Did the 18 Drinking Age Promote High School Dropout? Implications for Current Policy

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ABSTRACT. Objective: Disagreement exists over whether permissive minimum legal drinking age (MLDA) laws affected underage adolescents (e.g., those age 17 years with the MLDA of 18). We used MLDA changes during the 1970s and 1980s as a natural experiment to investigate how underage exposure to permissive MLDA affected high school dropout. **Method:** MLDA exposure was added to two data sets: (a) the 5% public use microdata samples of the 1990 and 2000 censuses (n = 3,671,075), and (b) a combined data set based on the 1991–1992 National Longitudinal Alcohol Epidemiological Survey (NLAES) and the 2001–2002 National Epidemiological Survey on Alcohol and Related Conditions (NESARC; n = 16,331). We used logistic regression to model different thresholds of MLDA on high school dropout. We also estimated models conditioned on demographic variables and familial risk of developing alcohol problems. **Results:** Only the MLDA of 18 predicted high school dropout. Exposure was associated with 4% and 13% higher odds of high school dropout for the census and NLAES/NESARC samples, respectively. We noted greater impact on women (5%–18%), Blacks (5%–19%), and Hispanics (6%). Self-report of parental alcohol problems was associated with 40% higher odds, which equals a 4.14-point increase in dropout rate for that population. **Conclusions:** The MLDA of 18 likely had a large impact on high school dropout rates, suggesting that the presence of legal-aged peers in a high school setting increased access to alcohol for younger students. Our results also suggest that policy can promote less dangerous drinking behavior even when familial risk of alcohol use disorders is high. (*J. Stud. Alcohol Drugs, 76,* 680–689, 2015)

EARLY ONSET OF REGULAR ALCOHOL USE has been linked to subsequent alcohol dependence, heavy drinking patterns, misuse of other substances, and even lower educational attainment, both contemporaneously and later in life (Agrawal et al., 2006, 2009; Grant et al., 2006, 2012; Hingson et al., 2006). However, there is concern that the relationship between adolescent and young adult drinking behavior and outcomes like educational attainment is primarily correlational, with early drinking serving as a marker for other genetic or environmental factors that are truly causal (Prescott & Kendler, 1999; Sartor et al., 2009b; Ystrom et al., 2014). For example, research suggests that children of parents who misuse alcohol are more likely to drink at an earlier age (Sartor et al., 2007), become alcohol dependent (Hingson et al., 2006), and drop out of high school (Townsend et al., 2007).

The impact of early drinking on education could then easily be overestimated if this underlying environmental and genetic liability were not properly understood, or, worse, if it were not controlled for at all. As Townsend and colleagues (2007) note, although a variety of cross-sectional and longitudinal studies have been performed assessing the relationship between teen drinking and high school dropout, many of those with more rigorous methodology have failed to observe significant dropout effects after adding additional covariates, raising questions about which factors are truly causal.

Randomized experiments are often the preferred method of dealing with potential biases introduced by unmeasured factors but are expensive and may reduce generalizability. One alternative is a quasi-experimental approach that uses "natural experiments." Exposure to permissive (i.e., under age 21) statewide minimum legal drinking age (MLDA) laws has been widely used as a natural experiment in past research because these policies were likely unrelated to individual-level risk factors (such as genetic liability for heavy drinking) and, unlike other factors such as increases in price due to taxes, changed because of national trends rather than owing to existing differences between states (Dee & Evans, 2003). The ability to legally purchase alcohol before age 21 has been linked not only to drinking behavior, for example, by promoting both earlier regular drinking onset and heavier drinking patterns (Cook & Moore, 1993; Dee & Evans, 2003), but also to outcomes that are likely a consequence of earlier risky drinking, such as increases in teen traffic fatalities, decreased educational attainment, and even higher rates of alcohol use disorder and heavy drinking later in life (Norberg et al., 2009; Plunk et al., 2013; Wagenaar & Toomey, 2002).

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However, research on the effects of permissive MLDA on education outcomes has also produced inconsistent results (Wagenaar & Toomey, 2002). For example, Dee and Evans (2003) noted that permissive MLDA likely increased risky teen drinking but found that the impact on high school dropout was unlikely to be meaningful for justifying policy. However, they focused on students who completed at least the 11th grade, which is a notable limitation—they did not assess high school dropout for younger underage students who could have been provided alcohol by their older peers (Dee & Evans, 2003).

This is an important limitation in light of what we know about how underage drinkers acquire alcohol in other settings where peers who are both under and over the legal age comingle. For example, underage drinkers on college campuses often report being able to easily obtain alcohol and that legal-aged drinkers were their primary source (Wagenaar et al., 1996; Wechsler et al., 2002). Most legal-aged college students also report frequently providing alcohol to underage peers (Brown et al., 2009). Further, efforts to curb underage college drinking are likely hampered by an environment characterized by easy access to alcohol coupled with a culture that promotes drinking to excess (Grucza et al., 2009; Johnston et al., 2008; Plunk et al., 2013).

It is within this context that we examined the impact of permissive MLDA on individuals who were underage high school students (e.g., age 17 while the MLDA was 18) at their time of exposure during the late 1970s to the mid-1980s. Similar to trends seen for college drinking, if high school students of legal drinking age provided alcohol to their younger peers, one would also expect to see an upswing in the negative consequences associated with teenage drinking, such as an increase in high school dropout. In addition, if underage students gained access to alcohol primarily because of comingling with older high school peers who were of legal age, we would expect the 18 drinking age to have a larger impact than other drinking ages (i.e., a legal drinking age of 19 or 20).

To this end, we assessed several different thresholds of permissive MLDA. We considered changes in MLDA to be a source of exogenous variation for estimating the impact of underage drinking; that is, we did not expect statewide MLDA to be linked to an individual's likelihood of dropping out of high school except through the impact of permissive MLDA exposure on drinking behavior. Further, if we did observe a policy effect, we would have expected it to be strongest in populations at the highest risk for underage drinking, such as individuals with a parental history of alcohol problems, which earlier research has not investigated. Last, we also considered how MLDA exposure might have affected specific drinking behaviors, such as high school–age weekly drinking, that could have accounted for increases in high school dropout.

Method

Source data

We analyzed two separate sources of data. The first series of analyses was based on data obtained from the Integrated Public Use Microdata Series site (Ruggles et al., 2010) for the 1990 and 2000 decennial censuses. The 5% samples for both waves were combined and further restricted based on criteria described below.

The sample for our second series of analyses was constructed from two nationally representative U.S.-based surveys: the National Longitudinal Alcohol Epidemiological Survey (NLAES), administered in 1991 and 1992, and the 2001 wave of the National Epidemiological Survey on Alcohol and Related Conditions (NESARC). Both surveys were based on the Alcohol Use Disorder and Associated Disabilities Interview Schedule series of structured interviews, used similar sampling strategies, and were conducted by the U.S. Bureau of the Census under supervision of the National Institute on Alcohol Abuse and Alcoholism (Grant et al., 1994, 2003).

Both sampled the adult noninstitutionalized population and included the District of Columbia; the NLAES was limited to the contiguous United States, whereas the NESARC sampled all states. In addition, the NLAES oversampled Blacks and those ages 18–29, whereas the NESARC oversampled Blacks, Hispanics, and the 18–24 age range. The NESARC had 43,093 respondents with an overall response rate of 81% (derived from 99% sampling frame and 89% household and 93% person-level response rates), and the NLAES had 42,862 respondents with an overall response rate of 90% (92% household and 98% person-level response rates; Grant et al., 1994, 2003).

For our main analyses, we restricted both data sets to those born between 1960 and 1969. These birth years put respondents at high school age during the period of greatest change in MLDA (1978–1987) and also allowed us greater flexibility for including additional covariates (e.g., education policy). We restricted all of our analyses to individuals who reported at least beginning high school. Further, only individuals who self-reported non-Hispanic White, non-Hispanic African American, or Hispanic ethnicity were included to ensure homogenous race/ethnicity covariates and to maximize comparability between all sources of data, which varied somewhat with regard to how some categories of race/ethnicity were assessed (e.g., multiple-race responses in the census have only been allowed since 2000). This yielded 3,671,075 respondents in the census sample and 16,331 in the combined NLAES/NESARC sample. Sensitivity analyses for the full range of MLDA changes, from 1949 to 1969, were also performed; sample sizes were larger for these analyses (n = 8,063,788 for the census-based sample; n = 33,625 for the NLAES/NESARC).

Outcome measures and covariates

High school dropout was used as the outcome for all analyses. The 1990 and 2000 census items assessing educational attainment did not differentiate between earning a diploma and receiving a General Educational Development (GED) credential; although the NLAES/NESARC did differentiate, we combined them into one category to ensure comparability between samples. This means our main analyses assessed failure to "complete" high school rather than "graduation" because GED recipients are grouped with those who earned a traditional high school degree (Heckman & Lafontaine, 2010). This could potentially lead us to underestimate the impact of permissive MLDA for those who dropped out and later obtained a GED; we explore how this affects results based on the NLAES/NESARC in a separate analysis in which we exclude GED recipients.

We included several state-level covariates that we based on exposure at age 17. Each state's Gini coefficient, a measure of statistical dispersion used to indicate income inequality (University of Texas Inequality Project, 2012), was included to control for socioeconomic factors that could make the impact of poverty more severe on the decision to drop out of high school (Coley & Baker, 2013). Citizen political ideology was included to model differences in political climate that might affect education policy adoption and funding, which could affect individuals' education outcomes (Berry et al., 1998). Exposure to two state-level education policies linked to high school dropout was also included: mathematics and science course graduation requirements and mandated high school exit exams (Plunk et al., 2014).

The NLAES/NESARC data allowed us to include additional alcohol-related variables. First, we explored the role of parental alcohol problems on respondent risk of high school dropout. Both surveys assessed self-reported familial drinking problems; we used the NLAES items asking if one's biological mother or father was "ever alcoholic" and the NESARC items that assessed whether one's biological mother or father was "ever an alcoholic or problem drinker." We constructed a dummy variable indicating that a respondent had endorsed these items for either biological parent. Past research suggests that parental drinking problems are significant risk factors for early drinking initiation and later progression to alcohol dependence (Sartor et al., 2007); further, the NLAES/NESARC items assessing familial alcohol problems are both valid and exhibit high reliability in the general population (Grant et al., 2003).

We also used the NESARC to construct high school age at first drink and high school age weekly drinking variables. Additional covariates included dummy variables for ever drinking, state, birth year, census/survey wave, race/ethnicity, and sex.

Minimum legal drinking age exposure coding

We defined permissive MLDA exposure as the ability to legally purchase alcohol before age 21. An underlying assumption of our analyses was that state of residence at time of survey was a reasonable proxy for residence at age of potential exposure. Our earlier work based on MLDA suggested that this was a reasonable assumption. For example, roughly 15% of NLAES respondents changed their MLDA exposure by moving away from their birth state before they were surveyed (Norberg et al., 2009). However, migration status was not a significant predictor of MLDA exposure—migration patterns based on MLDA exposure were very similar to unselective migration.

Further, our earlier work suggested that migration would bias our results toward a false-positive result only under very high selective migration: more than 25% (Grucza et al., 2012). Because there was no evidence that any selective migration based on MLDA exposure occurred, it was unlikely that estimating MLDA exposure was a source of confounding for our analyses.

MLDA exposure was determined as described elsewhere (Grucza et al., 2012; Norberg et al., 2009; Plunk et al., 2013), using a variety of sources, including peer-reviewed research (Du Mouchel et al., 1987; O'Malley & Wagenaar, 1991; Wagenaar, 1982), the Statewide Availability Data System (Ponicki, 2004), and news sources (Associated Press, 1996). We assessed three different thresholds of MLDA exposure in our analyses: (a) any permissive MLDA, to include the legal drinking age of 18, 19, or 20; (b) a threshold including the drinking ages of 18 and 19; and (c) a threshold that only considered the impact of the MLDA of 18. Each individual was assigned exposure based on the MLDA experienced at age 17.

Statistical methods

Our analytic method modelled exposure to an environmental change or treatment by comparing pre- and post-intervention differences in an outcome for exposed groups with those for unexposed comparison groups (Wooldridge, 2010). We used fixed-effects regression models to control for the potential impact of unobserved time-invariant heterogeneity, allowing intercepts to differ both between groups and across time (Allison, 2009; Wooldridge, 2010). This was achieved by including dummy variables for birth year and state of residence. We conducted both full-sample and conditional analyses, wherein we further restricted our samples based on demographic characteristics (sex and race/ethnicity). In our analyses using NLAES/NESARC data, we were also able to stratify based on risk of developing alcohol problems.

These conditional analyses allowed us to do two things: (a) assess possible differences across demographic groups

Table 1. Demographic characteristics by sample

	Census microdata		NLAES/NESARC	
Variable	*n*	%	*n*	%
Sample size	3,671,075	100.00	16,331	100.00
Educational attainment				
No high school diploma	460,359	12.54	2,053	12.57
High school diploma/GED	1,195,083	32.55	4,458	27.30
Some college, no degree	906,868	24.70	3,668	22.47
Any college degree	1,108,765	30.20	6,152	37.68
Race/ethnicity				
White	2,861,907	77.96	10,871	66.57
Black	431,002	11.74	3,031	18.56
Hispanic	378,166	10.30	2,429	14.87
Sex				
Men	1,809,561	49.29	6,958	42.60
Women	1,861,514	50.70	9,373	57.39
Education policy covariates				
Mandatory exit exam	102,713	2.80	40	2.76
Mean math and science CGR		1.82		1.85
Other state-level covariates				
Mean citizen political ideology		46.09		49.91
Mean income disparity		0.39		0.40
Risk for alcohol misuse				
Parental drinking problem	–	–	4,166	25.51

Notes: Census microdata are based on the 1990 and 2000 5% census samples. The NLAES/NESARC is based on the 1991–1992 National Longitudinal Alcohol Epidemiological Survey (NLAES) and the 2001–2002 National Epidemiological Survey on Alcohol and Related Conditions (NESARC). Both samples are restricted to birth years 1960–1969. GED = General Educational Development credential; CGR = the course graduation requirement for each state, which ranged from 0 to 6; exit exam denotes that a state required that students pass an exam to receive a diploma.

and (b) model any differential impact on a population that is at greater risk for developing alcohol problems and, thus, that is potentially most responsive to alcohol policy. Further, both series of conditional analyses allowed us to explore the possible impact of unmeasured factors that could be related to both policy exposure and high school dropout (Gruber & Mullainathan, 2005; Morgan & Winship, 2007).

Logistic regression was used for all analyses to estimate the relative odds of dropping out of high school based on permissive MLDA exposure. The basic structure of the regression model was as follows:

$$Y_{ist} = A_s + B_t + \beta_1 X_{1ist} + \ldots + \beta_n X_{nist} + \beta MLDA_{st} + \varepsilon_{ist}.$$

Y_{ist} is high school dropout status for i individual in s state in t year. A_s and B_t represent the fixed state and year effects, and X_1 through X_n are the additional covariates for each individual. *MLDA* is the coefficient of interest in our analyses and denotes permissive MLDA exposure for each state and year. This model allows us to take advantage of incremental changes in MLDA over time to estimate an average effect while controlling for potential time and state-invariant factors.

Because the NLAES/NESARC uses complex survey designs, we tested both design- (including sample weights) and model-based approaches. There were no significant differences between the model- or design-based approaches, and the model-based approach was chosen to maximize efficiency. All analyses were performed using Version 2.15.2 of the statistical language R (R Development Core Team, 2012). We used two-way cluster-robust standard errors to address possible correlation of observations within both state and time (Petersen, 2009). These were obtained using R code based on the work of Arai (2015).

Results

Sample description

Demographic information for both samples is listed in Table 1. Table 2 additionally reports high school dropout for each demographic group that we examined in our conditional analyses. The unweighted NLAES/NESARC sample differed from the nationally representative census sample. Oversampling likely contributed to differences based on race/ethnicity, as the NLAES/NESARC contained a higher proportion of Blacks and Hispanics (19% and 15%, respectively, compared with 12% and 10% for the census; Table 1). There was also a larger proportion of women in the NLAES/NESARC (57%, compared with 51% in the census sample). Educational attainment rates were similar, but with a larger proportion of college graduates in the NLAES/NESARC (38%) compared with the census sample (30%). Citizen political ideology also differed between samples; the mean score was four percentage points higher than the census. Education policy covariates were similar, as was mean in-

Table 2. High school dropout by demographic group and sample

	Census microdata (n = 3,671,075)		NLAES/NESARC (n = 16,331)	
Variable	n	%	n	%
Overall	460,359	12.54	2,053	12.57
By race/ethnicity				
White	269,484	9.41	1,134	10.44
Black	84,685	19.65	432	14.25
Hispanic	106,190	28.08	487	20.05
By sex				
Men	252,363	13.95	862	12.40
Women	207,996	11.17	1,191	12.71
Conditioned on parental drinking problem	–	–	511	12.26

Notes: Percentages are based on the total number of individuals in each group; these values are reported in Table 1. Census microdata are based on the 1990 and 2000 5% census samples. The NLAES/NESARC is based on the 1991–1992 National Longitudinal Alcohol Epidemiological Survey (NLAES) and the 2001–2002 National Epidemiological Survey on Alcohol and Related Conditions (NESARC). Both samples are restricted to birth years 1960–1969.

come disparity. In addition, 26% of the NLAES/NESARC sample reported having at least one biological parent with alcohol problems.

Permissive minimum legal drinking age threshold analyses, U.S. Census data

Results from a series of analyses modeling high school dropout predicted by different thresholds of underage permissive MLDA exposure can be seen in Table 3. Both the any-permissive-MLDA threshold (i.e., MLDA of 20, 19, and 18) and the MLDA-19-and-18 threshold exhibited small effect sizes and nonsignificant associations, but the MLDA of 18, when modeled as a separate category of permissive MLDA, produced a larger estimate that was also significantly associated with dropping out.

Minimum legal drinking age of 18 and high school dropout, U.S. Census data

Overall, underage exposure to the MLDA of 18 was associated with 4% higher odds of dropping out of high school in our nationally representative census sample (OR = 1.04, 95% CI [1.01, 1.08]; Table 4). When taken as separate groups in conditional analyses, neither men nor Whites exhibited significant associations, but underage exposure to MLDA of 18 was associated with 5% higher odds of high school dropout for women (OR = 1.05, 95% CI [1.02, 1.08]), 5% higher odds for Blacks (OR = 1.03, 95% CI [1.03, 1.08]), and 6% higher odds for Hispanics (OR = 1.06, 95% CI [1.03, 1.09]).

Minimum legal drinking age of 18 and high school dropout, NLAES/NESARC data

Across our full NLAES/NESARC sample, underage MLDA of 18 exposure was associated with 13% higher odds of dropping out of high school (OR = 1.13, 95% CI [1.05, 1.23]; Table 5, full models are available on request). Similar to our census analyses, conditional analyses based on sex and race/ethnicity resulted in larger estimates for some demographic groups; taken as a separate group, women exposed to the MLDA of 18 had 18% higher odds of dropping out (OR = 1.18, 95% CI [1.03, 1.35]). Other conditional analyses were likely hindered by smaller sample sizes; for

Table 3. Estimates for models predicting high school dropout at different thresholds of MLDA exposure at age 17, U.S. Census microdata

Variable	MLDA 18, 19, or 20	MLDA 18 or 19	MLDA 18
MLDA exposure	-0.008 (0.013)	0.004 (0.012)	0.037 (0.017)*
Income disparity	0.486 (1.826)	0.229 (1.778)	0.149 (1.573)
Political ideology	0.001 (0.001)	-0.001 (0.001)	0.001 (0.001)
Math/science CGR	0.025 (0.006)***	0.031 (0.008)***	0.030 (0.007)***
Exit exam	-0.170 (0.058)*	-0.172 (0.060)*	-0.119 (0.057)*
Black (ref. = White)	0.601 (0.066)***	0.600 (0.066)***	0.601 (0.065)***
Hispanic (ref. = White)	1.330 (0.146)***	1.331 (0.146)***	1.330 (0.146)***
Sex (ref. = Women)	0.223 (0.047)***	0.226 (0.048)***	0.227 (0.048)***

Notes: Standard errors are in parentheses. MLDA = minimum legal drinking age; ref. = reference; n = 3,671,075, based on the 1990 and 2000 5% census microsamples, restricted to birth years 1960–1969. CGR is the course graduation requirement for each state; exit exam denotes that a state required that students pass an exam to receive a diploma.
*p < .05; ***p < .001.

TABLE 4. Conditional analyses predicting high school dropout from MLDA of 18 at age 17, U.S. Census microdata

Variable	OR	[95% CI]	*n*
Full sample	1.04	[1.01, 1.08]*	3,671,075
Conditioned on sex			
Men	1.03	[0.98, 1.07]	1,809,561
Women	1.05	[1.02, 1.08]***	1,861,514
Conditioned on race/ethnicity			
White	1.01	[0.97, 1.06]	2,861,907
Black	1.05	[1.03, 1.08]***	431,002
Hispanic	1.06	[1.03, 1.09]***	378,166

Notes: MLDA = minimum legal drinking age; OR = odds ratio; CI = confidence interval; *n* = 3,671,075, based on the 1990 and 2000 5% census microsamples, restricted to birth years 1960–1969.
*$p < .05$; ***$p < .001$.

example, Blacks exhibited 19% higher odds of dropping out of high school, but this relationship was not statistically significant (OR = 1.19, 95% CI [0.99, 1.46]). Last, we conducted a separate analysis in which we excluded GED recipients; the resulting MLDA-of-18 parameter estimate did not differ from our analysis that included GED recipients but did seem to explain more of the variance involved (OR = 1.16, 95% CI [1.08, 1.25]; data not shown).

Minimum legal drinking age of 18 and problem drinking risk, NLAES/NESARC data

The NLAES/NESARC allowed us to stratify by risk of developing problematic drinking as indicated by self-report of parental alcohol problems. We noted differential impact based on this risk: Those who reported parental drinking problems exhibited 40% higher odds of dropping out of high school when exposed to the MLDA of 18 (OR = 1.40, 95% CI [1.32, 1.48]), a result that fell outside the confidence intervals for the full-sample analysis. MLDA of 18 exposure did not significantly affect those respondents who did not report parental alcohol problems (OR = 1.08, 95% CI [0.96, 1.20]; Table 5).

Minimum legal drinking age of 18 and age of onset of drinking/weekly drinking

We used only the NESARC in these analyses because the NLAES only assessed age at first drink, whereas the NESARC included an additional item assessing age at initiation of weekly drinking. As this resulted in a smaller sample, the whole period of changing MLDA—from birth years 1949 to 1969—was used to maximize power. Underage exposure to the MLDA of 18 did not affect ever-drinking for the NLAES/NESARC sample (OR = 1.10, 95% CI [0.94, 1.31]), nor did it have an impact when conditioned on individuals who reported parental alcohol problems (OR = 1.17, 95% CI [0.83, 1.63]).

Underage exposure to the MLDA of 18 was related to high school–age drinking initiation (i.e., ages 14–17); exposure was associated with 17% and 26% higher odds of having initiating drinking while of high school age for the whole NESARC sample and for those individuals reporting parental drinking problems, respectively (OR = 1.17, 95% CI [1.03, 1.34]; OR = 1.26, 95% CI [1.15, 1.46]). After we conditioned on any high school–age drinking, MLDA of 18 exposure was also associated with 14% higher odds of

TABLE 5. Conditional analyses predicting high school dropout from MLDA of 18 at age 17, combined NLAES/NESARC sample

Variable	OR	[95% CI]	*n*
Full sample	1.13	[1.05, 1.23]**	16,331
Conditioned on sex			
Men	1.09	[0.81, 1.46]	6,956
Women	1.18	[1.03, 1.35]*	9,375
Conditioned on race/ethnicity			
White	1.09	[0.87, 1.38]	10,870
Black	1.19	[0.99, 1.46]†	3,032
Hispanic	1.06	[0.90, 1.27]	2,429
Conditioned on self-report of parental alcohol problem			
Parental drinking problem	1.40	[1.32, 1.48]***	4,166
No parental drinking problem	1.08	[0.96, 1.20]	12,165

Notes: MLDA = minimum legal drinking age, based on the 1991–1992 National Longitudinal Alcohol Epidemiological Survey (NLAES) and the 2001–2002 National Epidemiological Survey on Alcohol and Related Conditions (NESARC); OR = odds ratio; CI = confidence interval.
†$p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$.

having transitioned to high school–age weekly drinking for individuals who reported parental drinking problems (OR = 1.14, 95% CI [1.12, 1.16]), but did not have a significant impact on the whole NESARC sample (OR = 1.00, 95% CI [0.91, 1.11]).

Supplemental analyses

As a specification check, we assessed how underage exposure to the MLDA of 18 affected high school dropout across the whole period of changing MLDA, from birth years 1949 to 1969. These models contained fewer covariates, as we had incomplete education policy data for this period, but produced results consistent with our main analyses both with census data (OR = 1.04, 95% CI [1.02, 1.06], n = 8,063,788) and for the NLAES/NESARC (OR = 1.08, 95% CI [1.00,1.17], n = 32,534).

Discussion

Summary of findings

We have extended previous work exploring how permissive MLDA affects young people by investigating its impact on underage high school students specifically. Using census data, we conducted threshold analyses of different levels of permissive MLDA on high school dropout based on exposure at age 17; only the MLDA of 18 affected underage high school students. We noted larger apparent effects for women, Blacks, and Hispanics, but overlapping confidence intervals did not suggest between-group differences. We then continued our analyses using NLAES/NESARC data, in which we again noted significant effects for the full sample and for women.

The NLAES/NESARC data also allowed us to assess the impact of parental alcohol problems, for which we observed a differential high school dropout effect based on MLDA of 18 exposure. Last, we noted that exposure to the MLDA of 18 was associated with increased odds of having initiated drinking during high school. The MLDA of 18 also promoted transitioning from regular to weekly drinking while of high school age, but only for those individuals with a history of parental alcohol problems.

Differences based on parental alcohol problems likely reflect increased genetic and environmental vulnerability to underage drinking (Agrawal et al., 2009; Prescott & Kendler, 1999; Sartor et al., 2009a). In addition, parental alcohol problems correspond with an increased likelihood of offspring deviant behaviors (Hussong et al., 2010; McGue et al., 2001; Meyers & Dick, 2010; Molina et al., 2010), including high school dropout. For instance, Hussong and colleagues (2010) report that even when parental problem drinking predates occurrence of offspring externalizing problems (i.e., distal effects), children of alcoholics are at greater risk for deviant behaviors than control offspring. These early problem behaviors affect later scholastic achievement via multiple pathways (e.g., Fergusson & Horwood, 1995), including, possibly, increased sensitivity to the environmental effects of more permissive MLDA. Therefore, it is possible that the MLDA of 18 moderated the genetic (and environmental) vulnerability to externalizing problems, including dropping out of high school, conferred by parental problem drinking.

Such a mechanism was previously noted in a study by Grant and colleagues (2012), which noted that even in twin pairs (who are matched for genetic and, putatively, some of the environmental effects of parental problem drinking), the twin reporting underage drinking was significantly more likely to report less than 16 years of education relative to his or her co-twin who consumed alcohol at age 18 or later. However, genetically informed studies such as these have traditionally relied on offspring age at first drink, which is strongly confounded with genetic risk to parental and offspring alcoholism. In contrast, we examined MLDA. This is an environmental exposure that is largely uncorrelated to familial vulnerability, although its impact on drinking behavior and its correlates and sequelae, such as high school dropout, may be more pronounced in those with a history of parental problem drinking.

Although the magnitude of the associations we note in our findings might not seem meaningful at first glance, the high prevalence of high school dropout in the United States means that small differences in relative odds can take on practical importance, especially for at-risk groups. For example, our results based on the census sample (OR = 1.04; Table 4) suggest that the MLDA of 18 was associated with a 0.41-point increase in high school dropout (i.e., 12.95%, up from 12.54%; Table 2). However, the impact of MLDA exposure is more pronounced for groups exhibiting higher baseline dropout rates: Permissive MLDA was associated with a 0.85-point increase in dropout for Blacks (20.5%, up from 19.65%) and 1.21 for Hispanics (29.29%, up from 28.08%). In this case, modest effect sizes also take on more importance because the negative health and social consequences related to high school dropout disproportionately affect these populations.

Further, permissive MLDA was associated with a 4.14-point increase in the dropout rate after we conditioned on risk for problem drinking (16.41%, up from 12.26%). This is a 34% increase in high school dropout for a population that, while already at greater risk for misusing alcohol, made up 25.5% of the NLAES/NESARC sample. These results are consistent with our finding that the MLDA of 18 was also modestly associated with increased odds to have transitioned from regular to weekly drinking while of high school age for this group. Those with high familial risk of alcoholism, who are already at elevated risk for drinking problems and high school dropout, appear to be at markedly

higher risk for both of these outcomes when the drinking age is set at 18.

Our findings have important implications. The most plausible way by which underage high school students were affected by the MLDA of 18 would be their 18-year-old peers, which suggests that permissive MLDA could have promoted high school environments similar to those we observe on college campuses today. Current proposals to lower the drinking age in response to risky underage college drinking would need to address the degree to which these behaviors would occur earlier at the high school level. The apparent differential effect based on predisposition for developing drinking problems also provides further evidence that policy can successfully affect drinking behavior in young adult populations characterized by high environmental and genetic risk.

Limitations and conclusion

We made several assumptions during the course of our analyses, which if violated could have biased our results and thus represent potential limitations of our study. First, we assumed that changes in MLDA were primarily exogenous and that drinking age in each state did not vary as a consequence of an unobserved confounding factor. However, the suitability of MLDA laws as exogenous predictors of alcohol use has been supported by past research, even over other alcohol policy, such as beer taxes (Dee & Evans, 2003). We also addressed this potential limitation in our analysis conditioned on familial risk, in which unobserved factors correlated with parental drinking problems were controlled for.

We also assumed that any error introduced by our method of estimating retrospective policy exposure was essentially random. Although we did introduce error into our analyses by estimating exposure, there was no evidence to suggest that this error would bias our results toward false-positive associations, which would require that the decision to drop out of high school was correlated with the decision to move to states with permissive MLDAs. If cross-state migration is uncorrelated with MLDA and high school dropout, as we assumed, our estimates likely underestimate the true relationship between permissive MLDA and high school dropout. We have established that this is a reasonable assumption in previous work (Grucza et al., 2012; Norberg et al., 2009).

Last, high school dropout is a complex phenomenon, and the data sets we used did not capture many relevant risk factors (e.g., individual-level factors including parental educational attainment, childhood experiences, and socioeconomic status; school- or district-level factors such as quality of instruction and local funding). As such, our estimates reflected the average effect of MLDA exposure while holding constant other unmeasured factors that might also have influenced educational attainment.

Our assumption that MLDA change is exogenous meant that we also assumed that MLDA is unrelated to these other unmeasured factors predictive of high school dropout. If this assumption was incorrect, we could have introduced serious bias into our analyses. However, we would have expected this bias to lead to false-positive associations only when an unmeasured factor predictive of high school dropout systematically changed along with MLDA (e.g., if states that adopted the MLDA of 18 also usually decreased school funding at the same time, or if children from single-parent households were more likely to later be exposed to lower MLDA). To the degree that our assumptions were reasonable, our results represent the average effect of MLDA exposure in addition to these other factors.

Even in light of these potential limitations, our findings imply that exposure to the MLDA of 18 significantly affected underage drinkers by promoting high school dropout. Our results are consistent in both a nationally representative census sample and when using a combined NLAES/NESARC data set, which additionally allowed us to establish an apparent differential effect based on risk of developing problematic drinking and to explore the impact of MLDA exposure on several high school–age drinking behaviors.

Our findings have implications for policy, most importantly that risky underage college drinking does not immediately justify calls to lower the drinking age. More broadly, we offer additional evidence that policy aimed at restricting underage access to alcohol can significantly influence drinking behavior despite high environmental and genetic risk of alcohol misuse.

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