, Jensen 2007, Lee 2013, Lovenheim and Steefel 2011). Identifying the causal effect of repeal on underage drinking behavior is a key concern, however, because the incidence of repeal of Sunday bans in particular cities and counties may be endogenous to underage drinking behaviors. Specifically, unobservable characteristics in each voting area may be correlated with the referendum and underage drinking behavior. For example, it is plausible that repeals are introduced and passed in cities/counties where attitudes toward underage drinking are more permissive. Alternatively, economic considerations, and not attitudes toward alcohol, may drive decision to repeal.

[2] While there is some cross city/county variation in on-premise alcohol sale laws, we ignore this variation because underage drinkers typically do not seek to acquire alcohol from such establishments.

Given the potential endogeneity of the referendum, we use several methods to ensure identification. First we run an event study type regression predicting the percentage of a school district that repeals as a function of school district specific demographic, economic, and industrial variables that may have an important impact on the decision to repeal at the city and county level, using a fixed effect approach. We incorporate variables that are found to have significant explanatory power in the event study regressions into our main specification (Lee 2012, Hoynes and Schanzenbach 2011, Besley and Case 2001, Gruber and Hungerman 2008).

Second, we examine pre-trends in underage alcohol use before repeal, differentiating between those school districts that experience any amount of repeal and those that observe no repeal. If repeal is indeed endogenous, one would expect to see differential changes in underage drinking levels in school districts that are to repeal than in school districts that will not repeal, before the repeals take place. We regress both log of Past30Use and Ease of Access on school district and year fixed effects, demographic variables, a lagged dependent variable, and a variable which equals the proportion of the school district that will have experienced repeal by 2013 interacted with a linear time trend in all years through the last year before the first cluster of repeals took place, 2011. The coefficient on the interacted term equals the average annual deviation of past 30 day use and ease of access in the repeal school districts relative to other school districts, *before* the repeals went into effect. If the interaction term is significant, it means we must include district repeal percentages interacted with a time trend in our final specification in order to account for pre-trends (Stehr 2007, Lovenheim and Steefel 2011).

There may be reason to believe that a fixed effects approach on its own cannot adequately control for an endogenous policy- it is not clear that simply including state economic variables leads to an unbiased estimate of the effect of repeal. For example, it may prove difficult

to account for all unobservable characteristics through control variables. Furthermore, FE cannot account for time varying differences across units. An instrumental variable specification may offer a more standard procedure to attenuate the influence of endogeneity on estimation (Besley and Case 2001). Therefore, our final method to secure identification is the use of an instrumental variable, tourist lodges. The rationale for this instrument is as follows: suitable instruments must be correlated with propensity to repeal but uncorrelated with unobservable factors affecting underage drinking behaviors in order to isolate the variation in underage drinking that is independent of repeal. The number of tourist lodging establishments measures the extent of tourism in a given area. Alcohol demand can be expected to be positively correlated to tourism activity, but, importantly, tourists consume the alcohol, not the residents (Nelson and Young 2001). Tourists and merchants who serve them are less concerned with Sunday regulations than are normal citizens (Price and Yandle 1987). Hence, localities with the potential for greater tourist revenues via alcohol sales may be more inclined to vote for Sunday sales. At the same time, bars associated with hotels do not offer a convenient source from which teens may secure alcohol. According to the Federal Trade Commission (2013), almost 72% of teens who drink get the alcohol from friends or family members, or they give money to someone else to buy it for them. This would indicate that the alcohol that teens procure is packaged.

The first stage of the IV estimation is thus:

$$\%Repeal_{it} = \alpha_i + \beta Z_{it} + \eta Tourist_{it} + \delta_t + t + \varepsilon_{it} \quad (2)$$

And the second stage is then:

$$Y_{it} = \alpha_i + \beta Z_{it} + \gamma \%\widehat{Repeal}_{it} + \delta_t + t + \varepsilon_{it} \quad (3)$$

Where $\%Repeal_{it}$ is the percentage of individuals in school district *i* at time *t* that have experienced repeal, $\%\widehat{Repeal}_{it}$ is the predicted value of $\%Repeal_{it}$ from equation (2),

Y_{it} is the log of average Past30Use responses for school district *i* in year *t*, or Ease of Access responses for school district *i* in year α_i is a school district fixed effect, Z_{it} is a vector of time varying control variables, t is a time trend, δ_t is a year fixed effect, and ε_{it} is an error term. $Tourist_{it}$ is the number of tourist lodges (hotels, hostels) in school district *i*, normalized by population of school district *i*, in year *t*.

For all regressions presented in this paper, the following variables are normalized by dividing by total school district population: Female, black, senior, educated, supermarkets, convenience stores, gas stations, package stores, activities, and tourist lodges.

CHAPTER 4

RESULTS

Identification Strategies

We begin this section by presenting the results of identification strategies that we introduce in the methods section above. The first concerns the event type study, in which we identify specific school district characteristics that predict the percentage of school district that has experienced repeal. The results are shown below in Table 4.1. As is evident from the table, *fraction black, fraction educated, median income, total population, religiosity, package stores,* and our instrument *tourist lodges,* are all significant in predicting percent repeal.

The second entails our search for pre-trends in underage past use and ease of access. The results of the regression, presented in Table 4.2, indicate that even before repeal occurred, school districts that would eventually experience any repeal were experiencing an annual increase in Past 30 Day Use relative to the control school districts that was 25 percent higher. Because the interaction term is significant in the past 30 day use regression, we include district repeal percentages interacted with a time trend in the final regressions. The interaction term is insignificant in the Ease of Access regression (Stehr 2007, Lovenheim and Steefel 2011).

Table 4.1. Models Predicting Percent Repeal

	Dependent Variable: Fraction SD Repeal			
	(1)	(2)	(3)	(4)
Fraction Female	-5.495***	-0.368	-0.39	-0.439
	(1.048)	(0.836)	(0.880)	(0.871)
Fraction Seniors	11.18***	0.963	0.713	0.513
	(2.370)	(2.086)	(2.085)	(2.057)
Fraction Black	4.776***	1.872**	2.034**	1.899**
	(0.973)	(0.793)	(0.794)	(0.775)
Median Age	0.0718***	-0.0188	-0.015	-0.0123
	(0.014)	(0.011)	(0.012)	(0.012)
Fraction Educated	-	3.065	3.555*	3.751**
		(1.873)	(1.887)	(1.873)
Median Income	-	4.913***	4.737***	4.831***
		(0.752)	(0.751)	(0.773)
Total Population	-	0.971***	0.976***	0.952***
		(0.254)	(0.254)	(0.254)
Religiosity	-	-1.595**	-1.638**	-1.825**
		(0.718)	(0.712)	(0.712)
Supermarkets	-	-	0.0978	0.096
			(0.113)	(0.112)
Conv Stores	-	-	0.11	0.105
			(0.081)	(0.081)
Package Stores	-	-	-0.547***	-0.529***
			(0.175)	(0.174)
Gas Stations	-	-	-0.0436	-0.0478
			(0.063)	(0.063)
Tourist Lodges	-	-	-	0.450**
				(0.182)
Linear Time Trend	-	-	-	-0.0188
				(0.012)
Year FE	✓	✓	✓	✓
School District FE	✓	✓	✓	✓
Obs	1014	1014	1014	1014
R-Squared	0.143	0.492	0.499	0.503

Notes: Standard Errors in Parentheses. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. This is effectively the first stage of our IV approach. Standard errors are robust to heteroskedasticity and first order serial correlation

Table 4.2. Time Trends in School Districts That Repeal Versus Those That Do Not

	Past30Use	Ease
%Repeal*t	0.251**	0.0176
	(0.121)	(0.045)
Year FE	✓	✓
School District FE	✓	✓
Obs	266	272

Notes: Dependent variables are log of Past 30 Day Use and Ease of Access. %Repeal*t is an interaction between an indicator for whether any proportion of a school district experienced Repeal and a linear time trend. The sample includes data from years 2008 to 2010. Regressors include population, median income, median age; fraction female, black, senior, educated; per capita supermarkets, package stores, convenience stores, gas stations, activities; religiosity.

Past 30 Day Use

We estimate equation (1) with the log of past 30 day use as the dependent variable using school district and year fixed effects and varying covariates (Table 4.3, column 1). We report the results for the dataset where the outliers are omitted as the results are similar as the model with the full dataset[3]. The dependent variable for column (2) is *log of past 30 day use* for just 10^{th} and 12^{th} grades. This specification also contains the interaction of %Repeal and year trend to account for pre-trends existing in the time path of past 30 day use.

We estimate equations (2) and (3) using Generalized Method of Moments using *log of past 30 day use* as the dependent variable and various covariates (Table 4.3, columns 3), and we estimate the *log of past 30 day use* for the 10^{th} and 12^{th} grade only using the GMM estimator (column 4). All models contain the regressors *fraction female*, *fraction black*, *fraction seniors*, and *median age*, but the results are insignificant and omitted.

The results from models (1) – (4) indicate that as communities in the school district repeal their Sunday sales bans on alcohol sales, past 30 day use does not change even at the 10 percent significance level. Further, confidence intervals for the fraction repealed hover tightly around 0.

Looking at the results in column (1), a $1,000 increase in median income is associated with a 7.4 percent decrease in Past 30 Day Use. This suggests that degree of opulence plays a role in underage drinking. For every one grocery store that is added per capita in a school district, Past 30 Day Use increases by about 0.1 percent. This suggests access has a minimal, but significant effect on use. For every one added activity per capita in a school district, Past 30 Day Use decreases by about 0.6 percent, indicating substitution of leisure as might be expected. Interestingly, the lagged past use variable is significant; for a 1 percent increase in the lagged

[3] All results, including for the full dataset, can be found in Appendix A.

Past Use variable, current Past Use decreases by 0.2 percent. This suggests that the perception of underage alcohol abuse last year may serve to allay alcohol abuse this year.

The results of the instrumental variable models (columns 3-4) provide similar results as the fixed effects models. Note that according to the Kleibergen-Paap rkW statistic, the instrument *tourist lodges* does not perform optimally, while the Stock-Wright weak instrument robust inference test p-value indicates orthogonality conditions are valid, and the Kleibergen-Paap underidentification test p-value indicates the matrix of reduced form coefficients is identified. Defying convention, we also estimated a specification using lagged *percent repeal* as an instrument for current percent repeal. The instrument performed exceptionally well based on the previous statistics. The effect of repeal was still found to be insignificant, however. Although a lagged endogenous variable is not an ideal instrument, this result is consistent with our other findings.

Table 4.3. Regression Coefficients for Log of Past30DayUse				
	Fixed Effects		IV GMM	
	(1)	(2)	(3)	(4)
Fraction Repeal	-0.0584	-0.0621	-0.372	-0.522
	(0.107)	(0.111)	(0.637)	(0.660)
	[-0.267,0.151]	[-0.279,0.155]	[-1.621,0.876]	[-1.816,0.773]
Fraction Educated	4.189	4.413	5.523	6.51
	(5.538)	(5.627)	(5.805)	(6.094)
Median Income	-7.368***	-7.923***	-5.246	-4.52
	(2.217)	(2.261)	(4.567)	(4.783)
Total Population	0.241	0.316	0.604	0.88
	(0.471)	(0.502)	(0.777)	(0.814)
Lagged Log Past30Use	-0.190***	-	-0.191***	-
	(0.047)		(0.049)	
Supermarkets	0.902***	1.040***	1.025**	1.233***
	(0.326)	(0.328)	(0.399)	(0.405)
Conv Stores	-0.247	-0.247	-0.178	-0.133
	(0.272)	(0.297)	(0.302)	(0.343)
Package Stores	0.128	0.337	-0.2	-0.196
	(0.534)	(0.565)	(0.771)	(0.851)
Gas Stations	-0.15	-0.238	-0.153	-0.24
	(0.177)	(0.186)	(0.180)	(0.196)
Religiosity	-0.577	-1.178	-1.321	-2.35
	(2.635)	(2.903)	(3.029)	(3.311)
Activities	-5.955**	-8.195***	-6.433**	-9.025***
	(2.601)	(2.521)	(2.686)	(2.669)
%Repeal*Year Trend	0.0119	0.0314	-	-
	(0.044)	(0.046)		
Lagged log Past30Use Te,Tw	-	-0.186***	-	-0.184***
		(0.047)		(0.049)
Linear Time Trend	-	-	✓	✓
Year FE	✓	✓	✓	✓
School District FE	✓	✓	✓	✓
Obs	702	702	702	693
K-Paap rkW t	-	-	6.718	6.871
Stock-Wright p	-	-	0.534	0.379
K-Paap ID p	-	-	0.0115	0.0105

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. For columns (5), (6), and (7), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified

Ease of Access

We estimate equation 1 with the *ease of access* index as the dependent variable and including school district and year fixed effects and varying covariates. The results are displayed in Table 4.4, columns 1-2. Again, the results here are for the dataset that excludes outliers[4]. For column (2), the dependent variable is *ease of access* for just 10^{th} and 12^{th} graders.

We estimate equations (2) and (3) using GMM with *ease of access* index as the dependent variable regressed on various covariates (columns 3). Finally, using the GMM estimator, we estimate *ease of access* for 10^{th} and 12^{th} graders only (column 4). The industry controls per capita *supermarkets*, *convenience stores*, *gas stations*, and *package stores* are included in regression for all models in Table 4.4, but they are omitted here because they are insignificant.

The results from models (1) – (4) indicate that for school districts that observe the relaxation of Sunday sale strictures, the perception of ease of access of minors does not change even at the 10 percent significance level. Further, confidence intervals for the fraction that repealed maintain closely around 0.

In examining the results in column (1), it is important to keep the interpretation of the index in mind, e.g. a lower *ease of access* value corresponds to an easier time procuring alcohol. The effects on *ease of access* are as follows. A one percentage point increase in the fraction of blacks in the population of the school district decreases the *ease of access* index by 0.022 points. This would indicate that minors who reside in black neighborhoods find it easier to acquire alcohol. For every one year increase in median age of the school district, the *ease of access* index is decreased by 0.050, suggesting that overall older populations provide an easier outlet for

[4] All results, including for the full dataset, can be found in Appendix A.

underage access, as is expected. And for every year that passes by, the *ease of access* index increases 0.073, which is indicative of a downward trend in the perception of ease of access.

As was true of the past use models, the results of the instrumental variable models (columns 3-4) provide similar results as the fixed effects model[5].

[5] See final paragraph of page 27for discussion of instrumental variable performance statistics.

Table 4.4. Regression Coefficients for Ease of Access

	Fixed Effects		IV GMM	
	(1)	(2)	(3)	(4)
Fraction Repeal	0.00646	0.0438	0.247	0.358
	(0.036)	(0.037)	(0.307)	(0.344)
	[-0.0649,.0778]	[-.0292,0.117]	[-.354,.848]	[-0.316,1.032]
Fraction Female	2.335*	1.437	2.349*	1.554
	(1.220)	(0.980)	(1.230)	(0.991)
Fraction Seniors	-1.465	-1.008	-1.053	-0.609
	(3.181)	(2.916)	(3.246)	(3.017)
Fraction Black	-2.236**	-1.575**	-2.982**	-2.622*
	(1.001)	(0.781)	(1.385)	(1.401)
Fraction Educated	-4.516	-2.637	-5.368*	-3.82
	(2.765)	(2.527)	(2.972)	(2.750)
Median Age	-0.0495**	-0.0369*	-0.0470**	-0.0333
	(0.022)	(0.021)	(0.023)	(0.022)
Median Income	1.055	0.73	-0.432	-1.171
	(0.846)	(0.878)	(2.147)	(2.345)
Total Populatin	0.0404	0.0346	-0.208	-0.282
	(0.212)	(0.188)	(0.377)	(0.395)
Lagged Ease of Access	-0.0672	-	-0.079	-
	(0.061)		(0.061)	
Religiosity	-0.344	-0.515	0.159	0.145
	(1.081)	(1.108)	(1.263)	(1.363)
Activities	0.672	0.99	0.922	1.37
	(1.180)	(1.338)	(1.255)	(1.478)
Time Trend	0.0728***	0.0494***	0.0794***	0.0574***
	(0.014)	(0.015)	(0.018)	(0.019)
Lagged Ease of Access Te,Tw	-	-0.0998	-	-0.115*
		(0.066)		(0.065)
Industry Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
School District FE	✓	✓	✓	✓
Obs	721	710	721	710
K-Paap rkW t	-	-	6.297	6.314
Stock-Wright p	-	-	0.385	0.237
K-Paap ID p	-	-	0.0142	0.0139

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. For columns (5), (6), and (7), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified

CHAPTER 5

DISCUSSION AND CONCLUSIONS

Using a panel data set on underage drinking patterns and perceptions combined with heterogeneity in percentage of school districts that experienced a repeal of Sunday alcohol sales bans, we analyze whether alcohol-related blue laws provide a secular public health benefit in ameliorating teen drinking behaviors within the state of Georgia. We employ a fixed effects empirical strategy that accounts for the percentage of individuals within a school district that are permitted to purchase alcohol on Sundays. We account for endogeneity of repeals in several ways following methods from the literature. Because the timing of repeal at the city and county level is staggered, the percent of a school district that has experienced repeal changes yearly. This allows us to control for shocks occurring uniformly across school districts that may affect student drinking behaviors. Further, we expand upon the literature by employing an instrumental variable specification, results of which remain commensurate with the results of the fixed effect models.

Our results indicate that there is no evidence that Sunday alcohol sales bans reduce underage alcohol use, nor do they effect the perception of access to alcohol for teens. These findings are not surprising given the conclusions drawn in past literature on the efficacy of blue laws as policy to thwart problems in public health. Further, the finding is in line with the conclusions that total consumption of alcohol is only slightly elevated after statewide sale bans are repealed in the US (Stehr 2007), and not at all elevated in the provinces of Canada (Carpenter and Eisenberg 2009). The effect seems to appear in a shifting of timing of purchasing, not

necessarily increased purchasing (Carpenter and Eisenberg 2009, Lovenheim and Steefel 2011). To the extent that these findings are applicable to Georgia remains to be seen; nevertheless, we feel confident that the legitimacy of our findings are validated by the results of these past studies.

Our results do show other significant determinants of underage alcohol use and perceptions. Our *Past30Use* regressions show that overall alcohol use was trending down over the period 2008 to 2013; as students were exposed to the negative consequences of alcohol use and its related risky behavior, they engaged in drinking less often. This may have been in part an effect of the survey from which we derived our data and its related prevention and intervention programs. The survey is meant to be a guide to identify specific areas of school climate that need the most policy focus. The extent that the survey and intervention programs worked may be partially reflected in the lagged *Past30Use* variable. The finding that alcohol use is in general trending downward in teens is in line with the findings of a major report released by the CDC(2014) in 2014, which showed that underage drinking rates among high school students is at its lowest level since 1991.

Median income's economic significance to underage drinking is not surprising. The higher opportunity cost of drinking due to more expendable income provides an explanation: more affluent families have more options to pass the time than do families that are less well off. The children of well off families engage in other behaviors besides drinking on weekends, for example. The mechanism of the effect of available per capita activities is related to that of income: if there are more activities in which to partake on the weekends, e.g. movies or sporting events, there is a higher opportunity cost for youths to drink. Supermarkets offer an easy outlet for youths to attain alcohol especially if it is through an older family member or friend, which, according to FTC, is the main source of youth alcohol. Interestingly, supermarkets in Georgia are

limited to selling beer and wine; it would be enlightening to know what type of alcohol the youths in the survey were consuming.

The *Ease of Access* regressions offer some striking results. Black communities are typically characterized by disproportionately high rates of drug addiction, teen pregnancy, unusually high levels of violent crime, and robbery (Wilson 1981, Wilson and Aponte 1985, Hogan and Kitagawa 1985). It may be that teen access to alcohol is made easier by such circumstances. Further, our finding is in line with the findings of Truong and Sturm (2009), who describe disadvantaged communities as harboring a higher concentration of alcohol outlets. Their results suggest that living close to alcohol outlets is a risk factor for youths' engaging in alcohol consumption.

Our finding of significant effects of other community characteristics on underage use and access should not be ignored by policy makers. The most important is the finding that black neighborhoods harbor an environment that provides easier access to alcohol for youths. Racial inequality is and has been without a doubt the most pressing issue facing society, especially considering the recent events in Fergusson, Missouri, and Rhode Island, New York. The inequality permeates not only to economic opportunity, crime, education, but even underage alcohol access. The fact that teens living in black communities are better able to procure alcohol presents itself as another symptom of inequality. Of course, policies aimed at any single one of these issues are likely palliative. Inequality needs to be attacked at its core; a subject beyond the scope of this paper.

The finding of the effect of activities is corroborated with research related to positive effects of youths' engagement in the community. There are a broad range of activities, such as sports, art, music, and politics, which provide positive outcomes for youth, one of which is

decrease in the rate of substance abuse (Astone et al. 2014, Burney 2011). Immersing youth into their communities offers an unconventional method to reduce underage drinking. Policy makers may attempt to stem the underage consumption of alcohol by encouraging after school activities, attracting performance venues, and expanding parks and outdoor space.

The results of this research have policy implications to those states that defend the efficacy of blue laws targeting alcohol, of which twelve remain. The rational of imposing a Sunday sales ban is becoming difficult to defend on economic grounds. We expose a potential secular benefit of the bans as false; the bans do not affect teens' drinking behaviors. In general our findings contribute to the literature on the efficacy of Sunday prohibition laws; it is part of the growing evidence that these laws do not achieve their objective, but rather serve as an inconvenience to consumers and also a forfeiture of potential tax revenue from lost sales. Furthermore, there is a rift in logic in banning sales for only one day. If a benefit of Sunday bans were to be identified, it may be the case that a ban pertaining to all days of the week is optimal. In this light, a Sunday ban is rather arbitrary.

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APPENDIX A

Complete Regression Results

The following tables contain full regression results. Table A.1 and A.2 display results of regressions run on the dataset that has been rid of outliers. Table A.3 and A.4 display results of regressions run on the full dataset.

Table A.1. Regression Coefficients Log of Past 30 Day Use, Outliers Omitted

	Fixed Effects				IV GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Repeal	-0.0952	-0.0791	-0.0584	-0.0621	-0.363	-0.377	-0.372	-0.522
	(0.107)	(0.106)	(0.107)	(0.111)	(0.940)	(0.653)	(0.637)	(0.660)
	[-305,.115]	[-288,.129]	[-267,.151]	[-279,.155]	[-.206,1.48]	[-.657,.902]	[-.621,.876]	[-.816,.773]
Fraction Female	3.233	2.463	2.576	3.825	2.426	2.311	2.51	3.64
	(2.731)	(2.653)	(2.627)	(2.649)	(3.746)	(2.659)	(2.632)	(2.688)
Fraction Seniors	-7.718	-5.888	-5.561	-7.231	-7.338	-6.497	-6.148	-8.13
	(7.043)	(6.683)	(6.434)	(6.481)	(7.064)	(6.927)	(6.686)	(6.865)
Fraction Black	-0.106	0.366	1.21	1.895	1.145	1.471	2.347	3.781
	(1.970)	(1.845)	(1.707)	(1.674)	(4.739)	(2.634)	(2.725)	(2.872)
Median Age	0.0379	0.0395	0.0528*	0.0283	0.0276	0.0305	0.049	0.0247
	(0.034)	(0.033)	(0.031)	(0.033)	(0.049)	(0.037)	(0.032)	(0.035)
Lagged Log Past30Use	-0.168***	-0.177***	-0.190***	-	-0.172***	-0.175***	-0.191***	-
	(0.046)	(0.047)	(0.047)		(0.051)	(0.048)	(0.049)	
%Repeal*Year Trend	0.00212	0.0309	0.0119	0.0314	-	-	-	-
	(0.042)	(0.044)	(0.044)	(0.046)				
Fraction Educated	-	2.158	4.189	4.413	-	3.23	5.523	6.51
		(5.527)	(5.538)	(5.627)		(5.686)	(5.805)	(6.094)
Median Income	-	-5.681***	-7.368***	-7.923***	-	-2.948	-5.246	-4.52
		(2.018)	(2.217)	(2.261)		(5.021)	(4.567)	(4.783)
Total Population	-	0.533	0.241	0.316	-	0.979	0.604	0.88
		(0.520)	(0.471)	(0.502)		(0.880)	(0.777)	(0.814)
Supermarkets	-	-	0.902***	1.040***	-	-	1.025**	1.233***
			(0.326)	(0.328)			(0.399)	(0.405)
Conv Stores	-	-	-0.247	-0.247	-	-	-0.178	-0.133
			(0.272)	(0.297)			(0.302)	(0.343)

Table A.1. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Package Stores	-	-	0.128	0.337	-	-	-0.2	-0.196
			(0.534)	(0.565)			(0.771)	(0.851)
Gas Stations	-	-	-0.15	-0.238	-	-	-0.153	-0.24
			(0.177)	(0.186)			(0.180)	(0.196)
Religiosity	-	-	-0.577	-1.178	-	-	-1.321	-2.35
			(2.635)	(2.903)			(3.029)	(3.311)
Activities	-	-	-5.955**	-8.195***	-	-	-6.433**	-9.025***
			(2.601)	(2.521)			(2.686)	(2.669)
Lagged log Past30Use Te,Tw	-	-	-	-0.186***	-	-	-	-0.184***
				(0.047)				(0.049)
Year Trend	-	-	-	-	-0.0547	-0.0167	0.0131	0.0461
					(0.090)	(0.046)	(0.044)	(0.048)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
School District Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Obs	702	702	702	693	702	702	702	693
K-Paap rkW t	-	-	-	-	3.259	6.875	6.718	6.871
Stock-Wright t	-	-	-	-	0.162	0.367	0.387	0.773
Stock-Wright p	-	-	-	-	0.687	0.545	0.534	0.379
K-Paap ID t	-	-	-	-	2.987	6.275	6.38	6.55
K-Paap ID p	-	-	-	-	0.084	0.0122	0.0115	0.0105

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. Confidence intervals are displayed for *Fraction Repeal* only. For columns (5), (6), (7) and (8), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified.

Table A.2. Regression Coefficients for Ease of Access, Outliers Omitted

		Fixed Effects			IV GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Repeal	0.0233	0.014	0.00646	0.0438	0.338	0.236	0.247	0.358
	(0.029)	(0.034)	(0.036)	(0.037)	(0.477)	(0.297)	(0.307)	(0.344)
	[-.034,.080]	[.0518,.079]	[-.0649,.077]	[-.029,.117]	[-.59,1.27]	[-.34,.81]	[-.35,.84]	[-31,1.03]
Fraction Female	1.514	2.377*	2.335*	1.437	2.397	2.436**	2.349*	1.554
	(1.216)	(1.248)	(1.220)	(0.980)	(1.804)	(1.241)	(1.230)	(0.991)
Fraction Seniors	-1.358	-1.194	-1.465	-1.008	-1.784	-0.857	-1.053	-0.609
	(3.209)	(3.104)	(3.181)	(2.916)	(3.327)	(3.154)	(3.246)	(3.017)
Fraction Black	-1.967**	-2.187**	-2.236**	-1.575**	-3.355	-2.790**	-2.982**	-2.622*
	(0.945)	(0.956)	(1.001)	(0.781)	(2.337)	(1.236)	(1.385)	(1.401)
MedianAge	-0.0504**	-0.0520**	-0.0495**	-0.0369*	-0.0396	-0.0469**	-0.0470**	-0.0333
	(0.021)	(0.021)	(0.022)	(0.021)	(0.028)	(0.023)	(0.023)	(0.022)
Lagged Ease of Access	-0.0678	-0.069	-0.0672	-	-0.0896	-0.0774	-0.079	-
	(0.062)	(0.061)	(0.061)		(0.067)	(0.060)	(0.061)	
Time Trend	0.0823***	0.0748***	0.0728***	0.0494***	0.0538	0.0830***	0.0794***	0.0574***
	(0.008)	(0.014)	(0.014)	(0.015)	(0.044)	(0.019)	(0.018)	(0.019)
Fraction Educated	-	-4.694*	-4.516	-2.637	-	-5.266*	-5.368*	-3.82
		(2.625)	(2.765)	(2.527)		(2.763)	(2.972)	(2.750)
Median Income	-	0.872	1.055	0.73	-	-0.658	-0.432	-1.171
		(0.737)	(0.846)	(0.878)		(2.235)	(2.147)	(2.345)
Total Population	-	-0.0208	0.0404	0.0346	-	-0.262	-0.208	-0.282
		(0.218)	(0.212)	(0.188)		(0.396)	(0.377)	(0.395)
Supermarkets	-	-	0.051	0.0482	-	-	-0.0305	-0.0579
			(0.136)	(0.131)			(0.171)	(0.178)
Conv Stores	-	-	0.209	0.122	-	-	0.164	0.0714
			(0.147)	(0.154)			(0.155)	(0.165)

Tabe A.2. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Package Stores	-	-	-0.201	-0.177	-	-	0.0402	0.138
			(0.329)	(0.345)			(0.484)	(0.522)
Gas Stations	-	-	0.0627	0.11	-	-	0.0665	0.118
			(0.096)	(0.101)			(0.100)	(0.106)
Religiosity	-	-	-0.344	-0.515	-	-	0.159	0.145
			(1.081)	(1.108)			(1.263)	(1.363)
Activities	-	-	0.672	0.99	-	-	0.922	1.37
			(1.180)	(1.338)			(1.255)	(1.478)
Lagged Ease of Access Te,Tw	-	-		-0.0998	-	-	-	-0.115*
				(0.066)				(0.065)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
School District Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Obs	721	721	721	710	721	721	721	710
K-Paap rkW t	-	-	-	-	2.938	6.624	6.297	6.314
Stock-Wright t	-	-	-	-	0.667	0.718	0.755	1.399
Stock-Wright p	-	-	-	-	0.414	0.397	0.385	0.237
K-Paap ID t	-	-	-	-	2.707	6.052	6.011	6.048
K-Paap ID p	-	-	-	-	0.0999	0.0139	0.0142	0.0139

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. Confidence intervals are displayed for *Fraction Repeal* only. For columns (5), (6), (7) and (8), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified.

Table A.3. Regression Coefficients Log of Past 30 Day Use, Full Dataset

	Fixed Effects				IV GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Repeal	-0.0466	-0.0377	-0.0462	-0.00786	-0.957	-0.807	-0.667	-0.518
	(0.091)	(0.090)	(0.090)	(0.094)	(1.015)	(0.679)	(0.634)	(0.560)
	[-.22,.13]	[-.215,.13]	[-.223,.13]	[-.193,.17]	[-2.946,1.03]	[-2.13,.524]	[-1.908,.575]	[-1.61,.579]
Fraction Female	-0.439	-0.287	-0.181	1.446	-2.807	-0.649	-0.318	1.179
	(2.218)	(2.352)	(2.348)	(1.615)	(3.785)	(2.377)	(2.302)	(1.669)
Fraction Seniors	1.023	2.042	1.214	-3.794	2.254	1.103	0.265	-4.376
	(4.972)	(4.768)	(5.098)	(4.485)	(5.401)	(5.215)	(5.382)	(4.802)
Fraction Black	0.892	0.956	0.705	0.669	4.547	3.044	2.636	2.422
	(1.915)	(1.942)	(2.043)	(1.555)	(5.272)	(2.939)	(3.060)	(2.716)
Median Age	0.0048	0.00135	0.00114	-0.00296	-0.0256	-0.0177	-0.00622	-0.00929
	(0.031)	(0.031)	(0.033)	(0.036)	(0.048)	(0.035)	(0.033)	(0.037)
Lagged Log Past30Use	-0.161***	-0.159***	-0.171***	-	-0.209**	-0.195***	-0.200***	-
	(0.054)	(0.054)	(0.054)		(0.085)	(0.066)	(0.065)	
%Repeal*Year Trend	-0.0362	-0.0151	-0.00792	-0.00452	-	-	-	-
	(0.033)	(0.035)	(0.034)	(0.036)				
Fraction Educated	-	-5.533*	-5.66	-4.467	-	-3.624	-3.437	-2.602
		(3.274)	(3.576)	(3.294)		(4.139)	(4.458)	(4.027)
Median Income	-	-1.622	-1.472	-0.884	-	3.296	2.135	2.095
		(1.226)	(1.418)	(1.414)		(4.865)	(4.063)	(3.633)
Total Population	-	-0.438	-0.414	-0.857**	-	0.316	0.178	-0.374
		(0.382)	(0.370)	(0.414)		(0.839)	(0.741)	(0.677)
Supermarkets	-	-	-0.0548	-0.133	-	-	0.141	0.0327
			(0.249)	(0.270)			(0.353)	(0.351)
Conv Stores	-	-	0.281	0.251	-	-	0.419	0.348
			(0.259)	(0.267)			(0.321)	(0.312)

Table A.3. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Package Stores	-	-	0.499	0.808	-	-	-0.0441	0.352
			(0.569)	(0.597)			(0.848)	(0.861)
Gas Stations	-	-	0.321*	0.302*	-	-	0.33	0.302*
			(0.195)	(0.176)			(0.204)	(0.182)
Religiosity	-	-	0.12	-0.852	-	-	-1.22	-1.962
			(2.140)	(2.270)			(2.665)	(2.646)
Activities	-	-	-0.399	-3.874	-	-	-1.147	-4.595
			(2.407)	(2.630)			(2.678)	(2.859)
Lagged log Past30Use Te,Tw	-	-	-	-0.193***	-	-	-	-0.216***
				(0.052)				(0.059)
Year Trend	-	-	-	-	0.047	-0.0209	-0.0159	-0.016
					(0.095)	(0.036)	(0.030)	(0.029)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
School District Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Obs	704	704	704	697	704	704	704	697
K-Paap rkW t	-	-	-	-	3.06	6.597	6.594	6.662
Stock-Wright t	-	-	-	-	1.457	2.136	1.565	1.125
Stock-Wright p	-	-	-	-	0.227	0.144	0.211	0.289
K-Paap ID t	-	-	-	-	2.817	6.03	6.273	6.361
K-Paap ID p	-	-	-	-	0.0933	0.0141	0.0123	0.0117

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. Confidence intervals are displayed for *Fraction Repeal* only. For columns (5), (6), (7) and (8), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified.

Table A.4. Regression Coefficients Ease of Access, Full Dataset

	Fixed Effects				IV GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction Repeal	0.0421	0.03	0.0224	0.0568	0.355	0.261	0.263	0.397
	(0.029)	(0.033)	(0.036)	(0.036)	(0.468)	(0.294)	(0.305)	(0.339)
	[-.015,.995]	[-.035,.953]	[-.048,.093]	[-.014,.128]	[-.56,1.27]	[-.315,.83]	[-.335,0.86]	[-.268,1.06]
Fraction Female	1.464	2.247*	2.218*	1.16	2.339	2.308*	2.232*	1.291
	(1.192)	(1.231)	(1.198)	(0.945)	(1.735)	(1.219)	(1.202)	(0.960)
Fraction Seniors	-1.669	-1.575	-1.621	-0.894	-2.1	-1.23	-1.213	-0.477
	(3.045)	(2.972)	(3.033)	(2.797)	(3.141)	(3.036)	(3.115)	(2.908)
Fraction Black	-1.804*	-2.006**	-2.040**	-1.292*	-3.173	-2.628**	-2.777**	-2.418*
	(0.943)	(0.955)	(1.001)	(0.769)	(2.265)	(1.209)	(1.356)	(1.356)
MedianAge	-0.0485**	-0.0496**	-0.0487**	-0.0366*	-0.0378	-0.0443**	-0.0462**	-0.0328
	(0.020)	(0.020)	(0.022)	(0.020)	(0.027)	(0.022)	(0.022)	(0.021)
Lagged Ease of Access	-0.0831	-0.0853	-0.0833	-	-0.11	-0.0974	-0.0986	-
	(0.062)	(0.061)	(0.062)		(0.069)	(0.060)	(0.061)	
Time Trend	0.0786***	0.0705***	0.0686***	0.0469***	0.0504	0.0791***	0.0753***	0.0556***
	(0.008)	(0.013)	(0.013)	(0.014)	(0.043)	(0.018)	(0.017)	(0.018)
Fraction Educated	-	-3.86	-3.731	-1.887	-	-4.461*	-4.591	-3.176
		(2.522)	(2.649)	(2.442)		(2.657)	(2.855)	(2.676)
Median Income	-	0.836	1.011	0.569	-	-0.752	-0.467	-1.484
		(0.715)	(0.824)	(0.823)		(2.213)	(2.135)	(2.320)
Total Population	-	0.0327	0.0877	0.14	-	-0.219	-0.16	-0.203
		(0.233)	(0.229)	(0.207)		(0.398)	(0.382)	(0.394)
Supermarkets	-	-	0.0222	0.0638	-	-	-0.0599	-0.0519
			(0.129)	(0.125)			(0.166)	(0.174)
Conv Stores	-	-	0.2	0.137	-	-	0.155	0.0822
			(0.146)	(0.142)			(0.156)	(0.158)

Table A.4. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Package Stores	-	-	-0.19	-0.218	-	-	0.0517	0.124
			(0.310)	(0.325)			(0.473)	(0.510)
Gas Stations	-	-	0.00817	0.0502	-	-	0.0114	0.0574
			(0.086)	(0.090)			(0.089)	(0.095)
Religiosity	-	-	-0.513	-0.526	-	-	-0.00917	0.188
			(1.010)	(1.030)			(1.208)	(1.307)
Activities	-	-	0.743	1.39	-	-	0.995	1.803
			(1.134)	(1.250)			(1.211)	(1.409)
Lagged Ease of Access Te,Tw	-	-	-	-0.0967	-	-	-	-0.118*
				(0.065)				(0.064)
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
School District Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Obs	721	721	721	710	721	721	721	710
K-Paap rkW t	-	-	-	-	3.004	6.703	6.391	6.376
Stock-Wright t	-	-	-	-	0.791	0.933	0.883	1.846
Stock-Wright p	-	-	-	-	0.374	0.334	0.347	0.174
K-Paap ID t	-	-	-	-	2.766	6.125	6.095	6.102
K-Paap ID p	-	-	-	-	0.0963	0.0133	0.0136	0.0135

Notes: Standard errors robust to heteroskedasticity and first order serial correlation. * indicates significance at the 10 percent level, ** indicates significance at the 5 percent level and *** indicates significance at the 1 percent level. Confidence intervals are displayed for *Fraction Repeal* only. For columns (5), (6), (7) and (8), the Kleibergen-Paap rkW statistics indicate the IV is not performing optimally. The Stock-Wright p-values indicate that orthogonality conditions are valid. The Kleibergen-Paap ID p-values indicate matrix of reduced form coefficients is identified.