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Can technology really help to reduce underage drinking? New evidence on the effects of false ID laws with scanner provisions☆

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ABSTRACT

In Volume 36 of this journal, Yoruk (2014) uses data from the National Longitudinal Survey of Youth 1997 and finds that false ID laws with scanner provisions have large impacts on binge drinking participation, frequency of alcohol consumption and binge drinking frequency among minors. This paper reexamines how false ID laws with scanner provisions affect underage drinking. I first demonstrate that analyses based on NLSY97 data fail falsification exercises testing for significant pre-intervention effects, and that the estimated effects based on these data are highly sensitive to the inclusion of a lead term and to sample selection, which weakens confidence in the large estimated effects reported in Yoruk (2014). I then use data from the Youth Risk Behavior Surveillance System to show that false ID laws with scanner provisions have no effect on underage drinking behavior.

1. Introduction

Youth alcohol use is a prominent public health issue in the United States. As the most commonly used and abused drug, alcohol is responsible for more than 4300 deaths and 185,000 emergency room visits among minors every year.[1] Moreover, recent studies have linked underage alcohol consumption to a variety of undesirable outcomes, including risky sexual behavior (Rees et al., 2001; Carpenter, 2005b; Waddell, 2012), mortality (Dee, 1999; Carpenter, 2004a; Carpenter and Dobkin, 2009; Grant, 2010), morbidity (Carpenter and Dobkin, 2017), crime (Carpenter, 2005a; Carpenter and Dobkin, 2015), poor academic performance (Carrell et al., 2011; Lindo et al., 2013), and unemployment (Renna, 2008). The medical and social costs associated with underage drinking are estimated to be in the billions of dollars per year (Miller et al., 2006).

Given these alarming statistics and findings, how can we best address this problem? Recently, several states have passed false ID laws with scanner provisions (hereafter, FSP laws): these laws incentivize alcohol retailers and bar owners to use electronic scanners to ensure that customers are at least 21 years old and have valid identification.[2] Yoruk (2014) uses data from the National Longitudinal Survey of Youth 1997 and a difference-in-differences design to estimate the effects of these policies on underage drinking and concludes that the adoption of FSP laws significantly reduces youth alcohol use. Moreover, the magnitude of the estimates suggests that FSP laws are extremely effective compared to other alcohol control policies. Prior work has shown that increasing the Minimum Legal Drinking Age (hereafter, MLDA) reduces drinking participation and binge drinking participation by approximately 5% (Dee, 1999; Carpenter et al., 2007); zero tolerance laws have no effect on drinking participation but reduce binge drinking participation by 13% for males (Carpenter, 2004b); social hosting laws have no effect on underage drinking (Dills, 2010); vertical ID laws reduce drinking participation for 16 year olds by 10% but have no effect on binge drinking or drinking frequency (Bellou and Bhatt, 2013).[3] In contrast, the estimates reported in Yoruk (2014) suggest that

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[1] See Centers for Disease Control and Prevention (CDC) and Substance Abuse and Mental Health Services Administration (MHSA).

[2] Prices for ID scanners range from $400 to $1300 (www.idscanner.com).

[3] Though not aiming at reducing underage drinking, Anderson et al. (2013a) suggests that medical marijuana laws reduce drinking participation for 18–19 year olds by around 14%.

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FSP laws reduce binge drinking participation by 15%, frequency of alcohol consumption by 20%, and binge drinking frequency by 30%.

What is the argument for FSP laws as an approach to reducing underage drinking? FSP laws provide an affirmative defense for retailers in prosecutions for sales to minors if they can show that the scanner was used properly. These laws have the potential to reduce alcohol sales to youth through two channels. First, there may be a detection effect because an electronic scanner makes it easier to detect fake identification used to purchase alcohol. Second, there may be a deterrence effect as scanners may deter underage youth from trying to purchase alcohol. However, FSP laws may not be effective if few retailers choose to install scanners in their stores or if underage youth substitute towards retailers that do not use scanners, borrow an ID from look-alikes who are over 21, or ask someone older than 21 to purchase alcohol on their behalf.[4] Given this theoretical ambiguity, it is necessary to empirically evaluate the effectiveness of these laws, highlighting the importance of Yoruk (2014). Moreover, if the laws have the large effects reported in Yoruk (2014), it may be efficient for policy makers in every state to consider adopting FSP laws.

In this paper, I reexamine the impact of FSP laws on underage drinking using a difference-in-differences method, exploiting within state variation induced by the timing of several states passing FSP laws. First, I use the restricted National Longitudinal Survey of Youth 1997 (NLSY97) in an attempt to replicate and extend the estimates reported in Yoruk (2014). I demonstrate that analyses based on NLSY97 data fail falsification exercises testing for significant pre-intervention effects, and the magnitude and statistical significance of the estimated effects based on these data are sensitive to the inclusion of a lead term in the specification, weakening confidence in the results originally reported in Yoruk (2014). Moreover, around 50% of the significant estimates disappear when the 1997 wave of the NLSY97 is included in the analysis, casting further doubt on our ability to draw strong conclusions based on analyses of these data. I then turn to another reasonable data set for estimating the effects of FSP laws. In particular, I use the 1991–2013 national Youth Risk Behavior Surveillance System (YRBS), which offers a larger sample size and a longer sample period than NLSY97. Moreover, the YRBS was specifically designed to study youth behaviors, such as alcohol and other drug use, risky sexual behavior, and tobacco use. Estimates based on these data indicate that FSP laws have no effect on drinking participation, binge drinking participation, or drinking and binge drinking frequency. In contrast to the estimates based on the NLSY97, these results are robust to changes in specifications and do not fail falsification tests. Moreover, estimates allowing for dynamic treatment effects indicate that FSP laws have neither short-term nor long-term effects. Taken together, these results suggest that FSP laws are unlikely to have significantly reduced youth drinking.

2. Reconsidering evidence from NLSY97

To reexamine the effects of FSP laws, I first use the restricted NLSY97 data—the same source of data as used by Yoruk (2014)—in an attempt to replicate and extend Yoruk's (2014) analysis. The NLSY97 consists of a nationally representative sample of approximately 9000 youths who were 12–16 years old as of December 31, 1996. These youths have been interviewed annually since 1997.[5] Following Yoruk (2014), I begin by restricting my attention to NLSY97 data from the period 1998–2005. I then include data from 1997 in the analysis.[6]

Table 1
Summary statistics.

Variable	*N*	Mean	S.D.
Panel A: if consumed alcohol			
Yoruk (2014): NLSY97 (98–05)	40,315	0.477	0.499
Replication: NLSY97 (98–05)	40,164	0.476	0.499
Extension: NLSY97 (97–05)	49,089	0.431	0.495
YRBS (98–05)	54,730	0.464	0.499
YRBS (91–13)	157,288	0.455	0.498
Panel B: if binge drank			
Yoruk (2014): NLSY97 (98–05)	40,249	0.276	0.447
Replication: NLSY97 (98–05)	40,097	0.275	0.446
Extension: NLSY97 (97–05)	49,020	0.246	0.431
YRBS (98–05)	56,539	0.292	0.455
YRBS (91–13)	164,501	0.286	0.452
Panel C: days of alcohol consumption			
Yoruk (2014): NLSY97 (98–05)	40,315	2.731	5.021
Replication: NLSY97 (98–05)	40,164	2.685	4.938
Extension: NLSY97 (97–05)	49,089	2.373	4.688
YRBS (98–05)	54,730	2.616	5.116
YRBS (91–13)	157,288	2.507	4.926
Panel D: days of binge drinking			
Yoruk (2014): NLSY97 (98–05)	40,249	1.283	3.308
Replication: NLSY97 (98–05)	40,097	1.249	3.214
Extension: NLSY97 (97–05)	49,020	1.108	3.042
YRBS (98–05)	56,539	1.299	3.465
YRBS (91–13)	164,501	1.233	3.332
Panel E: average drinks per day			
Yoruk (2014): NLSY97 (98–05)	40,034	0.542	1.553
Replication: NLSY97 (98–05)	39,883	0.548	1.562
Extension: NLSY97 (97–05)	48,786	0.481	1.465

Note: Sample weighted means are reported.

NLSY97 asks respondents how many days did they consume alcohol and engage in binge drinking (consuming five or more drinks in one sitting) in the past 30 days, respectively. Based on this information, I construct *Days of Alcohol Consumption* and *Days of Binge Drinking* to measure drinking and binge drinking frequency, respectively, and these variables have a value of zero if participants have not drunk or binge drunk in the past 30 days. Using information on drinking and binge drinking frequency, I also generate two dummy variables—*If Consumed Alcohol* and *If Binge Drank*—to measure unconditional drinking and unconditional binge drinking participation in the past 30 days, respectively. The remaining variable, *Average Drinks per Day*, measures drinking intensity, which is calculated as *Days of Alcohol Consumption* times average drinks per sitting divided by 30. Table 1 presents the summary statistics, allowing for a comparison of the sample I used and Yoruk's (2014). In the table, we see that the means and standard deviations I calculate from the NLSY97 sample are close to those reported in Yoruk (2014).

Following Yoruk (2014), I use a difference-in-differences methodology in the analysis, exploiting variation in the timing of FSP law adoption across states. Specifically, I estimate the following model:

$$Y_{istm} = \beta' X_{istm} + \alpha' S_{stm} + \gamma FSP_{istm} + \mu_s + \eta_t + \lambda_m + \delta_{st} + \varepsilon_{istm}$$

[4] Yoruk (2014) also mentions reasons why FSP laws may not work.
[5] NLSY97 starts to interview cohorts biennially after 2011.
[6] Following Yoruk (2014), I restrict the sample period to the year of 2005, because no interviewee in the NLSY97 sample is younger than 21 years old after 2005. Personal correspondence with Yoruk indicates that, while not mentioned in Yoruk (2014), those who do not have an exact interview date or exact birthday are dropped, along with those who reported drinking more than more than 30 drinks a day more than 30 drinks a day.