Published in final edited form as:
J Health Econ. 2014 July ; 36: 33–46. doi:10.1016/j.jhealeco.2014.03.004.

Can technology help to reduce underage drinking? Evidence from the false ID laws with scanner provision*

Barış K. Yörük†

Barış K. Yörük: byoruk@albany.edu

†Department of Economics, University at Albany, SUNY, 1400 Washington Ave., Albany, NY 12222

Abstract

Underage drinkers often use false identification to purchase alcohol or gain access into bars. In recent years, several states have introduced laws that provide incentives to retailers and bar owners who use electronic scanners to ensure that the customer is 21 years or older and uses a valid identification to purchase alcohol. This paper is the first to investigate the effects of these laws using confidential data from the National Longitudinal Survey of Youth, 1997 Cohort (NLSY97). Using a difference-in-differences methodology, I find that the false ID laws with scanner provision significantly reduce underage drinking, including up to a 0.22 drink decrease in the average number of drinks consumed by underage youth per day. This effect is observed particularly in the short-run and more pronounced for non-college students and those who are relatively younger. These results are also robust under alternative model specifications. The findings of this paper highlight the importance of false ID laws in reducing alcohol consumption among underage youth.

Keywords

alcohol consumption; false ID laws with scanner provision

1 Introduction

Underage drinking statistics in the United States are alarming. Although consuming alcohol under the age of 21 is illegal, people aged 12 to 20 years drink 11% of all alcohol consumed in the United States, and more than 90% of this alcohol is consumed in the form of binge drinking (Office of Juvenile Justice and Delinquency Prevention, 2005). On average,

*This paper uses confidential data provided by Bureau of Labor Statistics (BLS). The views expressed in this paper are those of the author and do not necessarily reflect those of the BLS. The author also acknowledges financial support by the National Institute on Alcohol Abuse and Alcoholism of National Institutes of Health under Award Number R03AA020636. The content of this paper is solely the responsibility of the author and does not necessarily represent the official views of the National Institutes of Health. Dohyung Kim and Jungtaek Lee provided valuable research assistance.

underage drinkers consume more drinks per drinking occasion than adult drinkers (National Research Council and Institute of Medicine, 2004). Alcohol is also responsible for more than 4, 700 deaths and 185, 000 emergency room visits per year among underage youth.[1] Furthermore, recent research linked alcohol consumption among minors to a long list of adverse social and economic outcomes including future alcohol dependency and abuse (Hingston, Heeren, and Winter, 2006), poor academic performance (Carrell, Hoekstra, and West., 2011 and Lindo, Swensen, and Waddell, 2013), reduced employment (Renna, 2008), crime (Carpenter and Dobkin, 2010), and risky sexual behavior (Waddell, 2012).

In light of these findings, policy makers have proposed several regulations to reduce the incidence of underage drinking. Among many others, perhaps the most direct form of regulation targeted towards young adults in the United States is imposing a minimum legal drinking age (hereafter, MLDA). Since 1988, it is illegal for youths under age 21 to purchase or consume alcohol in the United States. The punishments for purchasing or consuming alcohol under the MLDA or selling alcohol to a minor varies considerably across states, but include fines, jail time, loss of a liquor license for retailers, and temporary license revocation for underage buyers. Minors who use false identification may also face additional punishment (Bellou and Bhatt, 2013). However, it is clear that the MLDA or associated punishments does not entirely prevent underage drinking. Underage drinkers can access alcohol through a number of sources, including stealing, purchasing alcohol themselves using a false identification, obtaining it from stores that do not check for identification, and asking an older adult to purchase it on their behalf (Century Council, 2003). Furthermore, retailers often disregard age requirements and sell alcohol to minors (Preusser, Williams, and Weinstein, 1994). In an attempt address these problems, recently, several states have introduced laws that provide incentives to retailers and bar owners who use electronic scanners to ensure that the customer is 21 years or older and that the identification is legitimate.[2] These incentives include an affirmative defense in prosecutions for sales to minors if the retailer can show that the scanner was used properly.[3] For instance, section 7 of New York's ABC law provides retailers with an affirmative defense when properly using an ID scanner during the sale of alcohol to a minor. If a retailer inadvertently sells to a minor who provides a false identification and gets caught, then with the transaction record stored in an ID scanner, the retailer would be able to claim an affirmative defense. Without an ID scanner, the retailer may be found guilty of serving alcohol to a minor and depending on severity of the violation, may be forced to pay fines up to $10, 000, may lose its liquor license, and/or face jail time up to one year.[4]

Alcohol compliance checks are the most widely used tool to identify licensed alcohol establishments that sell alcohol to underage youth. During a compliance check, law enforcement officials send underage youth into retail stores or bars to purchase alcohol with their own underage identification. Using an electronic scanner to check whether the customer is 21 years or older appears to be a safe and relatively cheap way of passing a

[1]See, for example, Center for Disease Control and Prevention (2008) and Center for Behavioral Health Statistics and Quality (2012).
[2]Electronic scanners read birthdate and other information digitally encoded on identification cards and help retailers to determine whether the identification is valid and the buyer is 21 years or older.
[3]Affirmative defense is facts and arguments that, if true, will exonerate a defendant, even if all allegations in the complaint are true.
[4]The highlighs of current New York Liquor Law is available at http://www.sla.ny.gov/handbook-for-retail-licenses.

compliance check.[5] However, there are no data on the actual number of retail stores or bars that own and actively use an electronic scanner or the number of citations made for underage sales.[6]

A priori, it is not clear whether the false ID laws with scanner provision (hereafter, FSP laws) would have a meaningful impact on youth access or reduce underage alcohol consumption. For instance, the FSP laws may not affect underage drinking, if teens substitute towards other methods of obtaining alcohol, such as asking an older adult to purchase it on their behalf or obtaining it from retail stores that do not use electronic scanners. It is also possible that only few retailers actually use electronic scanners to avoid selling alcohol to minors. If this is the case, then the FSP laws should not affect youth access to alcohol in a meaningful way. Therefore, quantifying the impact of the FSP laws on underage alcohol consumption would provide valuable insights to policymakers in understanding the direct and indirect effects of these laws and for shaping the future alcohol control policies accordingly.

This paper contributes to the growing list of papers that are concerned with the responsiveness of alcohol consumption among young adults to alternative alcohol control policies. In particular, it is the first paper to investigate the effects of the FSP laws on underage alcohol consumption trends using confidential data from the National Longitudinal Survey of Youth, 1997 Cohort (NLSY97).[7] In order to estimate the impact of this laws on underage alcohol consumption, I exploit the substantial variation in the implementation of these laws across states. My empirical methodology is a diff-and-diff (DD) type approach in which the models are identified using within state variation in timing of policy adoption, controlling for differences across states that were not treated over the same time period. In addition to several individual and state level control variables, my empirical models also control for several state level alcohol control policies that were effective during the same time period that the FSP laws were introduced. These laws include BAC 0.08 law, restrictions on Sunday alcohol sales, vertical ID law, and social hosting law. Even after controlling for a variety of potentially confounding state policies that may affect alcohol consumption directly, the results from the DD type models show that the FSP laws significantly reduce underage drinking, including up to a 0.22 drink decrease in the average number of drinks consumed by underage youth per day. On average, underage individuals consume 0.54 drinks per day. Therefore, the estimated impact of the FSP law on average number of drinks consumed per day by underage youth is considerable and corresponds to an approximately 40 percent decrease from the mean (0.14 standard deviations). This result is also robust under alternative model specifications.

[5]Compared with the potential punishments that a retailer may face for inadvertently selling alcohol to minors, owning an electronic scanner is relatively cheap. An electronic scanner typically costs between $400 and $1,300 (www.idscanner.com).

[6]Several recent news from the popular press report that use of the ID scanners is on the rise. For instance, Irvine (2003) reports that ID scanners are gaining popularity with liquor retailers, police officers, and bar owners nationwide as fake IDs get ever-more sophisticated and difficult to spot. In Utah, the current liquor law explicitly requires retailers and bar owner to scan IDs of people who appear younger than 35. Information obtained through the scan is kept for seven days and law enforcement can inspect it in the event of a DUI or accident. In Pennsylvania, a recent proposal to privatize liquor sales by Majority Leader Mike Turzai mandates the use of ID scanners with age verification software and increases the fine for selling to minors to $10000. The proposal is available online at http://www.pahousegop.com/Display/SiteFiles/109/OtherDocuments/000_LCB_PrivatizationSummary_7_8_11.pdf.

[7] In contrast to the majority of the existing papers in the literature, this paper not only investigates the effectiveness of the FSP laws but also provides some evidence on the enforcement of such laws.

My results also imply that compared with those who are 18–20 year olds, the FSP laws are more effective in reducing alcohol consumption among 13–15 and 16–17 year olds. For these age groups, I find that these laws reduce the average number of drinks consumed per day by up to 0.25 and 0.19 drinks, respectively. Furthermore, alcohol consumption among non-college students is quite responsive to the FSP laws. These results imply that the effect of the FSP laws is strongest for underage individuals who are less likely to have access to a good fake ID (those who are younger than 18) and who are less likely to have easy access to alcohol in their general surroundings (non-college students).

I estimate the effect of the FSP laws on underage males and females separately and find that for males, the FSP law is associated with up to a 0.2 drink decrease in the average number of drinks consumed per day. I also extend the basic DD analysis and find that the impact of the FSP laws is concentrated in the short-run, which implies a learning behavior and shows that underage individuals substitute towards other methods of obtaining alcohol or can easily find alternative retailers that do not use electronic scanners in the long-run.

The rest of this paper is organized as follows. The next section provides a summary of the history of the FSP laws in the United States and discusses the relevant research. Section 3 presents the data and sets out the specifications for alternative empirical models. Section 4 presents the results and discusses the robustness of the main findings. Section 5 interprets the results, provides a discussion of policy implications, and concludes.

2 Background and literature review

Alcohol Policy Information System (APIS) provides the exact date of the introduction of the FSP laws for those states with a FSP law. In 1999, New York became the first state to pass a FSP law. Since then, 10 additional states have passed laws that provide incentives to retailers and bar owners who use electronic scanners to ensure that the customers is 21 years or older. The majority of these states have introduced their FSP laws during 2000–2005. Table 1 provides a list of the nine states with FSP laws during 1998–2005 and the date that the law is first introduced.[8] Two states, Utah and Nebraska, introduced their FSP laws in 2009 and 2010, respectively. Since these states did not have a FSP law before 2005, they are included in the empirical analysis as a part of the control group (states that did not have a FSP law during 1998–2005). Figure 1 shows the distribution of the states with the FSP laws across the country. While there are relatively few states with the FSP laws in the west and in the north, the geographical distribution of the states with the FSP laws across the country appears to be random.

This paper contributes to the existing literature which examines the effectiveness of various state level policies to reduce underage drinking. To my best knowledge, no previous study has examined the effect of the FSP laws on underage drinking. The majority of the existing studies investigates the impact of the MLDA on alcohol consumption among young adults.[9] For instance, Dee and Evans (2003) argue that teens who faced a lower MLDA were substantially more likely to drink. Carpenter et al. (2007) provide a historical comparative

[8] In my sample, only 1 respondent who resides in Arizona surveyed after 8/12/2005. Similarly, only 6 respondents who reside in Texas surveyed after 9/1/2005. Therefore, the main variation in the FSP laws comes from the remaining 7 states.

analysis of the effect of the MLDA laws on drinking behavior of high school seniors. They find that nationwide increases in the MLDA in the late 1970s and 1980s significantly reduced alcohol consumption by high school seniors.

Few other studies examine the impact of other state policies aimed at reducing teen drinking. Carpenter (2004) investigates the impact of zero tolerance (ZT) laws, which set the legal blood alcohol content limit for minors at low levels, and finds that these laws reduced heavy episodic drinking among underage males. Dee (1999) evaluates the policy responsiveness of teen drinking. He finds that cross-state heterogeneity can be important and that beer taxes have relatively small and statistically insignificant effects on teen drinking. Dills (2010) investigates the effect of state social host laws and finds that the introduction of these laws did not reduce underage drinking but reduced drunk-driving fatalities among 18-20 year olds. Recently, a number of states have begun issuing vertically designed (portrait-style) driver's license and state identification cards to individuals under the age of 21, while those above the age of 21 continue to receive the traditional horizontally shaped (landscape-style) cards. In a recent paper, Bellou and Bhatt (2013) investigate the impact of this policy and find that these laws decrease underage drinking and smoking for 16 years old in the short-run. Some of these state policies aimed at reducing teen drinking were introduced around the same time that the FSP laws were introduced. To address this potential problem, the empirical models in this paper explicitly control for the effect of state level heterogeneity, beer taxes, and several state level alcohol control policies, including vertical ID laws, social hosting laws, Sunday alcohol sales laws, and drunk driving laws. Finally, large scale public awareness campaigns about the dangers of teen drinking may also have significant effects on underage drinking. However, to my best knowledge, states that introduced the FSP laws did not engage in large public awareness campaigns about the dangers of teen drinking at the same time these laws were first introduced.

3 Data and methodology

I use data from the confidential version of the NLSY97 for the empirical analysis. The NLSY97 consists of a nationally representative sample of 9022 youths who were 12 to 16 years old as of December 31, 1996. Round 1 of the survey took place in 1997. In that round, both the eligible youth and one of that youth's parents received hour-long personal interviews. Since then, youths continue to be interviewed on an annual basis. In addition to standard demographic information, the survey respondents were also asked detailed questions about their alcohol consumption habits. The main outcome variables are derived from these questions. I present the description of these variables and their summary statistics in Appendix Table A1 for the full sample.

Since the FSP laws are designed to control underage drinking, I restrict my sample to those respondents who were younger than 21 as of the interview date. Due to the panel nature of the NLSY97, the respondents get older over time and after 2005, all respondents in the

---

[9]Several recent studies investigate the spillover effects of the MLDA on mortality, crime, education, psychological well-being, risky sexual behavior, smoking, and marijuana use. These studies include Carpenter and Dobkin (2009 and 2010), Carrell, Hoekstra, and West (2011), Ertan Yörük and Yörük (2012 and 2014), Crost and Guerrero (2012), Crost and Rees (2013), and Yörük and Ertan Yörük (2011 and 2013).

NLSY97 are older than 21. Therefore, my sample includes those respondents who are younger than 21 and were surveyed over the period 1998-2005. A unique feature of the confidential version of the NLSY97 is that it contains information on respondents' state of residence, exact date of birth, and exact interview date for each survey year. I use this information to determine whether the respondent was younger than 21 and her state enforces a FSP law as of the interview date.

In order investigate the impact of the FSP laws on alcohol consumption among underage individuals, I consider five alternative indicators of alcohol consumption. Two of these indicators are measures of drinking participation, i.e., whether the respondent consumed alcohol over the past month and whether the respondent engaged in heavy (binge) drinking in the past month.[10] Two of the remaining indicators are measures of drinking episodes per month, i.e., the number of days that the respondent had at least one drink and the number of days that she had five or more drinks on the same occasion during the past month. The remaining indicator of alcohol consumption measures the intensity of drinking as the average number of drinks that the respondent consumed per day during the past month.[11]

In order to estimate the impact of the FSP laws, I exploit the variation in the implementation of these laws across states and estimate DD type models for each alcohol consumption and alcohol consumption related outcome. Therefore, I compare relative changes in underage drinking habits and related outcomes between the states that passed a FSP law (treatment states) and states that did not (control states), before and after the adoption of FSP laws. Specifically, the main model that I estimate can be expressed as follows:

$$Y_{ismt}=\beta^{'}\mathbf{X}_{ismt}+\alpha^{'}\mathbf{S}_{smt}+\lambda treat_{ismt}+\mu_s+\gamma_m+\eta_t+(trend \times \mu_s)+e_{ismt}, \quad (1)$$

where *i* indexes individuals, *s* indexes states, *m* indexes months, and *t* indexes years. In this model, Y_{ismt} refers to the variety of alcohol consumption outcomes as discussed above. The vector $\mathbf{X}_{\text{ismt}}$ includes individual level characteristics such as gender, race, age, family size, and binary variables, controlling for different income ranges, marital status, employment status, educational attainment, and being a student.[12] The vector $\mathbf{S}_{\text{smt}}$ contains state level control variables such as unemployment rate, log of income per capita, state beer tax per gallon in 2005 prices, and dummy variables, controlling for various state policies on youth

[10]These binary variables are not observed directly. The respondents were asked the following questions: "During the last 30 days, on how many days did you have one or more drinks of an alcoholic beverage?" and "On how many days did you have five or more drinks on the same occasion during the past 30 days? By occasion we mean at the same time or within hours of each other". The alcohol participation variables for the corresponding questions are coded unity if the respondent reported consuming alcohol on at least one day during the past month and zero otherwise.

[11]The respondents were asked the following question: "In the past 30 days, on the days you drank alcohol, about how many drinks did you usually have?" In order to calculate the average number of drinks per day during a one month period, I multiply the number of days that the respondent drank alcohol with the average number of drinks that she had on those days and divide the result by 30.

[12] In order to control for the exact age in days, for each respondent, I calculate the difference in days between their interview date and the 21st birthday (MLDA) and used this variable and its quadratic term as an indicator of age. Using actual age in years instead of this alternative measure does not significantly affect the results. Binary variables controlling for educational attainment include four categories: less than high school, high school, two year college, and four year college. Binary variables controlling for different income ranges include eight categories: household income is less than $20,000, $20,000-$40,000, $40,000-$60,000, $60,000-$80,000, $80,000-$100,000, $100,000-$120,000, more than $120,000, and household income is missing. Race is controlled with dummies for being black and being Hispanic. Each outcome variable refers to the past month. Therefore, respondents who were interviewed within the first month after the policy change might have actually consumed alcohol before the policy change. To address this potential problem, the vector of individual level controls also contains a dummy variable that is equal to one for respondents who were interviewed within the first month after the policy change.

alcohol access such as presence of a BAC 0.08 law, Sunday alcohol sales, vertical ID law, and social hosting law. Drivers under age 21 are subject to tougher drunk driving laws, which are also known as "zero tolerance" (ZT) laws. In particular, it is illegal for drivers under age 21 to have measurable amounts of alcohol in their blood. In many states, BAC level is set to 0 or 0.02 for drivers under age 21. However, these laws are enacted between 1983 and 1998 and no state has changed its ZT law after July, 1998. The earliest interview date in my data set is October, 1998. Therefore, in my empirical analysis, state fixed effects should capture the potential impact of ZT laws on alcohol consumption and alcohol consumption related outcomes. Data for state level income per capita and unemployment rate come from Bureau of Labor Statistics (BLS). Data for state beer taxes come from Beer Institute's Brewer's Almanac (2012). I follow Yörük (2014) and use the information on state level alcohol policies as reported in the APIS to determine the exact date of the introduction of BAC 0.08 law, social hosting law, and Sunday alcohol sales for each state. I follow Bellou and Bhatt (2013) to identify whether a particular state enforces a vertical ID law at a given year. The state and year fixed effects are captured by μ_s and η_t, respectively. These variables control for potentially confounding influences that are time invariant within a state or are common to each survey year across states, respectively. Following Carpenter (2004), Equation (1) also includes dummy variables controlling for month fixed effects (γ_m) to account for the seasonality in drinking behavior. The main variable of interest, $treat_{ismt}$, equals one for those who reside in one of the states that enforced a FSP law as of the interview date. Models with binary dependent variables are estimated as linear probability models for ease of interpretation and in all regressions, standard errors are corrected for clustering at the state level (Bertrand et al., 2004).

The key identification assumption in Equation (1) is that, in the absence of the FSP laws, alcohol consumption among underage drinkers would have trended similarly between states which had enacted these laws and those which had not. One potential threat to this identification strategy is that the decision to pass a FSP law may reflect policy endogeneity. In particular, states that experienced relatively higher underage drinking rates might be more likely to adopt the FSP laws compared with those states with relatively lower underage drinking rates.

In Figure 2, I plot the annual alcohol consumption trends for the treatment and control states. This figure shows that compared with the control states, the treatment states were not experiencing high underage drinking rates. In fact, underage respondents residing in the control states tend to consume more alcohol compared with those who reside in the treatment states. For both the treatment and control states, alcohol consumption appears to be following a very similar and increasing trend until 2001. However, Average alcohol consumption in treatment states decreases from 2002 to 2004. Figures 3 and 4 present a more detailed analysis of alcohol consumption trends for alternative indicators of alcohol consumption. Figure 3 clearly shows that the FSP laws could not have been introduced as a reaction to increased underage drinking rates in the states that have passed these laws. As illustrated in this figure, trends in probability of consuming alcohol and engaging in binge drinking among underage individuals are fairly similar across states that passed FSP laws and those that did not prior to passage of these laws. Figure 4 presents a similar analysis for the number of drinking and binge drinking episodes per month, and confirms that prior to passage of the FSP laws, alcohol consumption trends among underage individuals were similar across the treatment and control states.[13] Furthermore, in order to address concerns of policy endogeneity formally, I extend the basic DD analysis in two ways. First, in certain models, I incorporate state-specific linear time trends ($trend \times \mu_s$) to Equation (1) by interacting a dummy for each state with a time trend that equals one in 1998, two in 1999, and so forth. These state-specific linear time trends control for a variety of other variables that are associated with underage drinking behavior but which trend smoothly within states over time. Inclusion of the state trends also reduces concerns for potential omitted variables bias. Second, following Gruber and Hungerman (2008), I consider models that contain a dummy for the two years before the effective date of the FSP law. If the treatment dummy is just picking up a pre-existing increasing trend in underage drinking rates at the treatment states, then this should be captured in this “lead” term.

Following Carpenter (2004), I also estimate separate models for males, females, and different age groups and extend the basic DD analysis to investigate the dynamic impact of the FSP laws on underage drinking behavior. In this model, I replace the treatment dummy with a series of time dummies for the years leading up to and after a state enforces a FSP law.

For all models, following Anderson, Hansen, and Rees (2012) who used the NLSY97 to investigate the effect of medical marijuana laws on drug use and alcohol consumption among young adults, I report unweighted DD estimates of the relationship between the FSP laws and the outcomes discussed above.[14] In a recent paper, Solon, Haider, and Wooldridge (2013) recommend reporting both the weighted and unweighted estimates because the contrast serves as a useful joint test against model misspecification and misunderstanding of the sampling process. I also present the results from the sample weighed DD models in Appendix Table A2 for the full sample and by gender. In general, results from the weighted and unweighted regressions are similar.

4 Results

4.1 Full sample

Table 2 presents DD estimates of the impact of the FSP laws on underage drinking behavior under several alternative specifications. The first specification excludes individual and state level covariates but controls for state, month, and year fixed effects. Estimates from this model suggest that the FSP law has a relatively small but statistically significant impact on several indicators of underage drinking behavior. In particular, the FSP law is associated with a 2.6 percentage point (0.05 standard deviations) decrease in the probability of

[13]Since FSP laws became effective at different times, Figures 2 and 3 are centered in the exact date the policy became effective in each of the treatment states (time 0) and plot alcohol consumption trends in the months leading up to and after this period for 20 months in 4-month blocks. For control states, average alcohol consumption trends during the same period are plotted. For instance, suppose that there are two treatment states with FSP laws (state A and B) and one control state that did not pass a FSP law during the analysis period (state C). Suppose further that FSP law became effective on 3/5/2005 in state A and on 12/11/2003 in state B. The average alcohol consumption at time 0 in state C is based on alcohol consumption rates in state C on 3/5/2005 and 12/11/2003.

[14] In technical sampling report of the NLSY97, Moore, et al. (2000) argue that using NLSY97 weights to perform weighted least squares when doing regression analysis may lead to incorrect estimates. In correctly specified models, using sample weights may also increase the variance of estimates and lead to loss of efficiency (DuMouchel and Duncan, 1983; Dickens, 1990).

engaging in binge drinking, a 0.2 day (0.04 standard deviations) decrease in the number of drinking days per month, and a 0.09 drink (0.06 standard deviations) decrease in the average number of drinks consumed per day. The results from the second specification show that these estimates are robust to the inclusion of individual and state level controls. Furthermore, the effect of the FSP law on the number of binge drinking days per month becomes significant once the control variables are included in the DD model. The third specification in Table 2 shows that inclusion of a lead term to regressions, which should pick up a potential pre-existing trend in underage drinking rates at the treatment states, does not have a considerable impact on the estimated effect of the FSP laws on several indicators of alcohol consumption among underage youth. Under this specification, the FSP law is associated with a 4.4 percentage point decrease in the probability of engaging in binge drinking and a 0.2 drink decrease in the average number of drinks consumed per day. The fourth specification in Table 2 reports the results from a general model, which also controls for the state-specific linear time trends. The results from this model show that the FSP law is associated with a 3.1 percentage point (0.06 standard deviations) decrease in the probability of engaging in binge drinking, a 0.52 day (0.1 standard deviations) decrease in the number of drinking days per month, a 0.41 day (0.12 standard deviations) decrease in the number of binge drinking days per month, and a 0.21 drink (0.14 standard deviations) decrease in the average number of drinks consumed per day.

Due to the longitudinal nature of data, it is also possible to control for individual fixed effects that control for time-invariant individual characteristics that may be associated with alcohol consumption. Although it is quite unlikely, if these characteristics are correlated with the introduction of the FSP laws, then the inclusion of the individual fixed effects may significantly affect the results. The results from the last four specifications reported in Table 2 show that this is not the case. The effect of the FSP law on several indicators of alcohol consumption among underage youth remains to be negative and statistically significant even after the individual fixed effects are controlled for. To further test the robustness of my estimates, in Table 3, I report the results from a falsification test. If the treatment dummy is just picking up pre-existing differences between treatment and control states, then one expects to observe a similar impact of the FSP law on those who are 21 years or older. However, Table 3 shows that as expected, FSP laws have no significant impact on alcohol consumption behavior of those young adults who have legal access to alcohol. This result further confirms that the significant decrease in several indicators of alcohol consumption among underage drinkers is due to the FSP laws.

The respondents of the NLSY97 get older over time and different treatment states adopted the FSP law at different times. Therefore, one cannot estimate the effect of the introduction of the FSP law in some treatment states on alcohol consumption behavior of certain age groups. For instance, West Virginia introduced the FSP law in 2003. However, the youngest respondent in the NLSY97 in 2003 was 18 years old. Hence, one can only use the variation from Virginia to estimate the effect of the FSP law on alcohol consumption of those who are 18 years old or older. Since the variation from different states can only be used to identify the impact of the FSP law on alcohol consumption behavior of certain age groups, the impact of the FSP law for the full sample may be driven by the alcohol consumption trends in certain treatment states. In order to test this possibility, I estimate several alternative models where I drop one, two, or three treatment states each time. In these models, I consider the average number of drinks consumed per day as the outcome variable and report the results in Table 4. The first specification is from Table 2 (specification 2). This model includes individual characteristics, state level control variables, and state, month, and year fixed effects. For the remaining specifications, I replicate this estimation by excluding the first state that introduced a FSP law (New York), first two states that introduced a FSP law (New York and Oregon), last two states that introduced a FSP law (Arizona and Texas), two random states (Connecticut and Ohio), and three random states (North Carolina, Pennsylvania, West Virginia). The effect of the FSP law on the average number of drinks consumed per day is negative and statistically significant under all specifications. This result shows that the estimated effect of the FSP law for the full sample is not driven by the alcohol consumption trends in certain treatment states.

4.2 Alternative samples

In Table 5, I investigate whether the effect of the FSP laws on alcohol consumption differs by gender. The results suggest that underage males are more responsive to FSP laws. For underage males, the FSP law is associated with a 3.4 to 4.1 percentage point (0.07 to 0.09 standard deviations) decrease in the probability of engaging in binge drinking, a 0.45 to 0.6 day (0.08 to 0.11 standard deviations) decrease in the number of drinking days, a 0.21 to 0.30 day (0.05 to 0.07 standard deviations) decrease in the number of binge drinking days, and a 0.13 to 0.20 drink (0.07 to 0.11 standard deviations) decrease in the average number of drinks consumed per day.

Table 6 reports the effect of the FSP laws on underage drinking behavior for different age groups.[15] For 13–15 year olds, the FSP law is associated with up to a 0.74 day (0.25 standard deviations) decrease in the number of drinking days per month and up to a 0.25 drink (0.27 standard deviations) decrease in the average number of drinks consumed per day. A similar impact of the FSP law is also observed for 16-17 year olds. The results from alternative models imply that for this age group, the FSP law is associated with up to a 0.44 day (0.11 standard deviations) decrease in the number of drinking days during the past month, and a 0.19 drink (0.15 standard deviations) decrease in the average number of drinks consumed per day. These estimates are highly significant and robust to the inclusion of individual fixed effects.

The effect of the FSP law on those who are relatively older (18–20 year olds) is quite limited. For this age group, the FSP law is associated with a 2.3 percent decrease in the probability of drinking and a 2.9 percentage point decrease in the probability of engaging in binge drinking. However, these estimates are not robust to the inclusion of individual fixed effects. One likely reason that the FSP law does not have a significant impact on the drinking behavior of this age group is that compared with those who are relatively younger, 18–20 year olds are more likely to know somebody who is 21 or older and therefore, can

[15]These results for different age groups should be interpreted with caution. Due to the panel structure of the NLSY97, the respondents get older over time. For instance, in 2002, every respondent of the NLSY97 is 18 years old or older. Therefore, when estimating the effect of the FSP laws on alcohol consumption patterns of 16-17 year olds, the main identification comes from five treatment states that introduced their FSP laws before 2002.

buy them alcohol. Compared with those who are relatively younger, retailers and bar owners may also be less likely to ask identification from 18–20 year olds since they may look older than 21. Another possibility is that students over 18 years old are in college where alcohol is easily accessible.[16] Therefore, the FSP law should have limited impact on underage college students. In order to test this hypothesis, I divide the sample of 18–20 year olds into two subsamples: those who are a college student and those who are not. Using DD models that contain individual characteristics, state level control variables, state, month, and year fixed effects, I estimate separate regressions for these two subsamples and report the results in Table 7. The results show that the FSP law significantly decreases several indicators of alcohol consumption among those who are not a college student. However, the effect of the FSP law on alcohol consumption among underage college students is statistically insignificant. This result provides further evidence that the FSP law does not have a significant impact on alcohol consumption among underage youth, who can access to alcohol through other means.

It is also plausible to argue that the enforcement of the FSP law would be significantly different in rural areas. In order to test this possibility, I estimate separate regressions for those who live in urban and rural areas. In general, I find that the estimated impact of the FSP law on those who live in rural areas is not significantly different than the estimated impact of the FSP law on those who live in urban areas. For instance, I find that the FSP law is associated with a 0.22 and 0.06 drink decrease in average number of drinks consumed by underage youth who live in rural and urban areas, respectively. However, this difference is not significant at conventional significance levels ($p - value = 0.187$).

4.3 Dynamic effects

In Table 8, I investigate dynamic responses in underage drinking behavior to adoption of FSP laws.[17] The estimates in this table are from a DD model which instead of the treatment dummy, contains binary indicators for the years leading up to and after a state introduces a FSP law. For the full sample, the coefficient estimates on years before the policy change are relatively small and not statistically significant at conventional significance levels, which imply that states that imposed a FSP law did not introduce this policy as a response to upsurges in teen alcohol consumption. On the other hand, Table 8 shows a significant drop in average alcohol consumption in the first year immediately following the policy adoption. Estimates for two to three years since adoption are uniformly negative, though not statistically significant. A similar result is also observed for underage males, whereas the short-run impact of the FSP laws of drinking behavior of underage females is negative but not statistically significant. In Figure 5, I plot the coefficient estimates from the dynamic DD analysis for alternative outcomes. In general, estimates for alternative outcomes show that the effect of the FSP law on alternative indicators of alcohol consumption one year before the policy change is quite small. The effect of the FSP law in the first year of the policy change is consistently negative. However, this effect appears to diminish after the second year of the policy adoption. One possible explanation for this result is that over time,

[16] I thank an anonymous referee for pointing this out.

[17] A similar analysis is also presented by Carpenter (2004), Bellou and Bhatt (2013), and Eisenberg (2003).

underage drinkers learn the new policy and find alternative ways to obtain alcohol such as purchasing it from retailers that do not use electronic scanners or asking an older adult to purchase it on their behalf.[18]

4.4 Spillover effects

Following Carpenter (2004), to provide further evidence on the plausibility of the my results, I investigate the effect of the FSP laws on beer consumption and the number of youth arrested for public drunkenness. Beer is the primary beverage of choice, particularly among youth (Carpenter, 2004). Therefore, considerable reductions in underage drinking due to FSP laws would lead to small reductions in overall beer consumption. To explore this hypothesis, I use data from The Beer Institute's Brewer's Almanac, which reports beer consumption per capita for each state from 2000 to 2010 and estimate a DD model of the effect of the FSP laws on the log of this variable. This model contains state and year fixed effects, as well as state level time variant characteristics such as the share of the population age 15—19, log of per capita income and beer tax per gallon in 2009 prices, unemployment rate, percent of female, black, white, and Hispanic population, poverty rate, percent of the population with a high school, college, and graduate degrees, dummy variables for the presence of a BAC 0.08 law and vertical ID law, and a dummy variable which is equal to one for the states with Democratic governors at a given year.[19] A primary shortcoming of beer consumption data is that they are only reported on an annual basis. Therefore, I code the FSP laws according to the fraction of the year they are in effect for each state, resulting in less precision. However, since I consider an extended time period, I was able to use the additional variation from the two late adopter states, i.e., Nebraska and Utah. The first column of Table 9 shows that overall beer consumption per capita decreases by 1.7% due to the FSP laws. Furthermore, this effect is highly significant. Figure 6 presents the coefficient estimates from a dynamic DD analysis for this outcome. As in other outcomes, the FSP law appears to have negative impact on beer consumption per capita during the first two years of the policy adoption. The effect of the FSP law on beer consumption starts to diminish after the third year of the policy change.

If FSP laws significantly reduce binge drinking among underage youth, one might expect to observe a decrease in the number of underage youth arrested for public drunkenness. In order to investigate the relationship between the FSP laws and the number of arrests for public drunkenness, I use data from FBI's National Incident-Based Reporting System (NIBRS). These data provide the most detailed information on various types of crime in the United States and include information on arrests for public drunkenness for 32 states for 2000-2010.[20] For this analysis, I consider arrests of underage youth and calculate the total number of arrests for public drunkenness for this group for each month.[21] I report the results

[18]Results from the dynamic DD analysis should be interpreted with caution due to the limitations of data. For instance, the only variation in the "4 plus years after" category comes from New York, Ohio, and Oregon (the first three states to adopt a FSP law).

[19]Data on share of population age 15–19, percent of female, white, black, and hispanic population, percent of population with a high school, college, and graduate degrees, and poverty rate come from the Census Bureau.

[20]NIBRS does not contain information on arrests for public drunkness for some states with FSP laws such as New York, North Carolina, Pennsylavinia, and Oregon.

[21]The total number of arrests for public drunkenness is not available for each month for each state during 2000–2010 since states adopted NIBRS gradually.

from a Poisson regression estimated with a full set of control variables, and state, year, and month fixed effects in the second column of Table 9. The estimated impact of the FSP laws on the number of underage youth arrested for public drunkenness is negative and highly significant. In particular, I find that the log count of the number of underage youth arrested for public drunkenness decreases by 0.22 points due to the FSP law. The estimates from a dynamic DD analysis presented in Figure 6 shows that the effect of the FSP law on the number of underage youth arrested for public drunkenness remains to be negative even in the long-run.

Another important policy question is whether the FSP law has any spillover effects on substance use among underage individuals.[22] In order to test the effect of the FSP laws on substance use, I consider four different outcomes. These are whether the respondent smoked over the past month, whether the respondent used marijuana over the past month, the number of days that the respondent smoked over the past month, and the number of days that the respondent used marijuana over the past month. Summary statistics for these variables are presented in Table A1. Results reported in Table 10 show that for most outcomes, the FSP law has a negative impact on substance use among underage youth. However, none of these estimates are significant at conventional significance levels. Therefore, the FSP law does not have a significant impact on substance use among underage individuals.

5 Conclusion

In this paper, I investigate the effect of the FSP laws on alternative indicators of alcohol consumption among underage youth using a restricted version of the NLSY97 which contains information on the state of residence and exact birth date of the respondents. To my best knowledge, this is the first paper to investigate the effects of these laws. Using DD type models and controlling for several other state level alcohol control policies, I document that the FSP laws significantly reduce underage drinking, including up to a 0.22 drink decrease in the average number of drinks consumed by underage individuals per day. Given that on average, underage individuals consume 0.54 drinks per day, this effect is considerable and corresponds to an approximately 40 percent decrease from the mean (0.14 standard deviations). Males, non-college students, and those who are relatively younger (13–15 year olds and 16–17 year olds) are more responsive to the FSP law. I also estimate dynamic DD models and find that the impact of the FSP laws is concentrated in the short-run, which implies that underage youth substitute towards other methods of obtaining alcohol in the long-run. My results also imply that the FSP law creates positive externalities and reduces the number of underage youth arrested for public drunkenness and overall beer consumption per capita. However, the FSP law does not have any significant impact on substance use among underage individuals.

The results of this paper are robust under several alternative model specifications. In particular, since the variation from different states can only be used to identify the impact of

[22]Several recent studies investigate the spillover effects of alcohol control policies on substance use among young adults. Examples include Crost and Rees (2013), Crost and Guerrero (2012), Yörük and Ertan Yörük (2011 and 2013).

the FSP law on alcohol consumption behavior of certain age groups due to the fact the respondents of the NLSY97 get older over time, the impact of the FSP law for the full sample may be driven by the alcohol consumption trends in certain treatment states. However, results from the robustness checks show that this is not the case.

Given the existing literature that relates alcohol consumption among minors to a long list of adverse social and economic outcomes such as mortality, crime, and academic performance, the positive effect of the FSP laws in reducing underage drinking would suggest that these laws could have a considerable positive impact on these outcomes. For instance, prior work has found that a 10% increase in drinking days increases the probability of arrest by about 1 percent (Carpenter and Dobkin, 2010). Therefore, given my estimate that the FSP law decreases drinking days by up to 0.52 days (a 19.1% reduction from the mean), the FSP law would also decrease the probability of arrest among minors up to 1.91%. Carpenter and Dobkin (2009) document that a 10% increase in the number of drinking days for young adults results in a 4.3% increase in mortality, primarily due to motor vehicle accidents, alcohol-related deaths, and suicides. This finding implies that FSP law would decrease alcohol related mortality among minors by up to 8.21%. In a recent paper, Balsa, Giuliano, and French (2011) examine the effects of alcohol use on high school students' quality of learning and find that an increase of one day per month in drinking frequency reduces GPA by 0.005 points. Hence, according to my estimates, the FSP law would lead to up to a 0.0026 point increase in average high school student's GPA. However, although these rough estimates provide some evidence on the positive impact of the FSP lawn on several social and economic outcomes, further research is needed to establish such relationships.

The results of this paper provide valuable insights to policymakers for shaping future alcohol control policies. For instance, in light of the evidence provided in this paper, a stricter false ID law, which would enforce the use of electronic scanners in all alcohol sales may significantly reduce alcohol consumption among minors and make the impact of the FSP laws sustainable in the long-run. The FSP laws may also be extended to cover cigarette sales to reduce smoking prevalence among youth.

References

1. Alcohol Policy Information System (APIS). National Institute on Alcohol Abuse and Alcoholism; available at http://alcoholpolicy.niaaa.nih.gov
2. Anderson, DM.; Hansen, B.; Rees, DI. Medical marijuana laws and teen marijuana use. 2012. Unpublished manuscript
3. Balsa AI, Giuliano LM, French MT. The effects of alcohol use on academic achievement in high school. Economics of Education Review. 30:1–15. [PubMed: 21278841]
4. Bellou A, Bhatt R. Reducing Underage Alcohol & Tobacco Use: Evidence from the Introduction of Vertical Identification Cards. Journal of Health Economics. 2013; 32:353–366. [PubMed: 23333955]
5. Bertrand M, Duflo E, Mullainathan S. How much should we trust in differences-in-differences estimates? Quarterly Journal of Economics. 2004; 119:249–276.
6. Brewer's Almanac. 2012. available at http://www.beerinstitute.org/statistics.asp?bid=200
7. Carrell SE, Hoekstra M, West JE. Does drinking impair college performance? Evidence from a regression discontinuity approach. Journal of Public Economics. 2011; 95:54–62.

8. Carpenter C. How do zero tolerance drunk driving laws work? Journal of Health Economics. 2004; 23:61–83. [PubMed: 15154688]
9. Carpenter C, Kloska DD, O'Malley P, Johnston L. Alcohol Control Policies and Youth Alcohol Consumption: Evidence from 28 Years of Monitoring the Future. The BE Journal of Economic Analysis & Policy (Topics). 2007; 7:article 25.
10. Carpenter C, Dobkin C. The effect of alcohol consumption on mortality: Regression discontinuity evidence from the minimum drinking age. American Economic Journal: Applied Economics. 2009; 1:164–182. [PubMed: 20351794]
11. Carpenter, C.; Dobkin, C. The drinking age, alcohol consumption, and crime. 2010. Unpublished Manuscript
12. Centers for Disease Control and Prevention. Alcohol Related Disease Impact (ARDI) application. 2008. available at http://apps.nccd.cdc.gov/DACH_ARDI/Default.aspx
13. Center for Behavioral Health Statistics and Quality. Findings on drug-related emergency department visits. Substance Abuse and Mental Health Services Administration; Rockville, MD: 2012. The DAWN report: Highlights of the 2010 drug abuse warning network (DAWN).
14. Century Council. TRU, Lifestyles and Marketing Study Wave 42. 2003. available at http://www.centurycouncil.org/underage-drinking/what-youth-say-about-alcohol
15. Crost B, Guerrero S. The effect of alcohol availability on marijuana use: Evidence from minimum legal drinking age. Journal of Health Economics. 2012; 31:112–121. [PubMed: 22381404]
16. Crost B, Rees D. The minimum legal drinking age and marijuana use: New estimates from the NLSY97. Journal of Health Economics. 2013; 32:474–476. [PubMed: 23199608]
17. Dee TS. State alcohol policies, teen drinking and traffic fatalities. Journal of Public Economics. 1999; 72:289–315.
18. Dee TS, Evans WN. Teen drinking and educational attainment: Evidence from two-sample instrumental variables estimates. Journal of Labor Economics. 2003; 21:178–209.
19. Dickens WT. Error components in grouped data: Is it ever worth weighting? Review of Economics and Statistics. 1990; 72:328–333.
20. Dills A. Social host liability for minors and underage drunk-driving accidents. Journal of Health Economics. 2010; 29:241–249. [PubMed: 20080308]
21. DuMouchel WH, Duncan GJ. Using sample survey weights in multiple regression analyses of stratified samples. Journal of the American Statistical Association. 1983; 78:535–543.
22. Eisenberg D. Evaluating the effectiveness of policies related to drunk driving. Journal of Policy Analysis and Management. 2003; 22:249–274.
23. Ertan Yörük C, Yörük BK. The impact of drinking on psychological well-being: Evidence from minimum drinking age laws in the United States. Social Science and Medicine. 2012; 75:1844–1854. [PubMed: 22884946]
24. Ertan Yörük, C.; Yörük, BK. Alcohol consumption and risky sexual behavior among young adults: Evidence from minimum legal drinking age laws. 2014. Unpublished Manuscript
25. Gruber J, Hungerman D. The church vs. the mall: What happens when religion faces increased secular competition? Quarterly Journal of Economics. 2008; 123:831–862.
26. Hingson RW, Heeren T, Winter MR. Age at drinking onset and alcohol dependence: Age at onset, duration, and severity. Pediatrics. 2006; 160:739–746.
27. Irvine, M. Underage drinkers, beware: Fake ID scanners on the rise. Associated Press; 2003. available at http://onlineathens.com/stories/101303/tec_20031013012.shtml
28. Lindo JM, Swensen ID, Waddell GR. Alcohol and student performance: Estimating the effect of legal access. Journal of Health Economics. 2013; 32:22–32. [PubMed: 23202254]
29. Moore, W.; Pedlow, S.; Krishnamurty, P.; Wolter, K. National Longitudinal Survey of Youth 1997 (NLSY97) Technical Sampling Report. Chicago: NORC; 2000.
30. National Research Council and Institute of Medicine. Committee on Developing a Strategy to Reduce and Prevent Underage Drinking. Division of Behavioral and Social Sciences and Education, The National Academies Press; Washington, DC: 2004. Reducing underage drinking: A collective responsibility.

31. Office of Juvenile Justice and Delinquency Prevention. Drinking in America: Myths, realities, and prevention policy. Washington, DC: 2005.

32. Preusser DF, Williams AF, Weinstein HB. Policing underage alcohol sales. Journal of Safety Research. 1994; 25:127–133.

33. Renna F. Alcohol abuse, alcoholism, and labor market outcomes: Looking for the missing link. Industrial and Labor Relations Review. 2008; 62:92–103.

34. Solon, G.; Haider, SJ.; Wooldridge, J. NBER Working Paper No. 18859. 2013. What are we weighting for?.

35. Waddell GR. Gender and the influence of peer alcohol consumption on adolescent sexual activity. Economic Inquiry. 2012; 50:248–263. [PubMed: 22329054]

36. Yörük BK, Ertan Yörük C. The impact of minimum legal drinking age laws on alcohol consumption, smoking, and marijuana use: Evidence from a regression discontinuity design using exact date of birth. Journal of Health Economics. 2011; 30:740–752. [PubMed: 21719131]

37. Yörük BK, Ertan Yörük C. The impact of minimum legal drinking age laws on alcohol consumption, smoking, and marijuana use revisited. Journal of Health Economics. 2013; 32:477–479. [PubMed: 23092933]

38. Yörük BK. Legalization of Sunday alcohol sales and alcohol consumption in the United States. Addiction. 2014; 109:55–61. [PubMed: 24103041]

Appendix

Table A1

Description of outcome variables and summary statistics

Outcome	N	Mean	S.D.	Description
Consumed alcohol	40315	0.477	0.499	=1 if the respondent consumed alcohol at least once in the past month, =0 otherwise.
Engaged in binge drinking	40249	0.276	0.447	=1 if the respondent consumed 5 or more drinks at least in one day in the past month, =0 otherwise.
No. of days consumed alcohol	40315	2.731	5.021	Number of days that the respondent consumed alcohol in the past month.
No. of days engaged in binge drinking	40249	1.283	3.308	Number of days that the respondent consumed 5 or more drinks in the past month.
Avg. no. of drinks consumed	40034	0.542	1.553	Average number of drinks consumed per day calculated as “No. of days consumed alcohol” multiplied by the number of drinks that the respondent consumed on the days she actually consumed alcohol and divided by 30.
Smoked	40349	0.345	0.475	=1 if the respondent smoked at least once in the past month, =0 otherwise.
No. of days smoked	40349	7.027	11.927	Number of days that the respondent smoked in the past month.
Used marijuana	40308	0.181	0.385	=1 if the respondent used marijuana at least once in the past month, =0 otherwise.
No. of days used marijuana	40308	1.997	6.334	Number of days that the respondent used marijuana in the past month.

Notes: Sample weighted means of the outcome variables are reported. S.D.: Standard deviation.

NIH-PA Author Manuscript

NIH-PA Author Manuscript

Table A2

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking: Sample weighted regressions

	Full sample		Male		Female	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Consumed alcohol						
Treat	−0.012 (0.038)	0.007 (0.040)	0.006 (0.030)	0.006 (0.027)	−0.029 (0.062)	0.013 (0.059)
R^2	0.107	0.094	0.111	0.112	0.123	0.091
N	39820	39820	20187	20187	19633	19633
Engaged in binge drinking						
Treat	−0.048** (0.023)	−0.036 (0.024)	−0.010 (0.035)	−0.022 (0.030)	−0.091** (0.040)	−0.055 (0.035)
R^2	0.094	0.076	0.104	0.107	0.084	0.062
N	39754	39754	20141	20141	19613	19613
No. of days consumed alcohol						
Treat	−0.681** (0.278)	−0.555* (0.315)	−0.677* (0.375)	−0.872** (0.369)	−0.731** (0.334)	−0.264 (0.354)
R^2	0.087	0.079	0.089	0.096	0.091	0.078
N	39820	39820	20187	20187	19633	19633
No. of days engaged in binge drinking						
Treat	−0.468*** (0.141)	−0.378*** (0.138)	−0.258 (0.299)	−0.273 (0.255)	−0.722*** (0.174)	−0.517*** (0.162)
R^2	0.072	0.055	0.075	0.076	0.061	0.048
N	39754	39754	20141	20141	19613	19613
Avg. no. of drinks consumed						
Treat	−0.246*** (0.078)	−0.220*** (0.082)	−0.154 (0.146)	−0.188 (0.115)	−0.356*** (0.075)	−0.265** (0.112)
R^2	0.054	0.035	0.055	0.051	0.044	0.030
N	39542	39542	19988	19988	19554	19554
Individual fixed effects	No	Yes	No	Yes	No	Yes

Notes: All regressions contain individual and state level controls, state, month, and year fixed effects, lead term, and state-specific linear time trend. Specifications 2, 4, and 6 add individual fixed effects. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs *, **, and *** denote statistical significance at 10, 5, and 1 percent significance level.

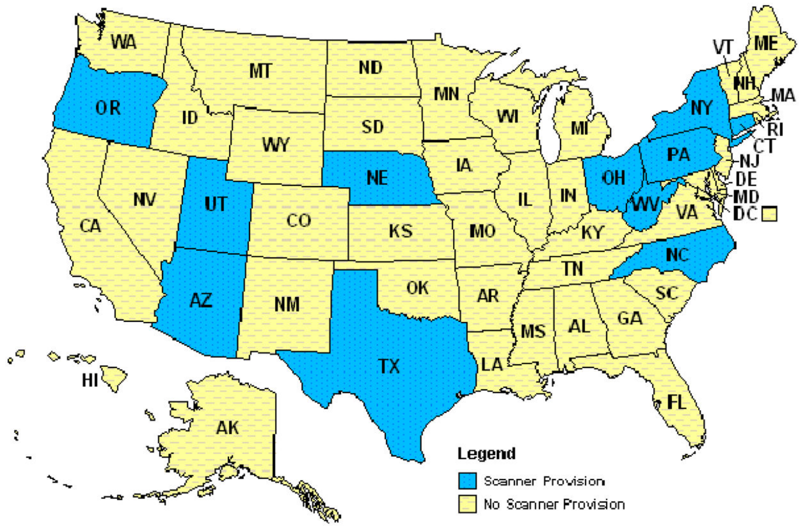

Figure 1.

False ID laws with scanner provisions as of January 1, 2012

Source: Alcohol Policy Information System (APIS), National Institute on Alcohol Abuse and Alcoholism (NIAAA). States with false ID laws introduced these laws at different dates. Table 1 provides a complete list of the "law effective date" for each state with a false ID law.

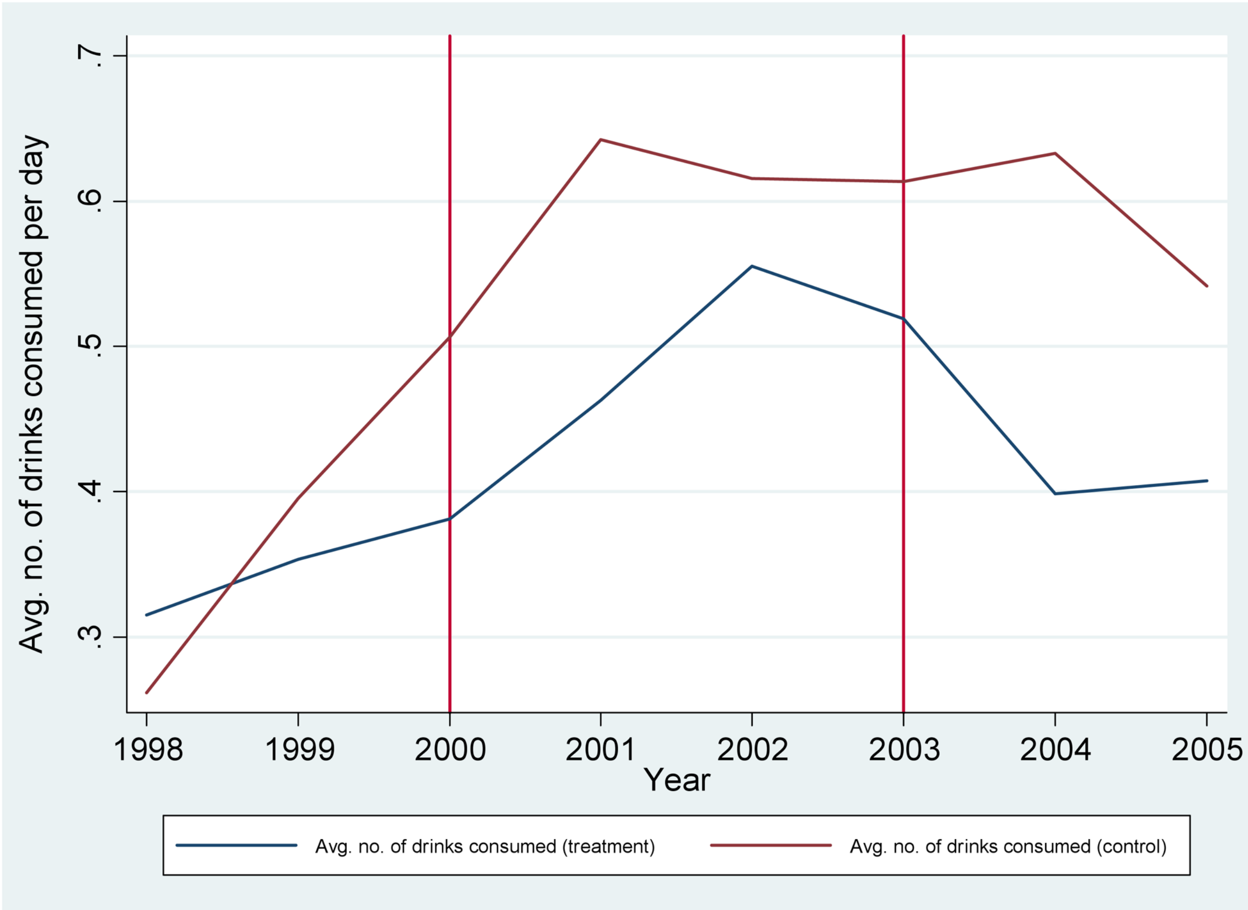

Figure 2.
Alcohol consumption trends in treatment and control states over time:Average number of drinks consumed per day
Notes: Mean alcohol consumption trends in treatment and control states over time are plotted.

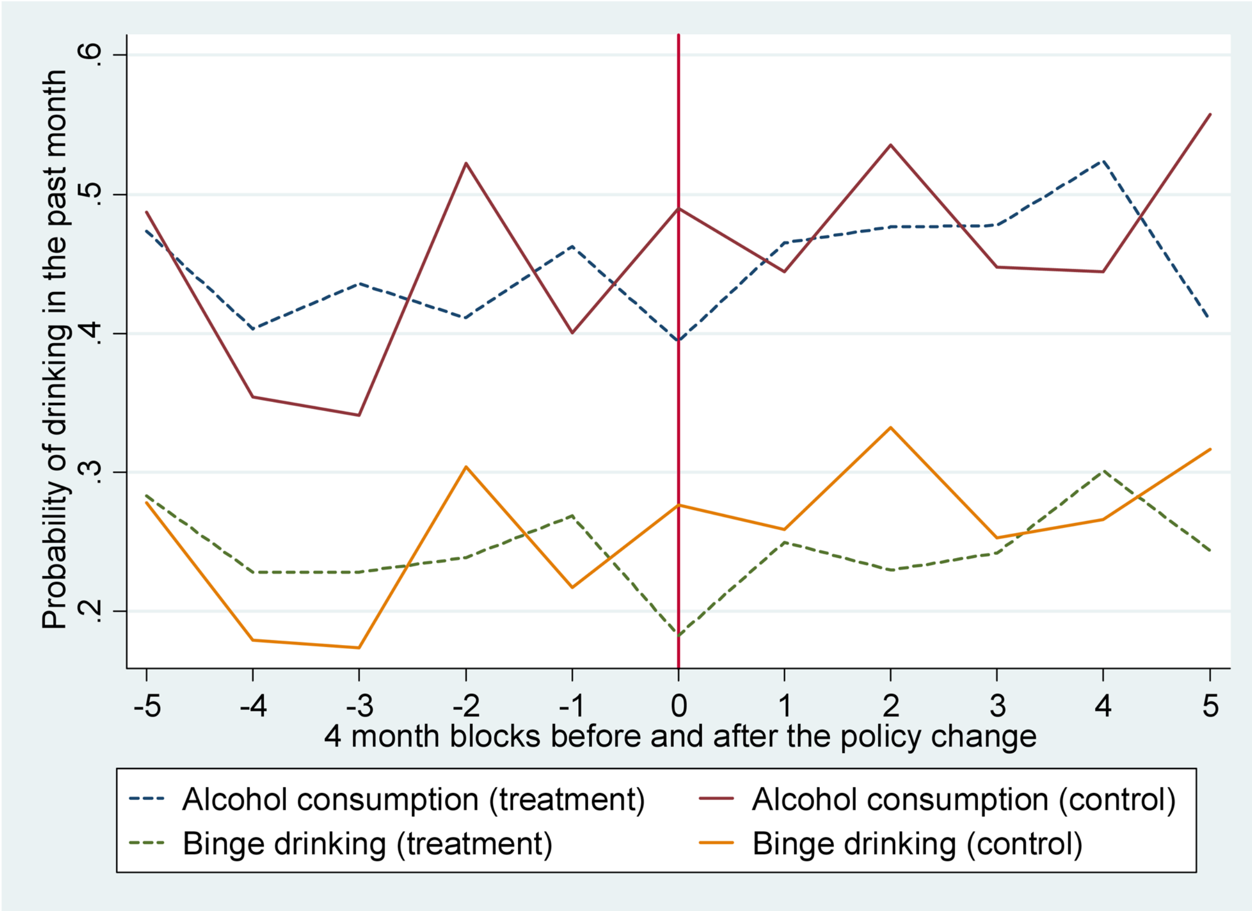

Figure 3.

Alcohol consumption trends in the treatment and control states before and after the policy change: Probability of consuming alcohol and engaging in binge drinking

Notes: Mean alcohol consumption trends in treatment and control states 20 months before and after the policy change are plotted.

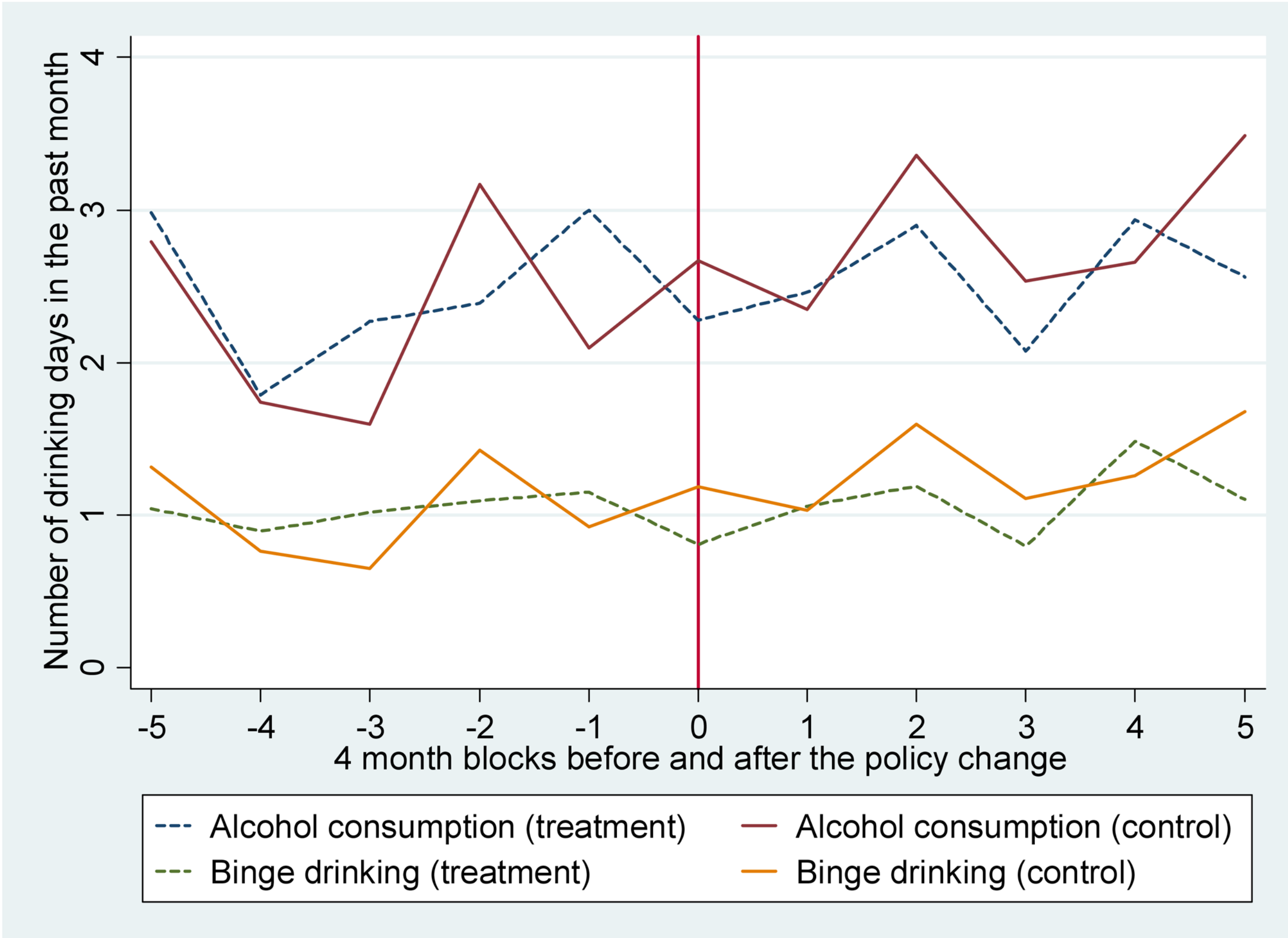

Figure 4.
Alcohol consumption trends in the treatment and control states before and after the policy change: Number of alcohol consumption and binge drinking days in the past month
Notes: Mean alcohol consumption trends in treatment and control states 20 months before and after the policy change are plotted.

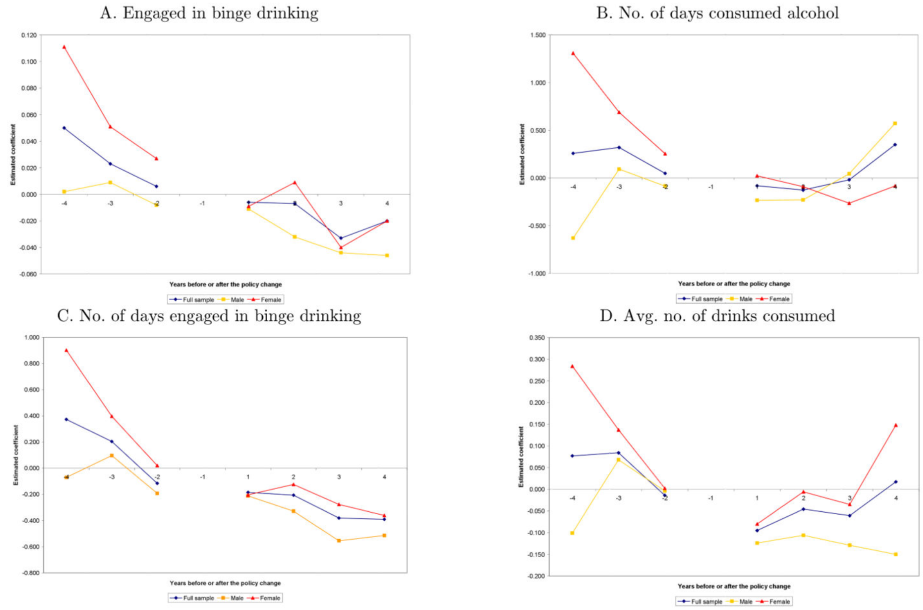

Figure 5.

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking: Dynamic responses for alternative indicators of alcohol consumption

Notes: Coefficient estimates of the effect of the FSP law on alternative outcome variables in dynamic diff-and-diff models are plotted. In all models, omitted category is one year before the policy change. All regressions include individual characteristics, state level controls, state, month, and year fixed effects, and state-specific linear time trends.

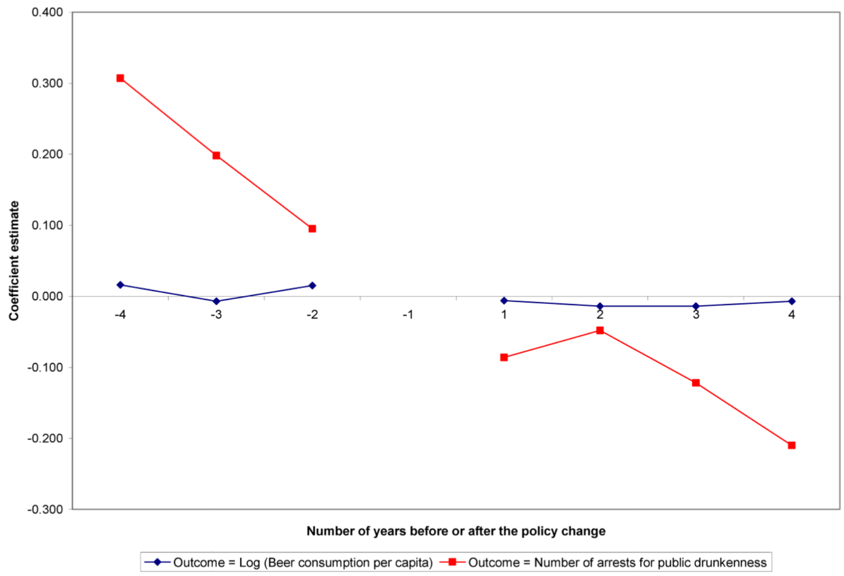

Figure 6.

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on overall beer consumption per capita and the number youth arrested for public drunkenness: Dynamic responses

Notes: Coefficient estimates of the effect of the FSP law on alternative outcome variables in dynamic diff-and-diff models are plotted. In all models, omitted category is one year before the policy change. All regressions include state level controls and state and year fixed effects. Month fixed effects are also controlled for when estimating the relationship between the FSP law and the number of youth arrested for public drunkenness.

Table 1

False ID laws with scanner provisions: 1998–2005

State	Law effective date
Arizona	8/12/2005
Connecticut	10/1/2001
New York	9/1/1999
North Carolina	11/14/2001
Ohio	9/21/2000
Oregon	1/1/2000
Pennsylvania	12/16/2002
Texas	9/1/2005
West Virginia	6/6/2003

Notes: Nebraska passed a false ID law with scanner provision on 7/15/2010. Utah passed a false ID law with scanner provision on 7/1/2009.
Source: Alcohol Policy Information System (APIS), National Institute on Alcohol Abuse and Alcoholism (NIAAA).

NIH-PA Author Manuscript
NIH-PA Author Manuscript

Table 2

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking: Full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Consumed alcohol (M=0.477, S.D.=0.499)								
TREAT	−0.008 (0.010)	−0.004 (0.013)	−0.008 (0.015)	0.013 (0.035)	0.000 (0.011)	0.002 (0.013)	0.006 (0.019)	0.022 (0.032)
R^2	0.040	0.106	0.106	0.108	0.069	0.078	0.078	0.082
N	40076	39820	39820	39820	40076	39820	39820	39820
Engaged in binge drinking (M=0.277, S.D.=0.477)								
TREAT	−0.026*** (0.010)	−0.023** (0.011)	−0.044*** (0.012)	−0.031* (0.017)	−0.026*** (0.008)	−0.024*** (0.009)	−0.037*** (0.013)	−0.027 (0.018)
R^2	0.032	0.096	0.096	0.098	0.051	0.061	0.061	0.064
N	40009	39754	39754	39754	40009	39754	39754	39754
No. of days consumed alcohol (M=2.730, S.D.=5.016)								
TREAT	−0.198* (0.112)	−0.182** (0.089)	−0.417** (0.141)	−0.522*** (0.190)	−0.229*	−0.264** (0.111)	−0.443*** (0.150)	−0.482** (0.222)
R^2	0.026	0.083	0.084	0.085	0.055	0.065	0.065	0.067
N	40076	39820	39820	39820	40076	39820	39820	39820
No. of days engaged in binge drinking (M=1.282, S.D.=3.302)								
TREAT	−0.142 (0.087)	−0.125* (0.063)	−0.371*** (0.0.94)	−0.408*** (0.088)	−0.149** (0.072)	−0.154** (0.068)	−0.346*** (0.094)	−0.349*** (0.085)
R^2	0.020	0.071	0.071	0.073	0.037	0.044	0.044	0.048
N	40009	39754	39754	39754	40009	39754	39754	39754
Avg. no. of drinks consumed (M=0.541, S.D.=1.547)								
TREAT	−0.093** (0.040)	−0.108*** (0.034)	−0.203*** (0.056)	−0.210*** (0.044)	−0.128*** (0.041)	−0.149*** (0.035)	−0.221*** (0.048)	−0.183*** (0.049)
R^2	0.013	0.051	0.051	0.052	0.022	0.027	0.027	0.029
N	39795	39542	39542	39542	39795	39542	39542	39542
Individual fixed effects	No	No	No	No	Yes	Yes	Yes	Yes
Control variables	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Lead term	No	No	Yes	Yes	No	No	Yes	Yes
State-specific linear time trend	No	No	No	Yes	No	No	No	Yes

NIH-PA Author Manuscript
NIH-PA Author Manuscript

Notes: All regressions include state, month, and year fixed effects. Control variables include individual characteristics and state level controls as discussed in the text. M: Sample weighted mean of the outcome variable for specifications 2–4 and 6–8. S.D.: Standard deviation of the outcome variable for specifications 2-4 and 6–8. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs *, **, and *** denote statistical significance at the 10, 5, and 1 percent significance levels.

Table 3

Falsification test: Diff-and-Diff Estimates of the effect of the false ID laws with scanner provisions on alcohol consumption of young adults who are 21 years or older

	21–22 year olds		21–26 year olds	
	(1)	**(2)**	**(3)**	**(4)**
Consumed alcohol				
Treat	−0.025 (0.029)	0.016 (0.026)	0.010 (0.039)	0.021 (0.024)
R^2	0.123	0.024	0.117	0.014
N	13237	13237	21599	21599
Engaged in binge drinking				
Treat	−0.005 (0.029)	0.024 (0.021)	−0.004 (0.027)	−0.008 (0.011)
R^2	0.129	0.026	0.125	0.015
N	13166	13166	21435	21435
No. of days consumed alcohol				
Treat	−0.631 (0.391)	−0.371 (0.650)	−0.229 (0.540)	0.085 (0.400)
R^2	0.104	0.027	0.090	0.013
N	13237	13237	21599	21599
No. of days engaged in binge drinking				
Treat	−0.325 (0.253)	0.019 (0.259)	−0.173 (0.211)	0.007 (0.155)
R^2	0.098	0.031	0.087	0.015
N	13166	13166	21435	21435
Avg. no. of drinks consumed				
Treat	−0.097 (0.107)	−0.011 (0.080)	−0.063 (0.119)	0.104 (0.088)
R^2	0.070	0.025	0.057	0.014
N	13103	13103	21351	21351
Individual fixed effects	No	Yes	No	Yes

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects, and state-specific linear time trend. Specifications 2 and 4 add individual fixed effects. Standard errors, corrected for clustering at the state level, are reported in parentheses.

NIH-PA Author Manuscript

NIH-PA Author Manuscript

Table 4

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on the average number of drinks consumed per day: Robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
Treat	−0.108 (0.034)***	−0.090 (0.042)**	−0.074 (0.038)*	−0.109 (0.033)***	−0.126 (0.040)***	−0.127 (0.036)***
R^2	0.051	0.052	0.052	0.053	0.052	0.051
N	39542	36873	36549	35059	38055	36381
Excluded treatment states		NY	NY	AZ	CT	NC
			OR	TX	OH	PA WV

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs *, **, and *** denote statistical significance at the 10, 5, and 1 percent significance levels.

Table 5

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking by gender

	MALE		FEMALE	
	(1)	**(2)**	**(3)**	**(4)**
Consumed alcohol				
TREAT	−0.015 (0.015)	−0.003 (0.016)	0.008 (0.015)	0.004 (0.016)
R^2	0.110	0.096	0.116	0.069
N	20187	20187	19633	19633
M (S.D.)	0.487 (0.499)		0.468 (0.499)	
Engaged in binge drinking				
TREAT	−0.034*** (0.010)	−0.041*** (0.011)	−0.011 (0.017)	−0.009 (0.015)
R^2	0.104	0.088	0.082	0.044
N	20141	20141	19613	19613
M (S.D.)	0.319 (0.466)		0.232 (0.422)	
No. of days consumed alcohol				
TREAT	−0.454*** (0.144)	−0.598*** (0.193)	0.100 (0.111)	0.059 (0.118)
R^2	0.084	0.080	0.084	0.059
N	20187	20187	19633	19633
M (S.D.)	3.157 (5.574)		2.289 (4.322)	
No. of days engaged in binge drinking				
TREAT	−0.205** (0.088)	−0.303*** (0.101)	−0.044 (0.077)	−0.018 (0.054)
R^2	0.072	0.060	0.058	0.034
N	20141	20141	19613	19613
M (S.D.)	1.660 (3.863)		0.891 (2.542)	
Avg. no. of drinks consumed				
TREAT	−0.132*** (0.041)	−0.203*** (0.045)	−0.084 (0.051)	−0.100* (0.058)
R^2	0.051	0.039	0.039	0.021
N	19988	19988	19554	19554
M (S.D.)	0.718 (1.889)		0.360 (1.060)	
Individual Fixed Effects	No	Yes	No	Yes

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects. Specifications 2 and 4 add individual fixed effects. M: Sample weighted mean of the outcome variable. S.D.: Standard deviation of the outcome variable. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs *, **, and *** denote statistical significance at the 10, 5, and 1 percent significance levels.

NIH-PA Author Manuscript

NIH-PA Author Manuscript

Table 6

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking by age groups

	13–15 year olds		16–17 year olds		18–20 year olds	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Consumed alcohol						
Treat	−0.003 (0.032)	−0.027 (0.031)	0.010 (0.022)	0.028 (0.028)	−0.023** (0.009)	−0.003 (0.023)
R^2	0.055	0.062	0.056	0.037	0.088	0.023
N	5156	5156	11502	11502	23162	23162
M (S.D.)	0.277 (0.448)		0.392 (0.488)		0.564 (0.496)	
Engaged in binge drinking						
Treat	0.012 (0.019)	−0.001 (0.025)	−0.021 (0.013)	−0.060*** (0.014)	−0.029* (0.016)	−0.007 (0.020)
R^2	0.045	0.060	0.057	0.034	0.088	0.018
N	5149	5149	11487	11487	23118	23118
M (S.D.)	0.114 (0.318)		0.215 (0.411)		0.343 (0.475)	
No. of days consumed alcohol						
Treat	−0.387*** (0.134)	−0.736*** (0.147)	−0.336** (0.156)	−0.439*** (0.107)	−0.117 (0.139)	−0.127 (0.171)
R^2	0.042	0.064	0.036	0.019	0.067	0.018
N	5156	5156	11502	11502	23162	23162
M (S.D.)	1.037 (2.925)		1.806 (3.850)		3.564 (5.676)	
No. of days engaged in binge drinking						
Treat	−0.089 (0.088)	−0.210 (0.120)	−0.103 (0.170)	−0.198 (0.128)	−0.125 (0.119)	−0.020 (0.104)
R^2	0.036	0.070	0.041	0.018	0.065	0.013
N	5149	5149	11487	11487	23118	23118
M (S.D.)	0.353 (1.544)		0.821 (2.371)		1.717 (3.864)	
Avg. no. of drinks consumed						
Treat	−0.136*** (0.031)	−0.246** (0.034)	−0.147** (0.063)	−0.185*** (0.050)	−0.071 (0.054)	−0.112 (0.066)
R^2	0.029	0.035	0.024	0.011	0.051	0.011
N	5137	5137	11435	11435	22970	22970
M (S.D.)	0.181 (0.897)		0.366 (1.289)		0.709 (1.744)	

	13–15 year olds		16–17 year olds		18–20 year olds	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Individual fixed effects	No	Yes	No	Yes	No	Yes

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects. Specifications 2, 4, and 6 add individual fixed effects. M: Sample weighted mean of the outcome variable. S.D.: Standard deviation of the outcome variable. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs *, **, and *** denote statistical significance at the 10, 5, and 1 percent significance levels.

Table 7

Diff-and-Diff estimates of the effect of the false ID laws with scanner provision on underage drinking: 18–20 year olds

	College student	
	Yes	**No**
Consumed alcohol		
Treat	0.003 (0.023)	−0.045 (0.015)***
R^2	0.100	0.082
N	8036	15126
Engaged in binge drinking		
Treat	0.009 (0.020)	−0.057 (0.025)**
R^2	0.114	0.085
N	8030	15088
No. of days consumed alcohol		
Treat	0.391 (0.356)	−0.475 (0.188)**
R^2	0.084 0.070	
N	8036	15126
No. of days engaged in binge drinking		
Treat	0.148 (0.199)	−0.321** (0.152)
R^2	0.085	0.067
N	8030	15088
Avg. no. of drinks consumed		
Treat	0.054 (0.087)	−0.132 (0.088)
R^2	0.072	0.053
N	7996	14974

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs ** and *** denote statistical significance at the 5 and 1 percent significance levels.

Table 8

Diff-and-Diff estimates of the effect of the false ID laws with scanner provisions on underage drinking: Dynamic responses

	Avg. no. of drinks consumed		
	Full sample	**Male**	**Female**
4+ years before	0.077 (0.111)	−0.101 (0.267)	0.284*** (0.070)
3 years before	0.084 (0.052)	0.068 (0.108)	0.137*** (0.048)
2 years before	−0.014 (0.051)	−0.004 (0.101)	0.002 (0.036)
1 year before (omitted)			
1 year after	−0.095*** (0.035)	−0.124* (0.066)	−0.080 (0.053)
2 years after	−0.046 (0.068)	−0.106 (0.131)	−0.006 (0.042)
3 years after	−0.061 (0.081)	−0.130 (0.150)	−0.035 (0.045)
4+ years after	0.017 (0.139)	−0.150 (0.259)	0.148 (0.110)
R^2	0.052	0.053	0.042
N	39542	19988	19554

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects, and state-specific linear time trends. Standard errors, corrected for clustering at the state level, are reported in parentheses. The signs * and *** denote statistical significance at 10 and 1 percent significance levels, respectively.

Table 9

The spillover effects of the false ID laws with scanner provisions on overall beer consumption per capita and the number youth arrested for public drunkenness

	Log (Beer consumption per capita)	**Number of arrests for public drunkenness**
Treat	−0.017 (0.007)***	−0.219 (0.074)***
R^2	0.981	
Pseudo R^2		0.645
N	561	2462

Notes: The first column reports the estimated effect of the FSP law on log(beer consumption per capita) from an OLS regression. The second column reports the estimated effect of the FSP law on the number of arrests for public drunkenness from a Poisson regression. Both models contain state and year fixed effects and a set of state-level control variables as discussed in the text. The second model also controls for month fixed effects. Standard errors, corrected for clustering at the state-year level, are reported in parentheses. The sign *** denotes statistical significance at the 1 percent significance level.

Table 10

Diff-and-Diff estimates of the spillover effects of the false ID laws with scanner provisions on underage smoking and marijuana use

	Full sample	Male	Female
Smoked			
Treat	−0.007 (0.015)	−0.004 (0.022)	−0.009 (0.022)
R^2	0.105	0.113	0.116
N	39850	20219	19631
M	0.344	0.354	0.334
S.D.	(0.475)	(0.478)	(0.472)
No. of days smoked			
Treat	−0.053 (0.393)	−0.014 (0.495)	−0.020 (0.458)
R^2	0.152	0.161	0.159
N	39850	20129	19631
M	7.004	7.216	6.785
S.D.	(11.914)	(12.025)	(11.794)
Used marijuana			
Treat	−0.002 (0.014)	−0.004 (0.021)	−0.001 (0.017)
R^2	0.043	0.045	0.048
N	39810	20174	19636
M	0.181	0.203	0.158
S.D.	(0.385)	(0.402)	(0.365)
No. of days used marijuana			
Treat	−0.180 (0.242)	−0.344 (0.388)	0.008 (0.190)
R^2	0.045	0.050	0.037
N	39810	20174	19636
M	1.993	2.564	1.404
S.D.	(6.323)	(7.257)	(5.120)

Notes: All regressions include individual characteristics, state level controls, state, month, and year fixed effects, and state-specific linear time trend. M: Sample weighted mean of the outcome variable. S.D.: Standard deviation of the outcome variable. Standard errors, corrected for clustering at the state level, are reported in parentheses.