NBER WORKING PAPER SERIES

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Working Paper 23803
http://www.nber.org/papers/w23803

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2017

We thank Carlos Dobkin, Hayley Chouinard, and Louis-Philippe Beland for their helpful comments. All errors are ours alone. The corresponding author is Benjamin Cowan. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 23803
September 2017
JEL No. I12,I24,I26

ABSTRACT

Previous studies have shown that years of formal schooling attained affects health behaviors, but little is known about how the stringency of academic programs affects such behaviors, especially among youth. Using national survey data from the Youth Risk Behavior Surveillance System (YRBS), we study the effects of mathematics and science high-school graduation requirements (HSGR) on high school students' risky health behaviors--specifically on drinking, smoking, and marijuana use. We find that an increase in mathematics and science HSGR has significant negative impacts on alcohol consumption among high-school students, especially males and non-white students. The effects of math and science HSGR on smoking and marijuana use are also negative but generally less precisely estimated. Our results suggest that curriculum design may have potential as a policy tool to curb youth drinking.

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I. INTRODUCTION

Since the seminal work of Grossman (1972), the relationship between schooling and health has been well established. Years of formal schooling attained is strongly correlated with health behaviors and outcomes, with many papers providing evidence for a causal effect of schooling on health.[1] Most previous work on this question measures schooling in terms of years spent in school or highest degree attained while ignoring heterogeneity in the quality of schooling.[2] Even less is known about how educational quality affects the health behaviors of youth specifically.[3] This question is important given that drinking and smoking habits established in youth are often persistent over the life cycle (Farrell and Fuchs, 1982; Wechsler et al., 1995; Arria et al., 2008); thus, deterring risky behaviors at younger ages could have significant long-term benefits (Gruber and Zinman, 2001; Auld, 2005).

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[1] Readers can refer to the following papers on education and health: education and smoking (De Walque, 2007; Grimard and Parent, 2007; Park and Kang, 2008; Heckman et al., 2014), education and self-reported health (Adams, 2002; Arendt, 2005), education and mortality (Lleras-Muney, 2005; Buckles et al., 2016), education and infant health (McCrary and Royer, 2011), education and various health behaviors (Kenkel et al., 2006; Cutler and Lleras-Muney, 2010; Conti et al., 2010; Savelyev and Tan, 2014); and a literature review on the impacts of education on health (Cutler and Lleras-Muney, 2006).

[2] Exceptions include Fletcher and Frisvold (2011, 2014), who find that college selectivity affects both health behaviors and health outcomes. Frisvold and Golberstein (2011) find that improvements in school quality, measured by the pupil-teacher ratio, average teachers' wages, and length of the school year, amplify the effects of education on self-rated health, smoking, obesity, and mortality. Sansani (2011) shows that school quality, measured by its financial return, length of the school term, and relative teacher wage, predicts mortality.

[3] Cowan (2011) finds that lower college costs mitigate risky behaviors among high-school students. Jensen and Lleras-Muney (2012) show that increasing schooling and decreasing work reduce smoking among teens in the Dominican Republic.

In this study, we examine how high school graduation requirements (HSGR) regarding mathematics and science credits impact the health behaviors of high-school students. The graduation requirements, set by individual states, represent the minimum number of courses in specific subjects that must be passed in order to earn a high school diploma in that state. Though a few states do not have specific course requirements (leaving it up to local school districts instead), most states set the minimums themselves. Inspired by the National Commission on Excellence in Education's influential 1983 report, *A Nation at Risk*, which argued in favor of raising high school stringency in order to address the academic underachievement of American youth, many states have legislated increases in their HSGR over the last several decades (Clune and White, 1992; Chaney et al., 1997; Teitelbaum, 2003). A growing literature in economics has studied various education and labor-market effects of these changes (Clune and White, 1992; Chaney et al., 1997; Schiller and Muller, 2003; Teitelbaum, 2003; Federman, 2007; Goodman, 2017).

In our paper, we follow most of the literature in focusing specifically on mathematics and science requirements because these courses have been shown to be important determinants of various future socioeconomic outcomes for youth including college attainment and earnings.[4]

[4] Levine and Zimmerman (1995) find that high-school math and science courses have a positive impact on future earnings. Similarly, Rose and Betts (2004) and Joensen and Nielsen (2009) show that mathematics courses taken in high school are related to future earnings. Bottia et al. (2015) find that taking physics and attending a school with a math and science focused program are closely associated with students' choice of STEM as a major. Kim et al. (2015) show that completing an Algebra II course in high school leads to a higher chance of going to college. Federman (2007) shows that higher state math and science graduation requirements lead to a higher probability of choosing a technology major in college. Goodman (2017) finds that state changes in minimum high school

This paper adds to the literature on youth risky behavior by considering how the stringency of high school via changes to state-level graduation requirements affects students' drinking, smoking, and marijuana use.

There are at least two possible channels through which HSGR may affect risky health behaviors. The first is through the time constraint. Stricter state requirements for math and science lead students to take more courses and to enroll in higher-level courses (Clune and White, 1992; Chaney et al., 1997; Schiller and Muller, 2003; Teitelbaum, 2003; Federman, 2007; Goodman, 2017). Schiller and Muller (2003) find that students in states with higher graduation requirements tend to enroll in higher level math courses as freshmen and persist in taking more advanced courses. The positive change of courses both in quantity and difficulty may leave students with less time to engage in substance use.

Second, there may be expectation effects. Since taking courses in math and science in high school has positive impacts on future college attainment and earnings, students in states with higher HSGR may have higher expectations about these future outcomes. Becker (1965) shows theoretically that an increase in expected future earnings could induce a decline in the amount of time dedicated to consumption activities because time becomes more expensive. As Cowan (2011) shows empirically, greater expectations for college attainment are associated with better health behaviors in high school. Therefore, higher HSGR may lead to a decrease in substance use through this channel.

Using Youth Risk Behavior Surveillance System (YRBS) national survey data from 1993 to 2013 and adopting a generalized difference-in-difference (DID) framework, we show that

math requirements substantially increase black students' completed math coursework and later earnings.

an increase in math and science HSGR has significant negative impacts on the alcohol consumption of high-school students with no accompanying increase in the consumption of cigarettes or marijuana. The effects of HSGR are typically larger among males than females and non-white students than white students. These results are consistent with Goodman (2017), who finds that state math requirements affect math course taking and later earnings of blacks but not whites, with the largest effects occurring for black males specifically.[5] Furthermore, our results are robust to the inclusion of many state-level education policy variables that have been used in the HSGR literature as controls, which guards against the possibility that HSGR is merely a proxy for other policy changes that sometimes coincide with updates to a state's HSGR.

A weakness of our study is that with our data, we cannot distinguish the mechanism by which HSGR affects alcohol use. We note, however, that the effects we estimate are concentrated among a group—racial/ethnic minority students—whose course-taking and post-high school outcomes are plausibly most affected by HSGR. Nevertheless, we acknowledge that more evidence is needed to determine how HSGR alters the substance use decisions of high-schoolers.

Another potential shortcoming of our paper is that more stringent HSGR may induce some students to drop out of school, and our data contains only high-school students. One might suspect that students who engage in riskier behaviors are more likely to drop out, causing

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[5] In a supplementary analysis, Goodman (2017) also estimates large effects of math requirements on math courses taken by Hispanics. These results are not part of the main analysis due to the use of state of birth as a proxy for state of high school attendance, which is missing for a large proportion of (foreign born) Hispanics.

sample selection bias. We initially address this issue by examining the effect of HSGR on the probability of school enrollment at different ages using U.S. Census and American Community Survey data. We find little evidence that dropout is affected by HSGR over our sample period. Some previous studies do find that HSGR affects dropout (Lillard and DeCicca, 2001; Plunk et al., 2014).[6] We show that this is likely due at least in part to differences in sample periods across studies.

Another way we address the sample selection issue is by adopting the method proposed by Carpenter and Stehr (2008), which is to estimate our models using only students who are 16 years old or under. Partly because of compulsory schooling laws, there are many fewer youth absentees under 17, making selection less of a concern in this subsample. In this case, the magnitudes of some of our results diminish modestly, which is consistent with either larger effects of HSGR among older students or modest selection bias. Regardless, we continue to find economically and statistically significant effects of HSGR on drinking among the younger subsample, indicating that the potential selection bias cannot account for the negative impacts on drinking identified in our main regressions.

Lastly, we test the validity of our identification strategy using a placebo-type analysis. Because HSGR is a non-binary treatment and many states changed their HSGR more than once, a traditional event-study framework is not suitable in our setting. Instead, we examine the effects of "placebo" policies that are lags and leads of true changes in HSGR. The results support the notion that changes in students' health behaviors are indeed caused by HSGR, since

[6] In contrast, Clark and See (2011), examining the possible dropout effect of the higher graduation standard in Florida, find no effect.

the policy effects generally get weaker as the placebo treatment moves further away from the true treatment period.

Though comparing different policies for curbing substance use among youths is beyond the scope of our paper, our results suggest that improving the rigor of high-school education may be an attractive way to accomplish this goal. First, raising HSGR increases math and science course-taking, which is a primary goal of the education reforms since the 1980s (Clune and White, 1992; Teitelbaum, 2003). Second, raising HSGR may be more feasible than increasing taxes enough to have similar effects on risky behaviors, especially when raising the (full) price of one substance might only push youths to other substances.[7] However, more work is needed to ascertain the full costs and benefits of making high school education more rigorous.

II. DATA

II.A. HSGR

Data on math and science HSGR is taken from the Digest of Education Statistics (DES) published by the National Center for Education Statistics (NCES). The DES reports the minimum Carnegie units of mathematics and science courses required for high-school graduation by 50 states and the District of Columbia.[8] Until 2001, the DES reported the first graduating class that was affected by a change to HSGR. Starting in 2002, the DES stopped

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[7] Chaloupka and Laixuthai (1997) find that drinking among youths is negatively related to beer prices while positively related to the full price of marijuana, suggesting that beer and marijuana are substitutes among youths. DiNardo and Lemieux (2001) show that raising the minimum legal drinking age increases marijuana consumption. Crost and Guerrero (2012) find that alcohol and marijuana are substitutes in a minimum drinking age law context.

[8] One Carnegie unit is defined as 120 hours of instruction time, which can be roughly translated into one academic year (two semesters) of instruction in one course.

reporting this information. Hence, we collected the impact cohort information from the education board or department of each state. Readers can refer to Appendix 1 for further explanation. The final compiled dataset contains math and science HSGR for graduation years 1993 to 2014 matched with each student using his/her state and predicted graduation year. In order to match HSGR with each student, we assume all students are admitted in fall and expected to graduate in 4 years. Since the national YRBS is conducted during February to May of each odd-numbered year (Brener et al., 2013), a 12^{th} grade student surveyed in year *X* in state *Y* would be matched with the HSGR of graduation year *X* in state *Y*, and an 11^{th} grade student surveyed in year *X* in state *Y* would be matched with the HSGR of graduation year *X+1* in state *Y*, and so on.

We add the required Carnegie units of math and science courses together to get the *total number of minimum required Carnegie units in math and science courses* by each state and graduation year, which will be used in our regressions and be referred to as *HSGR* or *math and science HSGR*.[9] Table 1 and Figure 1 shows the HSGR of each state for the selected graduating classes of 1993, 2003, and 2013. Our dataset indicates that from the graduating class of 1993 to that of 2013, 41 states (including the District of Columbia) changed their HSGR at least once and 10 states remained unchanged.[10] Appendix 2 shows the complete HSGR dataset we use

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[9] We also tried coding math and science HSGR separately and included them in our models. The results suggest negative impacts of both math HSGR and science HSGR, with no clear evidence of which one is more important. But since many states change math HSGR and science HSGR at the same time, including these separately yield less precise results due to the collinearity. Therefore, like Plunk et al. (2014), this study combines math and science HSGR together.

[10] State changes to HSGR have been focused on math and science courses for several decades (Teitelbaum, 2003).

(total math and science HSGR), sorted by the number of units states required for the graduation class of 1993. In the results section, we examine the possibility that states that choose to make larger changes to their HSGR have different time-varying characteristics than states that make smaller changes.

II.B. YRBS

Our data on the health behaviors of high-school students comes from the biannual Youth Risk Behavior Surveillance System (YRBS) national survey from 1993 to 2011. It contains 147,374 observations of high school students in 47 states. Various health behaviors, as well as demographic information on gender, age, grade, and race, are documented.[11]

Unfortunately, the YRBS survey does not contain some potential determinants of health behaviors such as family income and parental education. However, the absence of these variables would only be a problem if there were different trends in these factors across

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In fact, during our sample period, only 9 states changed the English/language arts courses requirements, and 24 states changed the social studies requirements. From the graduating class of 1993 to that of 2013, the average HSGR (unweighted) across states has gone up from 2.32 to 2.91 (in Carnegie units) for mathematics and from 2.10 to 3.26 for science, compared with an increase of HSGR from 3.85 to 3.97 for English/language arts and an increase from 2.63 to 3.02 for social studies. Data sources: Table 152, DES1993 and Table 234.30, DES2013 (https://nces.ed.gov/programs/digest/). Considering the lack of variation in English/language arts and social studies requirements over time, we do not include them in our regressions.

[11] The response rate in YRBS varies by year. For example, 1993 YRBS has an overall response rate of 70% (78% school response rate * 90% student response rate); 2003 YRBS has 67% (81% * 83%); while 2013 YRBS has 68% (77% * 88%). Throughout 1993 to 2011, no YRBS survey has an overall response rate lower than 60%, and all YRBS surveys have a student response rate of over 80%. See: 1993-2013 YRBS Data User's Guide (https://www.cdc.gov/healthyyouth/data/yrbs/data.htm).

treatment and control states. To address concerns about omitted variable bias, we add various state-specific economic and policy variables (and state-specific linear time trends, in some cases) as control variables in our models. These variables include median income, unemployment rate, expenditures per pupil for public elementary and secondary education (which will be referred to as "public school per pupil spending"), dummies indicating whether states require a high school exit exam, the pupil-teacher ratio in public elementary and secondary schools (which will be referred to as "pupil-teacher ratio"), cigarette tax, beer tax, and medical marijuana legalization status from 1993 to 2011. The data sources are described in Appendix 3. These variables are included to control for financial support for education, other education reforms, the full price of substances, and differences in the economic environment across states over time.[12] The education policy covariates largely encompass those used as controls in the HSGR literature including Lillard and DeCicca (2001), Plunk et al. (2014), Federman (2007), and Goodman (2017).

Observations with at least one missing variable of interest, which include observations from a few states in which there is no state level HSGR, are dropped from our regression sample. That leaves us 116,063 total observations, including 56,110 males and 59,953 females, and 49,051 non-Hispanic white students ("white" for the purposes of this study) and 67,012 students who either identify as Hispanic or a racial category other than white ("non-white").[13]

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[12] Previous studies have shown a relationship between alcohol taxes and youth drinking (Dee, 1999; Xuan et al., 2013), cigarette taxes and youth smoking (Carpenter and Cook, 2008; DeCicca et. al., 2008; Hansen et al., 2015), marijuana legalization and youth marijuana use (Chu, 2014; Anderson et al., 2015; Pacula et al., 2015; Wen et al., 2015), and macroeconomic conditions and health (Ruhm, 1995, 2003, 2005, 2015; Ruhm and Black, 2002).

[13] The YRBS has changed its questions on race/ethnicity several times over the years. For earlier years, it is

Summary statistics of the data we use are shown in Table 2.[14] The vast majority of students are between 14 and 18 years old. Students from each grade make up about a quarter of the total sample. Black and Hispanic students are over-sampled relative to white students.

The percentage of students who use each of alcohol, cigarettes, and marijuana is striking. About 45% of high school students consumed alcohol in the last 30 days. The proportions of students that binge drank, smoked, and used marijuana in the last 30 days are 28%, 25%, and 22%, respectively. During our sample period, high school students engaged in an average of 1.3 days of binge drinking, 2.7 days of drinking, 3.7 days of smoking, and 3.2 occasions of marijuana use in the last 30 days. Male students were more likely to consume these substances than females, and they tended to do so more frequently. As documented in other studies (Barnes and Welte, 1986; O'Malley and Johnston, 2002; Miller et al., 2007), white students tend to engage in higher levels of risky health behaviors than do non-white students.[15]

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impossible to distinguish Hispanic white students from all Hispanics. Roughly 85% of students in our "non-white" category are black or Hispanic or both.

[14] Observations are weighted to be representative at the state level (the weighting method is discussed in detail in the next section). The summary statistics of the unweighted data are very similar to the weighted values and available upon request. After dropping observations with missing variables, t-tests show that students in our regression sample tend to have healthier behaviors compared with the students dropped from our sample. The comparison details are shown in Appendix 4.

[15] Note that marijuana use is measured in "occasions" instead of "days" due to the YRBS survey design. Drinking days, binge drinking days, smoking days, and marijuana use occasions (in last 30 days) are categorical variables. In the YRBS national survey, students report their health behaviors by answering multiple choice questions. For example, students are asked "During the past 30 days, on how many days did you have at least one drink of alcohol?" and to choose from "A) 0 days, B) 1 or 2 days, C) 3 to 5 days, D) 6 to 9 days, E) 10 to 19 days, F) 20 to 29 days, and G) All 30 days". We take the midpoint of each group as an approximation of the actual drinking

III. EMPIRICAL METHOD

The empirical specification of our model is:

$$HB_{ist} = \alpha HSGR_{ist} + X_{ist}\beta + S_{st}\gamma + \delta_s + \tau_t + \epsilon_{ist}. \quad (1)$$

In this model, HB_{ist} is individual i 's health behavior in state s at time t. $HSGR_{ist}$ represents the math and science HSGR the individual faces as defined in the previous section, which depends on her state and predicted graduation year. X_{ist} contains all individual-level control variables including dummies for gender, age, grade, and race. S_{st} stands for all state-level control variables including median income, public school per pupil spending, pupil-teacher ratio, unemployment rate, beer tax, cigarette tax, a state high school exit exam dummy, and medical marijuana legalization status dummies. δ_s and τ_t are state and year dummies, respectively. Our assumption for identifying α is the typical DID assumption that changes to HSGR within a state over time are exogenous to youths' health behavior decisions.

As shown in Table 2, we have four measures of drinking behavior, including "did binge drink in last 30 days (1 if yes, 0 if no)", "did drink in last 30 days (1 if yes, 0 if no)", "number of days binge drinking in last 30 days", and "number of days drinking in last 30 days"; two measures of smoking behavior, including "did smoke in last 30 days (1 if yes, 0 if no)" and "number of days smoking in last 30 days"; and finally, two measures of marijuana use,

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days when running OLS models (e.g., if a student chose the answer D, we label this student as having 7.5 drinking days in the last 30 days). The same method applies to other categorical measures of substance use. Alternatively, we analyze these categorical responses in ordered probit models and find similar results to our baseline OLS ones (results available upon request).

including "did use marijuana in last 30 days (1 if yes, 0 if no)" and "times marijuana used in last 30 days". We use OLS to construct our baseline estimates in line with most previous studies, but we also use discrete choice models, the results of which are consistent with those found in the body of the paper and are available upon request.

There are two major issues regarding the YRBS national survey data. First, the data is not representative at the state level, and the number of students surveyed from each state varies considerably across cohorts. Because of that, we weight our sample so that observations in each state-year pair are representative of the state's share of national public high-school students, an approximation of the proportion of total high-school students in each state among all high-school students in the nation.[16] In other words, the weight for each observation is calculated by

$$Weight_{ist} = \frac{State\ \ Public\ \ High\ \ School\ \ Enrollment_{st}}{National\ \ Public\ \ High\ \ School\ \ Enrollment_t \times Observations_{st}}.$$ [17]

[16] The YRBS dataset contains a weighting variable itself, which is calculated based on student sex, race/ethnicity, and grade to address nonresponse and the oversample of black and Hispanic students in order to be representative at the national level, but not at the state level. In contrast, our weighting method produces a representative dataset at the state level in terms of the state's share among national students so that it can be used to produce state-level estimates. However, our method is not able to adjust for nonresponse or demographic factors. As a result, blacks and Hispanics remain over-sampled using our weighting method. Since our main goal is to estimate state level policy effects, we believe our weighting method is more appropriate. The oversampling by race/ethnicity is addressed by adding these variables into the regressions as control variables.

[17] We also examined all models reported below using the unweighted YRBS data. For drinking and binge drinking, point estimates have the same sign, remain statistically significant in most cases, but are somewhat smaller in absolute value compare with those from the weighted data. Estimates for smoking and marijuana use are not significantly different from zero. These results are available upon request.

The second issue is the potential for sample selection. As mentioned above, some previous studies show that increases in HSGR may unintentionally raise high-school dropout rates. Since YRBS data only contains high-school students who were currently enrolled, we need to address the concern that the effects of HSGR on substance use identified in our study are due to higher dropout rates of students with riskier behaviors.[18] Most studies that use YRBS data do not explicitly address this issue (Carpenter and Cook, 2008; Anderson, 2010; DeSimone, 2010; Disney et al., 2013; Xuan et al., 2013; Anderson and Elsea, 2015; Hansen et al., 2015). However, this selection issue is crucial in this paper because unlike previous studies, in which the covariates are fairly unlikely to affect the dropout rate, HSGR more plausibly does so.

To examine how important this issue might be to our analysis, we examine the effect of HSGR on dropout among 14-18 year-olds using a similar set of covariates described above. This analysis requires the use of another dataset since the YRBS only interviews students; thus, we use 1990 and 2000 U.S. Census and 2001-2013 American Community Survey (ACS) data. We code "dropout" as "1" if a 14-18 year-old has had at least some high school education, is not currently enrolled in school, and does not have a high-school diploma or equivalent. As with our YRBS analysis, the graduation year is assumed to be the current year if students are 18, one year later if students are 17, two years later if students are 16, etc.[19]

Table 3 shows the effects of HSGR on dropouts for the whole time period from year 1990

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[18] One study by Bray et al. (2000) shows that marijuana use is positively related with dropping out from high school.

[19] The ACS does not report the exact month a survey was conducted, making our matching of HSGR less accurate than in YRBS. We also conducted our analysis assuming the graduation year is year+17-age (with 18 year-olds dropped from the sample). The results again indicate no effect of HSGR on dropout and are available upon request.

to 2013 (from graduation year of 1990 to 2014) in column (1), for only graduation years up to and including 2004 in column (2) and for only graduation years after 2004 in column (3). We do not find evidence that dropout is affected by HSGR over the whole sample period. This appears to be inconsistent with Lillard and DeCicca (2001) (which uses the 1980 and 1990 U.S. Censuses) and Plunk et al. (2014) (which uses the 1990 and 2000 U.S. Censuses and the 2001-2011 ACS data but restricts the sample to those graduating between 1980 and 1999), who find HSGR has a positive impact on dropout. When we restrict our sample to roughly the first half of graduating classes (2004 and before), we indeed find a statistically significant (at the 5% level) positive effect of HSGR on dropout, though it is economically small: a one unit increase in HSGR raises the likelihood of dropout by 0.2%. This is in line with the estimates in Lillard and DeCicca (2001) and Plunk et al. (2014), which range from roughly 0.15% to 0.4% for a comparable change in HSGR. These findings suggest that sample selection due to HSGR's effect on dropout is at most very limited during our sample period; furthermore, the difference in our results and those of previous studies is likely due at least in part to differences in sample periods across studies.

Carpenter and Stehr (2008) also propose a remedy for the problem of selection due to school absence by restricting the sample to only students who are 16 or under. Over our sample period, students were required to stay in school by law until at least 16 years of age in every state, with many states requiring even longer attendance.[20] Using U.S. Census and American

[20] In 1994, 33 states required attendance until age 16, 9 states until age 17, and 9 states until age 18. In 2013, 22 states required attendance until age 16, 9 states until age 17, and 19 states until age 18. Source: Digest of Education Statistics, National Center for Education Statistics (For compulsory schooling ages data in 1994, see:

Community Survey data, we calculated school enrollment rates and dropout rates by different age groups for the years 1990, 2000, and 2010. The results, presented in Tables 4a and 4b, show clear gaps in the enrollment rates between 16 years old and 17 years old and that dropouts before age 17 are fairly rare. Although the enrollment rate is not 100% even for younger individuals, restricting our sample to the younger subgroup mitigates the concern that our results are being driven by sample selection. Thus, our strategy for dealing with this issue using YRBS is to only include students who are under 17 in the same regressions as in our main analysis. We show in Section V that selection due to dropout cannot account for our main results.

IV. MAIN RESULTS

Tables 5 to 8 show the results of our baseline regressions, with the effects of HSGR on binge drinking and drinking listed in Tables 5 and 6, and the effects of HSGR on smoking and

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https://nces.ed.gov/programs/digest/d95/dtab148.asp; for data in 2013, see: https://nces.ed.gov/programs/digest/d13/tables/dt13_234.10.asp). Although in some states, like Florida, students are allowed to drop out when they turn 16, extra requirements like parents' consent and filing formal declaration of intent to terminate school enrollment with the school district are needed if they decide to drop out at age 16, making it harder than doing so at 17 or older. Source: Florida Department of Education (http://www.fldoe.org/how-do-i/attendance-enrollment.stml).

It would be useful to control for the effects of the compulsory schooling laws in the regressions. However, we can only find compulsory schooling law information for every even year during our sample period. Considering YRBS cohorts are from odd years, matching these two sets of data is problematic. Because of this, we do not add compulsory schooling law dummies in the baseline regressions or in the main robustness check regressions. However, we did run the regressions with the nearest year law dummies added later, and the results are very similar to those without them and are available upon request.

marijuana use listed in Tables 7 and 8, respectively. All models are estimated using OLS with standard errors clustered at the state level. Column (1) of all four tables shows the results from models estimated with state and year fixed effects and individual-level controls but without state-level controls. State-level control variables are added in column (2). Finally, in column (3), both state-level controls and state-specific linear time trends are included.[21]

Tables 5 and 6 show consistently negative impacts of HSGR on drinking across all measures and all subgroups: it decreases the probability of binge drinking and drinking as well as the number of drinking days and binge drinking days. As for the subgroup results, the magnitudes of HSGR's effects on drinking and binge drinking are larger among males than females and larger among non-white students than white students for every measure and every specification.

Most results in columns (1) and (2) of Table 5 are statistically significant (at the 10% level or better), except for the effects on female binge drinking days in column (2) and the effects on white binge drinking days in both columns. A similar pattern can be seen in Table 6, except that results for female drinking days and all results for whites are insignificant. The fact that the inclusion of a rich set of state characteristics does little to affect the HSGR point estimates is reassuring, since if HSGR were highly correlated with other observable state characteristics that affect drinking, it might be the case that it was also correlated with unobserved ones as well.

After adding state-specific linear time trends (column (3)), some results lose significance.

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[21] We also tried models in which all controls except state, year, and grade fixed effects were removed, and the results were very similar to those in column (1). These results are available upon request.

Point estimates in columns (3) are smaller (usually modestly) in absolute value in most cases but still remain negative, and standard errors increase. These changes are because the state-specific time trends are correlated with HSGR and thus likely pick up some of the effects of HSGR in addition to any pre-existing trends across states (Wolfers, 2006). Because of this, the results after adding state-specific time trends are not necessarily more trustworthy than those without them. However, it is worth noting that in the cases of both binge drinking behaviors and number of drinking days, point estimates remain significant at the 10% level or better for the total sample and non-white students in particular.

Throughout the rest of the paper, we treat the specification shown in column (2) as our preferred specification, which includes state dummies, year dummies, individual controls and state controls. Our state level controls include a broad range of factors that other papers in the HSGR literature have used to control for state-level heterogeneous trends and the possibility that HSGR changes are made in conjunction with other educational reforms.

Using our preferred specification with the total sample, the point estimates of the effect of HSGR on "did binge drink in last 30 days" and "did drink in last 30 days" are both -0.016, meaning that one additional unit in math and science HSGR reduces the probability of both binge drinking and drinking by 1.6%. These reductions constitute 5.8% of the students who did binge drink and 3.5% of the students who did drink in our sample.[22] For male students, both estimates are -0.018, constituting 5.8% of males who did binge drink and 3.8% of males who did drink. For female students, the reductions are 1.4% for binge drinking and 1.3% for

[22] These two numbers are calculated as 100*(0.016/0.275) = 5.8 percent for binge drinking and 100*(0.016/0.454) = 3.5 percent for drinking. Percentage changes discussed below are calculated using the same method.

drinking, constituting 5.8% of females who did binge drink and 3.0% of females who did drink. For white students, one additional unit of HSGR reduces the probability of binge drinking by 1.2%, or 3.7% at the mean; while for non-white students, the reduction is 1.9%, or 8.1% at the mean. Furthermore, one additional unit of HSGR reduces the probability of drinking of non-white students by 2.0%, or 4.7% at the mean.

Tables 5 and 6 also show that one additional unit of HSGR reduces binge drinking days by 0.120 and drinking days by 0.174 for the total sample, which is a 9.5% drop at the mean in binge drink days and a 6.4% drop at the mean in drinking days. The estimated declines among males are 6.7% at the mean in binge drinking and 5.6% at the mean in drinking; while the estimated declines among non-white students are 10.9% at the mean in binge drinking days and 7.3% at the mean in drinking days (effects that are insignificant at conventional levels are not discussed here).[23]

Table 7 reports the effects of HSGR on smoking and Table 8 on marijuana use. The results are consistent with the results for drinking regarding the estimated signs but different in terms of statistical significance. From Table 7, the effect of HSGR on smoking is only significant in one specification for the total sample ("did smoke in last 30 days", column (2)). The effects on males are significant in column (2) for both smoking measures and are larger than those for females, and the effects on non-white students are significant for "did smoke in last 30 days."

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[23] We have also estimated the models for each gender by race grouping. Perhaps due to a smaller sample size for each group (24,421 white males, 31,689 non-white males, 24,630 white females, and 35,323 non-white females), the results are not as precisely estimated as our main results. But these results also indicate that the effects of HSGR are largest among non-white males. These results are available upon request.

Based on the results shown in column (2), a one-unit increase in HSGR decreases the likelihood of smoking in the last 30 days by 1.0% for the total sample and 1.3% for male students and non-white students, which are equivalent to 4.0% of the total sample who did smoke, 4.8% of males who did smoke, and 6.2% of non-white students who did smoke. Furthermore, one additional unit of HSGR reduces smoking days by 0.168 for male students, which is a 4.0% drop at the mean among this group.

Table 8 shows the effects of HSGR on marijuana use. No results are significant for either measure of marijuana use among the total sample. The results do suggest that higher HSGR leads to lower marijuana use among non-white students: a one unit increase in HSGR decreases the likelihood of marijuana use by 1.5%, or 6.8% of students who did use marijuana; and it reduces marijuana use occurrences by 0.294, or a 9.1% reduction at the mean among non-white students).[24,25]

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[24] Despite the lack of statistical significance, the estimated signs for marijuana use are consistent with those of drinking and smoking. This insignificance could come from under-reporting. Although all three substances are illegal for youth, marijuana use is arguably accompanied with harsher social criticism. Two studies also point out that people tend to under-report marijuana use (Mensch and Kandel, 1988; Bessa et al., 2010).

[25] In addition to drinking, smoking and marijuana use, YRBS also has measures of cocaine use including "did use cocaine in last 30 days" and "times cocaine used in last 30 days". It also records the use of heroin, methamphetamines, and other drugs. But those measures are based on lifetime consumption (instead of consumption in the past month), which are not suitable for this study. There are 4.5% of high school students who report using cocaine in the last 30 days. Appendix 5 shows the results with cocaine use as the dependent variable. YRBS also ask questions about chewing tobacco, snuff, or dip. There are very few students who consume tobacco in such forms. Nevertheless, we have combined these behaviors with smoking and constructed a variable for "consumed any tobacco product during the last 30 days (1 if "yes", 0 if "no")". The effects of HSGR on "any tobacco use" are generally negative but insignificant for the total sample and all subgroups. These results are

The results throughout this section suggest that the largest effects of HSGR on risky behaviors are on male students and non-white students. This is consistent with several facts. First, since we cannot condition on parental income or neighborhood characteristics, these results likely indicate that the effects of HSGR are larger for less affluent students attending poorer schools (since race and ethnicity are strongly correlated with these factors; see Reardon et al., 2008). This seems plausible given state graduation requirements are more likely to bind for such students. Furthermore, recent evidence from the HSGR literature find that effects on course-taking, and later earnings are largest for minority students, particularly males (Goodman, 2017).

V. YOUNGER SUMSAMPLES

As discussed above, we also adopt the approach of Carpenter and Stehr (2008) and restrict the sample to students who are 16 years old and under to conduct our robustness check. HSGR may affect younger students' health behaviors through the time constraint because students facing higher HSGR tend to enroll in higher level courses from the beginning of high school (Schiller and Muller, 2003). Since dropout rates are very low for students in this age category, the results are less likely to be contaminated by sample selection bias due to dropout. All methods and the control variables remain the same as in the previous section. In our database, 66,527 observations out of 116,063, or 57 percent of the total observations, are under 17 years old. 56 percent of male students and 59 percent of females are under 17, while 59 percent of whites and 56 percent of non-whites are under 17.

Table 9 shows the effects of HSGR on health behaviors discussed in the last section for

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available upon request.

the younger subsample using our preferred specification (column (2) in Tables 5-8). The results for binge drinking and drinking behaviors are consistent with those in Tables 5 and 6. Nearly all the estimated signs are negative. Point estimates are typically only slightly smaller in absolute value compared with those for all ages. Regarding smoking and marijuana use, point estimates experience a larger reduction in magnitude and loss of significance relative to Tables 7 and 8 (though coefficients generally remain negative for the total sample, males, and non-whites). This may be due to sample selection as described previously, though given our results on high-school dropout (Table 3), we believe it is more likely due a combination of true larger effects for older students and a reduced sample size. Nevertheless, the smoking and marijuana results should be interpreted with caution, and we place our greatest confidence in the estimated effects of HSGR on (binge) drinking.[26]

VI. PLACEBO ANALYSIS

To test the validity of our DID design, we construct a placebo test in this section. The goal is to mitigate the concern that the trends in risky behavior would have been different between the treatment states and control states even in the absence of treatment. Traditionally, such tests are performed under an event-study framework by replacing the treatment variable with cohort-specific dummy indicators (relative to treatment) and examining the effects of each indicator. However, our data structure prevents us from adopting the typical event study framework,

[26] We also examined the robustness of our results by using probit models for binary dependent variables and ordered probit models for multinomial dependent variables. Regressions are performed both for all students and those under 17 using our preferred specification, which includes individual controls, state controls, state and time dummies. The results are consistent with their counterparts in Tables 5 through 8 and are available upon request.

because HSGR is a non-binary policy and a substantial number of states (20) changed their HSGR more than once from 1993 to 2013. Simon (2016) proposes to use only large policy changes (in his case, excise tax hikes) to reduce the events that are considered to be treatments, and he limits the sample period to a certain time window to guarantee there is only one discrete event per state. Unfortunately, this method is not suitable for us given that most HSGR changes are the same size (1 or 2) and distributed fairly uniformly throughout our sample time frame.

In our placebo test, we construct 10 different placebo HSGR treatments, which are essentially 6 leads (by shifting the true HSGR 1-6 cohorts forward) and 4 lags (by shifting the true HSGR 1-4 cohorts backward) of the true HSGR. In other words, by imposing the "n cohorts ahead (after)" placebo HSGR, students of graduation year t are being treated with the HSGR of graduation year $t \pm n$.[27]

If our DID method is valid, we expect the effects of these placebo HSGR to be smaller than the effects of the true HSGR because of mismatching. In addition, the further a placebo HSGR deviates from the true HSGR, the more severe the mismatch is, and thus the smaller the effect of the placebo HSGR should be. On the contrary, if the placebo HSGR effects do not show such a pattern, it would call our identification strategy into question. For example, if the effects got stronger as the placebo HSGR was shifted forward, we would have reason to believe that there were different pre-treatment trends between control and treatment groups caused by

[27] We choose to use 4 lags because our HSGR data begins with the graduating cohort of 1989, 4 years prior to our first cohort (graduation year 1993) in the YRBS data. To construct the 6 HSGR leads, for those observations that would receive a placebo HSGR for a graduation year beyond 2015, we simply assign them the last HSGR available (graduation year 2015). As illustrated below, with this number of leads and lags, we find that the effects of (placebo) HSGR diminish as the placebo HSGR gets farther from the true HSGR.

unobserved differences between the two, which would mean our baseline estimates of HSGR on risky health behaviors are likely biased upward (in absolute value).[28]

We only perform the placebo test on drinking behaviors since these behaviors are most affected by HSGR in our baseline models. Table 10 shows the effects of placebo HSGR's on these behaviors under our preferred specification for the full sample. n cohorts ahead (after) means the placebo HSGR is generated by shifting the true HSGR forward (backward) by n cohorts. The effects of the true HSGR are the same as shown in columns (2) of Tables 5 and 6 (full sample) and indeed are stronger than any placebo effect in terms of both magnitude and significance. The effects of placebo HSGR's generally get smaller the further they are from the true HSGR, supporting the validity of our DID design.

VII. CONCLUSION

We study the effects of high-school graduation requirements (HSGR) regarding mathematics and science on high-school students' health behaviors, including drinking, smoking, and marijuana use. We find that an increase in math/science HSGR has significant negative impacts on the alcohol consumption of high-school students, especially males and non-white students. These results are consistent with Goodman (2017), who finds the strongest effects of state math requirements on math course taking and later earnings to be among minority students. Estimated effects on smoking and marijuana use are consistently negative though often not statistically different from zero, especially among our younger subsample.

[28] It is important to note that our placebo HSGR's will still pick up some effects of the true HSGR because they will still partially coincide with the true HSGR, unlike a traditional event-study design in which each cohort/bin indicator "turns on" just once at a certain cohort/bin. This is one shortcoming of this design.

The potential selection issue due to high-school dropout is addressed. We find no evidence of an effect of HSGR on the probability of high-school dropout using the Census and ACS over our sample period. Furthermore, restricting our sample to individuals under 17 years old (among whom enrollment rates are very high), we find very similar results to our main findings for (binge) drinking behaviors.

There are limitations of our study. Perhaps the most important is that we cannot, with the data used in this paper, assess which mechanisms contribute to the curbing of risky behavior when HSGR changes. This is an important subject for future research. We also note the possibility that missing data/information on some students gives us a distorted view of how HSGR affects risky behavior for the full high-school population: participation in YRBS surveys is roughly 70% over our sample period, and about 13% of the observations in the data are excluded from the regression analysis due to missing information on health behaviors or covariates. More work is needed to determine how missing values on youth risk behaviors affect estimates such as the ones in this paper.

Our results have implications for the role education plays in fostering healthy behaviors and deterring criminal activity (since underage use of alcohol is illegal). Many papers in this literature rely on differences in compulsory schooling laws by cohort and/or location to identify the effects of education on health and criminal behavior (Adams, 2002; Lleras-Muney, 2005; Oreopoulos, 2007; Chou et al., 2010; Machin et al., 2011; Anderson, 2014; Güneş, 2015). There are two drawbacks to this. The first is that compulsory schooling laws affect the quantity of schooling (for some youths) but likely have little effect on high-school stringency. Another drawback is that since treatment effects are identified only for those whose behavior changes in the face of a change in compulsory schooling laws (Angrist and Krueger, 1991), we do not know how schooling affects risky behaviors for youths who are unaffected by changes in these laws. Our paper contributes on both fronts by examining how academic stringency (in the form of higher HSGR) affects behavior, which policy is likely to affect at least some youths who would have graduated from high school regardless of the compulsory schooling law they face.

As mentioned above, there is empirical evidence suggesting that different substances may be substitutes among youths. For example, raising the price of alcohol by increasing its excise tax or raising the minimum drinking age is likely to decrease alcohol consumption but could increase the consumption of marijuana. Since HSGR is a constraint imposed on time, it does not seem to induce a shift from one substance to another in this way. Our results suggest that HSGR could be an attractive policy alternative to curb youth drinking, and that perhaps high-school course requirements or curriculum are an understudied means of discouraging risky behavior. However, since an increase in HSGR imposes a stricter time constraint, it is possible that it could reduce students' sleeping or exercise. The limitation of our dataset prevents us from examining the effects of HSGR on these health behaviors. Further work could explore whether the benefits of higher HSGR outweigh its costs overall.

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Figure 1: Total Mathematics and Science HSGR for Graduating Class of 1993, 2003, and 2013 (in Carnegie Units)

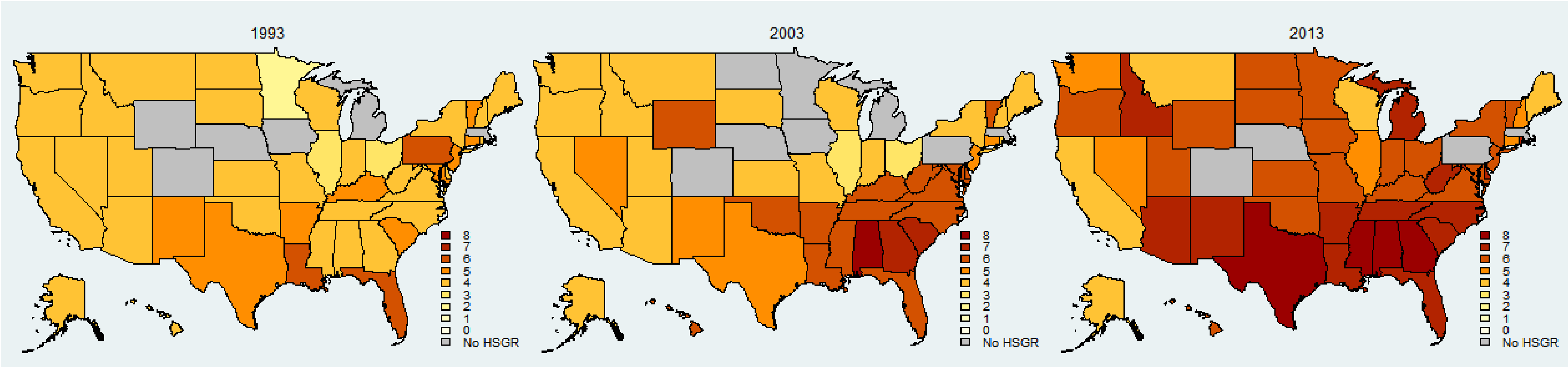

Table 1: Mathematics and Science HSGR for Graduating Class of 1993, 2003, and 2013 (in Carnegie Units)

	1993		2003		2013	
States	Math	Science	Math	Science	Math	Science
AL	2	2	4	4	4	4
AK	2	2	2	2	2	2
AZ	2	2	2	2	4	3
AR	3	2	3	3	4	3
CA	2	2	2	2	2	2
CO	---	---	---	---	---	---
CT	3	2	3	2	3	2
DE	2	2	3	3	4	3
DC	2	2	3	3	4	4
FL	3	3	3	3	4	3
GA	2	2	4	3	4	4
HI	2	2	3	3	3	3
ID	2	2	2	2	4	3
IL	2	1	2	1	3	2
IN	2	2	2	2	3	3
IA	---	---	---	---	3	3
KS	2	2	2	2	3	3
KY	3	2	3	3	3	3
LA	3	3	3	3	4	3
ME	2	2	2	2	2	2
MD	3	2	3	3	3	3
MA	---	---	---	---	---	---
MI	---	---	---	---	4	3
MN	1	1	---	---	3	3
MS	2	2	3	3	4	4
MO	2	2	2	2	3	3
MT	2	2	2	2	2	2
NE	---	---	---	---	---	---
NV	2	2	3	2	3	2
NH	2	2	2	2	3	2
NJ	3	2	3	2	3	3
NM	3	2	3	2	4	3
NY	2	2	2	2	3	3
NC	2	2	3	3	4	3
ND	2	2	---	---	3	3
OH	2	1	2	1	3	3
OK	2	2	3	3	3	3
OR	2	2	2	2	3	3
PA	3	3	---	---	---	---
RI	2	2	2	2	4	3

SC	3	2	4	3	4	3
SD	2	2	2	2	3	3
TN	2	2	3	3	4	3
TX	3	2	3	2	4	4
UT	2	2	2	2	3	3
VT	2.5	2.5	3	3	3	3
VA	2	2	3	3	3	3
WA	2	2	2	2	3	2
WV	2	2	3	3	4	3
WI	2	2	2	2	2	2
WY	---	---	3	3	3	3

Notes: "---" means that the graduating requirements are decided by local boards, so there is no state-level HSGR. In our regressions, we exclude students living in states with no state-level requirements.

Table 2: Descriptive Statistics

	Total Sample		by Gender				by Race			
			Male Only		Female Only		White Only		Non-White Only	
	No. of Obs. =116,063		No. of Obs. =56,110		No. of Obs. =59,953		No. of Obs. =49,051		No. of Obs. =67,012	
Variables	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Health Behaviors										
did binge drink in last 30 days (1 if yes, 0 if no)	0.275	0.447	0.311	0.463	0.241	0.428	0.328	0.469	0.234	0.423
did drink in last 30 days (1 if yes, 0 if no)	0.454	0.498	0.470	0.499	0.438	0.496	0.494	0.500	0.422	0.494
did smoke in last 30 days (1 if yes, 0 if no)	0.252	0.434	0.273	0.446	0.231	0.421	0.304	0.460	0.210	0.407
did use marijuana in last 30 days (1 if yes, 0 if no)	0.222	0.416	0.260	0.439	0.185	0.388	0.224	0.417	0.220	0.414
number of days binge drinking in last 30 days	1.269	3.409	1.721	4.194	0.833	2.343	1.358	3.103	1.198	3.632
number of days drinking in last 30 days	2.71	5.586	3.345	6.556	2.099	4.371	2.774	4.943	2.659	6.046
number of days smoking in last 30 days	3.691	8.735	4.204	9.277	3.197	8.15	4.978	9.987	2.673	7.445
times marijuana used in last 30 days	3.209	9.184	4.445	10.91	2.017	6.931	3.183	9.040	3.229	9.296
High School Graduation Requirements (HSGR)										
minimum total required units on math and sciences	5.017	1.279	5.010	1.286	5.023	1.272	4.912	1.287	5.100	1.267
Individual Controls										
male	0.491	0.500	1	0	0	0	0.501	0.500	0.483	0.500
age	16.13	1.279	16.18	1.293	16.08	1.263	16.09	1.257	16.16	1.295
9th grade	0.236	0.425	0.234	0.424	0.238	0.426	0.239	0.427	0.234	0.423
10th grade	0.242	0.428	0.241	0.428	0.244	0.429	0.246	0.431	0.239	0.426
11th grade	0.255	0.436	0.254	0.435	0.256	0.436	0.256	0.437	0.254	0.435
12th grade	0.252	0.434	0.252	0.434	0.252	0.434	0.250	0.433	0.254	0.435
white	0.442	0.497	0.451	0.498	0.433	0.495	1	0	0	0
black	0.197	0.398	0.184	0.388	0.21	0.407	0	0	0.353	0.478
other races	0.361	0.480	0.365	0.481	0.358	0.479	0	0	0.647	0.478
Hispanic	0.271	0.445	0.270	0.444	0.272	0.445	0	0	0.486	0.500
State Controls										
median income (USD)	23,237	2,802	23,258	2,789	23217	2,815	23,180	2,715	23,282	2,869
public school per pupil spending (USD)	4,426	1,095	4,435	1084	4418	1109	4,519	1,124	4,354	1,069
unemployment rate (%)	6.263	2.106	6.27	2.125	6.255	2.088	6.190	2.055	6.320	2.144
medical marijuana legalization (1 if legal, 0 if illegal)	0.191	0.393	0.186	0.389	0.196	0.397	0.155	0.362	0.219	0.414
cigarette tax (cents per pack)	36.6	29.5	36.66	29.13	36.54	29.86	36.23	32.36	36.89	27.03
beer tax (cents per gallon)	13.48	8.931	13.34	8.828	13.61	9.027	12.56	8.638	14.20	9.091
state high school exit exam	0.337	0.473	0.330	0.470	0.344	0.475	0.260	0.439	0.398	0.489
pupil-teacher ratio	16.87	2.947	16.87	2.930	16.87	2.964	16.54	2.770	17.13	3.055

Notes: Observations are weighted to be representative at the state level. Weighting method is provided in the empirical method section. Monetary figures are deflated (CPI1982-1984=100).

Table 3: The Effect of HSGR on High School Dropouts

	(1)	(2)	(3)
	whole time period	graduation year<=2004	graduation year>2004
total sample	0.0005	0.0021**	-0.0002
	(0.0005)	(0.0010)	(0.0005)
	N=3,400,851	N=1,790,650	N=1,610,201
male only	0.0004	0.0019	-0.0000
	(0.0005)	(0.0013)	(0.0006)
	N=1,748,724	N=919,932	N=828,792
female only	0.0006	0.0022*	-0.0003
	(0.0006)	(0.0013)	(0.0006)
	N=1,652,127	N=870,718	N=781,409
white only	0.0004	0.0023	-0.0005
	(0.0007)	(0.0014)	(0.0005)
	N=2,451,775	N=1,302,005	N=1,149,770
non-white only	0.0001	0.0011	0.0003
	(0.0012)	(0.0027)	(0.0013)
	N=949,076	N=488,645	N=460,431

Notes: 1. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2. Data source: 1990 US Census 5% public use sample, 2000 US Census 5% public use sample and 2001-2013 American Community Survey from Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. Integrated Public Use Microdata Series (IPUMS): Version 6.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2015.

3. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weights are generated by IPUMS to indicate how many persons in the U.S. population are represented by a given person. Control variables include state dummies, individual controls, state level controls, and graduation year dummies. Individual controls include age, gender, race, and inflation-adjusted family income. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, and a state high school exit exam dummy.

4. Individuals ages 14-18 are included in the regressions. High school dropout is defined as having had some level of high school education, not being currently enrolled, and not having a high school degree or GED. The graduation year is assumed to be the current year if students are 18, 1 year later if students are 17, 2 years later if students are 16, etc.

Table 4a: School Enrollment Percentage by Age

age	year 1990	year 2000	year 2010
13	96.48	98.84	98.39
	(0.18)	(0.11)	(0.13)
14	96.29	98.69	98.16
	(0.19)	(0.11)	(0.13)
15	95.63	97.92	97.98
	(0.20)	(0.14)	(0.14)
16	93.32	95.62	97.1
	(0.25)	(0.20)	(0.17)
17	88.12	90.85	94.39
	(0.32)	(0.29)	(0.23)
18	73.52	75.08	81.02
	(0.44)	(0.43)	(0.39)

Notes: Standard errors are shown in parentheses. Sample sizes are 2,388,466 for year 1990, 2,704,322 for year 2000, and 2,956,263 for year 2010. Data source: 1990 US Census 1% public use sample, 2000 US Census 1% public use sample and 2010 American Community Survey from Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. Integrated Public Use Microdata Series: Version 6.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2015.

Table 4b: High School Dropout Percentage by Age

age	year 1990	year 2000	year 2010
13	0.11	0.08	0.06
	(0.03)	(0.03)	(0.02)
14	0.81	0.21	0.47
	(0.09)	(0.05)	(0.07)
15	2.19	0.8	1.14
	(0.15)	(0.09)	(0.11)
16	4.46	2.36	1.85
	(0.21)	(0.15)	(0.13)
17	7.97	5.15	3.15
	(0.27)	(0.22)	(0.17)
18	10.89	10.4	5.57
	(0.31)	(0.31)	(0.23)

Notes: Standard errors are shown in parentheses. High school dropout is defined as having had some level of high school education, not being currently enrolled, and not having a high school degree or GED. Sample sizes are 2,388,466 for year 1990, 2,704,322 for year 2000, and 2,956,263 for year 2010. Data source: 1990 US Census 1% public use sample, 2000 US Census 1% public use sample and 2010 American Community Survey from Steven Ruggles, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. Integrated Public Use Microdata Series: Version 6.0 [Machine-readable database]. Minneapolis: University of Minnesota, 2015.

Table 5: The Impact of Total Math and Science HSGR on Binge Drinking

Dependent Variables		(1)	(2)	(3)
did binge drink in last 30 days (1 if yes, 0 if no)				
Total Sample		-0.014***	-0.016***	-0.012**
		(0.004)	(0.005)	(0.006)
by Gender	Male Only	-0.016**	-0.018**	-0.015*
		(0.006)	(0.007)	(0.009)
	Female Only	-0.011**	-0.014***	-0.007
		(0.005)	(0.004)	(0.005)
by Race	White Only	-0.012**	-0.012**	-0.009
		(0.005)	(0.005)	(0.006)
	Non-White Only	-0.014**	-0.019***	-0.015**
		(0.006)	(0.005)	(0.007)
number of days binge drinking in last 30 days				
Total Sample		-0.081***	-0.120***	-0.088**
		(0.029)	(0.043)	(0.040)
by Gender	Male Only	-0.082*	-0.115**	-0.065
		(0.042)	(0.046)	(0.050)
	Female Only	-0.040*	-0.030	-0.020
		(0.022)	(0.023)	(0.028)
by Race	White Only	-0.032	-0.031	-0.015
		(0.029)	(0.028)	(0.036)
	Non-White Only	-0.083***	-0.130***	-0.086*
		(0.027)	(0.033)	(0.046)
State Dummies		YES	YES	YES
Year Dummies		YES	YES	YES
Individual Controls		YES	YES	YES
State Level Controls		NO	YES	YES
State Specific Linear Time Trends		NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 116,063, including 56,110 males, 59,953 females. 49,051 are whites, and 67,012 are non-whites.

Table 6: The Impact of Total Math and Science HSGR on Drinking

Dependent Variables		(1)	(2)	(3)
did drink in last 30 days (1 if yes, 0 if no)				
Total Sample		-0.016***	-0.016***	-0.007
		(0.004)	(0.005)	(0.006)
by Gender	Male Only	-0.020***	-0.018**	-0.008
		(0.006)	(0.007)	(0.009)
	Female Only	-0.012**	-0.013**	-0.005
		(0.005)	(0.005)	(0.005)
by Race	White Only	-0.011	-0.009	-0.003
		(0.007)	(0.008)	(0.009)
	Non-White Only	-0.018***	-0.020***	-0.009
		(0.004)	(0.004)	(0.006)
number of days drinking in last 30 days				
Total Sample		-0.129***	-0.174**	-0.126**
		(0.042)	(0.065)	(0.061)
by Gender	Male Only	-0.164**	-0.188**	-0.110
		(0.065)	(0.076)	(0.089)
	Female Only	-0.048	-0.045	-0.024
		(0.041)	(0.041)	(0.046)
by Race	White Only	-0.034	-0.026	-0.022
		(0.041)	(0.038)	(0.053)
	Non-White Only	-0.149***	-0.195***	-0.118*
		(0.045)	(0.056)	(0.068)
State Dummies		YES	YES	YES
Year Dummies		YES	YES	YES
Individual Controls		YES	YES	YES
State Level Controls		NO	YES	YES
State Specific Linear Time Trends		NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 116,063, including 56,110 males, 59,953 females. 49,051 are whites, and 67,012 are non-whites.

Table 7: The Impact of Total Math and Science HSGR on Smoking

Dependent Variables		(1)	(2)	(3)
did smoke in last 30 days (1 if yes, 0 if no)				
Total Sample		-0.007	-0.010**	-0.007
		(0.005)	(0.005)	(0.005)
by Gender	Male Only	-0.011*	-0.013**	-0.008
		(0.006)	(0.006)	(0.008)
	Female Only	-0.004	-0.006	-0.005
		(0.006)	(0.004)	(0.004)
by Race	White Only	-0.001	-0.003	0.002
		(0.006)	(0.006)	(0.006)
	Non-White Only	-0.009	-0.013**	-0.016**
		(0.006)	(0.006)	(0.007)
number of days smoking in last 30 days				
Total Sample		-0.109	-0.160	-0.068
		(0.123)	(0.104)	(0.105)
by Gender	Male Only	-0.137	-0.168*	-0.031
		(0.108)	(0.095)	(0.119)
	Female Only	-0.068	-0.126	-0.047
		(0.155)	(0.128)	(0.119)
by Race	White Only	-0.019	-0.113	0.045
		(0.144)	(0.144)	(0.140)
	Non-White Only	-0.098	-0.136	-0.161
		(0.106)	(0.099)	(0.121)
State Dummies		YES	YES	YES
Year Dummies		YES	YES	YES
Individual Controls		YES	YES	YES
State Level Controls		NO	YES	YES
State Specific Linear Time Trends		NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 116,063, including 56,110 males, 59,953 females. 49,051 are whites, and 67,012 are non-whites.

Table 8: The Impact of Total Math and Science HSGR on Marijuana Use

Dependent Variables		(1)	(2)	(3)
did use marijuana in last 30 days (1 if yes, 0 if no)				
Total Sample		-0.004	-0.007	-0.008
		(0.005)	(0.005)	(0.006)
by Gender	Male Only	-0.003	-0.005	-0.005
		(0.006)	(0.007)	(0.009)
	Female Only	-0.005	-0.008*	-0.010**
		(0.005)	(0.005)	(0.005)
by Race	White Only	0.006	0.004	0.006
		(0.007)	(0.006)	(0.007)
	Non-White Only	-0.012*	-0.015**	-0.018**
		(0.006)	(0.007)	(0.008)
times marijuana used in last 30 days				
Total Sample		-0.034	-0.112	-0.189
		(0.120)	(0.102)	(0.134)
by Gender	Male Only	-0.040	-0.102	-0.188
		(0.150)	(0.136)	(0.181)
	Female Only	-0.011	-0.084	-0.132
		(0.111)	(0.096)	(0.117)
by Race	White Only	0.137	0.070	0.081
		(0.163)	(0.127)	(0.157)
	Non-White Only	-0.185	-0.294**	-0.396**
		(0.123)	(0.126)	(0.150)
State Dummies		YES	YES	YES
Year Dummies		YES	YES	YES
Individual Controls		YES	YES	YES
State Level Controls		NO	YES	YES
State Specific Linear Time Trends		NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 116,063, including 56,110 males, 59,953 females. 49,051 are whites, and 67,012 are non-whites.

Table 9: The Impact of Total Math and Science HSGR on Health Behaviors, Age<=16

		(1)	(2)	(3)	(4)
	Total Sample	by Gender		by Race	
Dependent Variables		Male Only	Female Only	White Only	Non-White Only
did binge drink in last 30 days (1 if yes, 0 if no)	-0.011***	-0.015**	-0.008*	-0.009*	-0.013**
	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)
number of days binge drinking in last 30 days	-0.112*	-0.096**	-0.004	-0.041	-0.075***
	(0.061)	(0.037)	(0.021)	(0.031)	(0.026)
did drink in last 30 days (1 if yes, 0 if no)	-0.012**	-0.017**	-0.008	-0.011	-0.013**
	(0.005)	(0.007)	(0.006)	(0.009)	(0.005)
number of days drinking in last 30 days	-0.172*	-0.168**	-0.003	-0.037	-0.135***
	(0.090)	(0.070)	(0.039)	(0.047)	(0.047)
did smoke in last 30 days (1 if yes, 0 if no)	-0.005	-0.008*	-0.002	-0.004	-0.004
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)
number of days smoking in last 30 days	-0.053	-0.067	-0.025	0.010	-0.051
	(0.089)	(0.078)	(0.133)	(0.120)	(0.086)
did use marijuana in last 30 days (1 if yes, 0 if no)	-0.004	-0.002	-0.006	0.001	-0.009
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)
times marijuana used in last 30 days	-0.035	-0.044	-0.045	0.033	-0.135
	(0.092)	(0.129)	(0.101)	(0.099)	(0.109)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Control variables include state dummies, year dummies, individual controls, and state level controls. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 66,527, including 31,233 males, 35,294 females. 28,800 are whites, and 37,727 are non-whites.

Table 10: The Effects of Placebo HSGR on (Binge) Drinking Behaviors

	(1)	(2)	(3)	(4)
	did binge drink in last 30 days (1 if yes, 0 if no)	number of days binge drinking in last 30 days	did drink in last 30 days (1 if yes, 0 if no)	number of days drinking in last 30 days
4 cohorts after	-0.009**	-0.028	-0.012**	-0.047
	(0.004)	(0.033)	(0.005)	(0.052)
3 cohorts after	-0.014***	-0.081*	-0.016***	-0.137**
	(0.004)	(0.041)	(0.004)	(0.064)
2 cohorts after	-0.013***	-0.077*	-0.016***	-0.123*
	(0.003)	(0.043)	(0.003)	(0.067)
1 cohort after	-0.015***	-0.108**	-0.015***	-0.171***
	(0.004)	(0.040)	(0.004)	(0.063)
True HSGR	-0.016***	-0.120***	-0.016***	-0.174**
	(0.005)	(0.043)	(0.004)	(0.065)
1 cohort ahead	-0.014***	-0.103**	-0.015***	-0.150**
	(0.004)	(0.043)	(0.004)	(0.060)
2 cohorts ahead	-0.012***	-0.104**	-0.011**	-0.135**
	(0.004)	(0.043)	(0.005)	(0.061)
3 cohorts ahead	-0.010**	-0.109**	-0.010*	-0.131**
	(0.005)	(0.044)	(0.005)	(0.064)
4 cohorts ahead	-0.008*	-0.097**	-0.010**	-0.101
	(0.004)	(0.048)	(0.005)	(0.064)
5 cohorts ahead	-0.002	-0.068	-0.005	-0.052
	(0.005)	(0.044)	(0.005)	(0.056)
6 cohorts ahead	-0.001	-0.055	-0.004	-0.025
	(0.005)	(0.045)	(0.005)	(0.061)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Models are estimated with state dummies, year dummies, individual controls, and state level controls. Individual controls include age dummies, grade dummies, gender, and race. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. Numbers of observations range between 115,593 and 116,063.

Appendix 1: Compiling Methodology for High School Graduation Requirements

DES publishes HSGR data for the years 1990, 1993, 1996, 1998, 2001, 2002, 2004, 2006, 2008, 2011, and 2013, but not for other years within this period. DES also reports the first graduating class that is affected by a newly implemented HSGR up until 2001 (implementation of a new HSGR generally comes several years after it is first reported in DES). We compile a dataset that contains the HSGR students in each state, cohort and grade face from the graduating class of 1989 to 2015.

For example, DES1996 shows that the math and science HSGR for Alabama is 4 units for mathematics and 4 units for science. It also documents that the first graduating class for which these requirements apply is 2000. Thus, the graduating classes before 2000 are not affected by the HSGR reported in DES1996. To recover the HSGR of the graduating classes prior to 2000, we need to resort to DES reports before 1996. In DES 1993 we can find previous HSGR information (2 units for mathematics and 2 units for science) and also the first graduating class for which the old HSGR applied (in this case the graduating class of 1989). Also, we find that in DES reports following DES1996, the math and science HSGR of Alabama reported always remains as “4 units for mathematics and 4 units for science.” This means that for the state of Alabama we have recovered all relevant HSGR information: for the graduating classes of 1999 and before, the math and science HSGR is 2 units for mathematics and 2 units for science, and for the graduating classes of 2000 and after, the math and science HSGR is 4 units for mathematics and 4 units for science. To verify this information, we double-checked our procedure using each state’s legislation.

Starting in 2002, DES stops reporting the first graduating class for which a new HSGR will apply. As a result, we collect the implementation year data from the state board of education or department of education of each state when it is unclear from the DES reports.

For example, in the case of Delaware, we have the information about the math and science HSGR and which graduating class the requirements will impact until 2001 from DES (3 units in mathematics and 3 units in science). After 2001, although we can still get the information about math and science HSGR for each year from DES, there is no impact year data from DES. In this case, we turn to the website of the Delaware Department of Education. In the Delaware state code "14 Delaware Code, Section 122(d)[29]", it is stated:

"2.0 ***Current Graduation Requirements***

A public school student shall be granted a State of Delaware Diploma when such student has successfully completed a minimum of twenty-two credits in order to graduate including: 4 credits in English Language Arts, ***3 credits in mathematics, 3 credits in science****, 3 credits in social studies, 1 credit in physical education, 1/2 credit in health, 1 credit in computer literacy, 3 credits in a Career Pathway, and 3 1/2 credits in elective courses.*

3.0 ***Graduation Requirements Beginning with the Class of 2011 (Freshman Class of 2007-2008)***

3.1 ***Beginning with the graduating class of 2011,*** *a public school student shall be granted a State of Delaware Diploma when such student has successfully completed a minimum of twenty two (22) credits in order to graduate including: four (4) credits in English Language Arts****, four (4) credits in Mathematics; three (3) credits in Science****, three (3) credits in Social Studies, one*

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[29] Source: http://regulations.delaware.gov/register/july2006/proposed/10%20DE%20Reg%2030%2007-01-06.htm

(1) credit in physical education, one half (1/2) credit in health education, three (3) credits in a Career Pathway, and three and one half (3.5) credits in elective courses."

From this state code, we can pin down the exact graduation class for which the new requirements apply. In this case, starting with the graduating class of 2011, the minimum required units in mathematics were changed from 3 to 4.

For some states, students are given the option to select different types of diplomas that have different sets of requirements. For example, Indiana offers students who entered school before the fall of 2007 two types of diploma ("general" and "core 40") with two different sets of requirements. We are unable to separate students who choose the general diploma from those who choose the "core 40" diploma in YRBS. In this case, we must use our own judgment to pick a set of requirements that is applied to all students. Our criterion is to choose the least stringent of the possible sets of requirements (since we are trying to define a *minimum* course requirement) unless it is clear that the non-academic requirements associated with the least stringent set are so onerous that it is unlikely to be a choice for the vast majority of students.

To continue with the example from above, the Indiana General Assembly made completion of "core 40" a graduation requirement for all students beginning with those who entered high school in the fall of 2007. Therefore, for the class of 2010 and after, there is only one unique set of requirements. However, for the class of 2009 and before, there are two sets of requirements. Here we choose the requirements of the "general" diploma since this is consistent with the standard diploma requirements of other states. In addition, it is the *minimum* graduation requirement one could face.

Another example is Texas. There are 3 different programs: "recommended", "advanced", and “minimum”. We choose the “minimum” program requirements for the graduating class of 2007 and before as the their HSGR because it is the *minimum* requirement. However, for the class of 2008 and after, we choose the “recommended” program requirements because a newly implemented law made it very difficult for most students to enroll in the minimum program. The law requires parents’ consent, age restrictions, and failing to enter grade 10 once as qualifications for a student to choose the “minimum” program.[30] In this sense, the “recommended” program appears to be the standard one, which is also consistent with the DES report.

For the state of South Dakota, the DES reports from different years are inconsistent with each other, and we could not find additional information from the South Dakota Department of Education to help correct the information despite our best efforts. As a result, we have to use DES data without knowledge of the implementation year even after 2001 for South Dakota’s HSGR.

Our complete compiled dataset of HSGR is given in Appendix 2.

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[30] Sources: 1. Texas Education Code, §§7.102, 28.002, 28.023, 28.025, 28.054, and 38.003. “Chapter 74. Curriculum Requirements Subchapter D. Graduation Requirements, Beginning with School Year 2001-2002” (http://ritter.tea.state.tx.us/rules/tac/chapter074/ch074d.html);

2. Texas Education Code, §§7.102(c) (4), 28.002, and 28.025. “Chapter 74. Curriculum Requirements Subchapter D. Graduation Requirements, Beginning with School Year 2004-2005” (http://ritter.tea.state.tx.us/rules/tac/chapter074/ch074e.html);

3. Texas Education Code, §§7.102(c) (4); 28.002; 28.00222; and 28.025. “Chapter 74. Curriculum Requirements Subchapter D. Graduation Requirements, Beginning with School Year 2007-2008” (http://ritter.tea.state.tx.us/rules/tac/chapter074/ch074f.html)

Appendix 2: The Total Number of Minimum Required Carnegie Units in Math and Science Courses by State for Graduation Class 1993-2014

STATES	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	Variation
MN	2	2	2	2	2	2	2	---	---	---	---	---	---	---	---	6	6	6	6	6	6	6	Yes
IL	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	3	4	4	5	5	5	5	Yes
OH	3	3	3	3	3	3	3	3	3	3	3	6	6	6	6	6	6	6	6	6	6	6	Yes
MT	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	No
AK	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	No
CA	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	No
ME	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	No
WI	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	No
NV	4	4	4	4	4	4	4	4	4	4	5	5	5	5	5	5	5	5	5	5	5	5	Yes
AL	4	4	4	4	4	4	4	8	8	8	8	8	8	8	8	8	8	8	8	8	8	8	Yes
AZ	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	5	7	7	Yes
DE	4	4	4	4	4	4	4	6	6	6	6	6	6	6	6	6	6	6	7	7	7	7	Yes
DC	4	4	4	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	8	8	8	8	Yes
GA	4	4	4	4	6	6	6	6	7	7	7	7	7	7	7	7	7	7	7	8	8	8	Yes
HI	4	4	4	4	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	Yes
ID	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	6	7	7	Yes
IN	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	6	6	6	6	6	Yes
KS	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	6	6	6	6	6	6	Yes
MS	4	4	4	4	4	4	4	4	4	6	6	6	6	6	6	6	7	7	7	8	8	8	Yes
MO	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	6	6	6	6	6	Yes
NH	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	5	5	5	5	5	5	5	Yes
NY	4	4	4	4	4	4	4	4	4	4	4	4	6	6	6	6	6	6	6	6	6	6	Yes
NC	4	4	4	4	4	6	6	6	6	6	6	6	6	7	7	7	7	7	7	7	7	7	Yes
ND	4	4	4	4	4	4	4	4	4	---	---	---	---	---	---	---	---	---	---	---	6	6	Yes
OK	4	4	4	4	4	4	4	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	Yes
OR	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	5	5	6	6	6	Yes

RI	4	4	4	4	4	4	4	4	4	4	4	4	4	5	5	7	7	7	7	7	7	7	Yes
SD	4	4	4	4	4	4	4	4	4	4	4	4	4	5	5	6	6	6	6	6	6	6	Yes
TN	4	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	7	7	Yes
UT	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	6	6	6	6	Yes
VA	4	4	4	4	4	4	4	4	4	6	6	6	6	6	6	6	6	6	6	6	6	6	Yes
WA	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	4	5	5	Yes
WV	4	4	4	4	4	4	4	4	4	4	6	6	6	6	6	6	6	7	7	7	7	7	Yes
CT	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	No
MD	5	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	No
NJ	5	5	5	5	5	5	5	5	5	5	5	5	6	6	6	6	6	6	6	6	6	6	Yes
NM	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	6	6	6	6	7	7	Yes
AR	5	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	7	7	7	7	7	7	Yes
KY	5	5	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	Yes
SC	5	5	5	5	5	5	5	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	Yes
TX	5	5	5	5	5	5	5	5	5	5	5	5	5	5	5	6	6	6	8	8	8	8	Yes
VT	5	5	5	5	5	5	5	5	5	6	6	6	6	6	6	6	6	6	6	6	6	6	Yes
FL	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	7	7	7	7	Yes
LA	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	7	7	7	Yes
PA	6	6	6	6	6	6	6	6	---	---	---	---	---	---	---	---	---	---	---	---	---	---	Yes
CO	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	No
MA	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	No
NE	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	No
IA	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	6	6	6	6	Yes
MI	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	---	7	7	7	7	Yes
WY	---	---	---	---	4	4	4	4	4	4	6	6	6	6	6	6	6	6	6	6	6	6	Yes

Notes: 1. Source: Education Commission of the States, Clearinghouse Notes. 2.To keep consistency, once a state has double standards for standard graduation requirement and college preparation requirement, we choose standard graduation requirement. 3. ---: Requirements are made by local board. 4. For FL, there are basically two programs, a traditional 4 years program and a college preparation 3 years program. All requirements for math and science are the same but the impact year of the newest change for 3-year program is 2010, but for the 4-year program it is 2011. Here we choose 2011 as the impact year in alliance with the spirit of minimum requirements.

Appendix 3: Data Sources

1. State median income. Source: U.S. Census Bureau, Historical Income Tables: Households, "Table H-8. Median Household Income by State." (http://www.census.gov/data/tables/time-series/demo/income-poverty/historical-income-households.html). Most recent date of access: Aug 20, 2016.
2. Expenditures per pupil for public elementary and secondary education. Data for the years 1993-2001 comes from NCES, "A Historical Overview of Revenues and Expenditures for Public Elementary and Secondary Education, by State: Fiscal Years 1990–2002, Adjusted current expenditures per pupil for public elementary and secondary education." (https://nces.ed.gov/pubs2007/npefs13years/). Data for the years 2003-2013 comes from Census Bureau, Public School System Finances, "Per Pupil Amounts for Current Spending of Public Elementary-Secondary School Systems." (http://www.census.gov/govs/school/). Most recent date of access: Aug 20, 2016.
3. Unemployment rate. Source: Annual Unemployment Rates by State. U.S. Department of Labor, Bureau of Labor Statistics. Local Area Unemployment Statistics (LAUS). Data used in this study is second-hand data that is collected and compiled by Iowa Community Indicators Program, Iowa State University. (http://www.icip.iastate.edu/tables/employment/unemployment-states). Most recent date of access: Aug 20, 2016.
4. Sstate high school exit exam requirement status. State High School Exit Exams: A Policy in Transition, Center on Education Policy, 2012; State Profiles on Exit Exam Policies, Center on Education Policy, retrieved November 7, 2012 from http://www.cep-

dc.org/page.cfm?FloatingPageID=79.

5. Pupil-teacher ratio. Source: U.S. Department of Education, National Center for Education Statistics, Common Core of Data (CCD).
6. Cigarette tax. Source: Orzechowski, William, and Robert Walker. “The tax burden on tobacco.” Historical compilation, 49 (2014).
7. Beer tax. Source: World Tax Database, University of Michigan's Ross School of Business for the years 1993-2002 and Tax Foundation, “State Beer Excise Tax Rates” for the years 2003-2013. We then use Alcohol Policy Information System (https://alcoholpolicy.niaaa.nih.gov/taxes_beer.html) to verify the data. All tax rates are the rates on the first day of each year. Numbers are rounded up to the cent to achieve consistency of the two datasets. Based on the drinking pattern of youth, using beer taxes to approximate tax burden for youth alcohol consumption is more accurate than the liquor tax or wine tax.
8. Medical marijuana legalization year. Medical Marijuana legalization data comes from ProCon.org. “23 Legal Medical Marijuana States and DC.” (http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881). Most recent date of access: Aug 20, 2016. Besides medical marijuana legalization, recreational marijuana legalization will likely to affect youths’ consumption of marijuana. However, since our YRBS data ends in 2013 and all recreational marijuana legalization laws were adopted after that year, we do not include them in the regressions.
9. State and national high school student enrollment. Source: Digest of Education Statistics 1993-2015. U.S. Department of Education, National Center for Education Statistics,

Common Core of Data (CCD) 1993-2015.

10. CPI. Source: U.S. Department of Labor, Bureau of Labor Statistics. Consumer Price Index - All Urban Consumers. (http://www.bls.gov/cpi/#tables). Most recent date of access: Aug 20, 2016.

Appendix 4: Comparison of the Sample Dropped and the Sample Used

	Sample Used (N=116,063)		Sample Dropped			T-statistics for mean difference
Variables	Mean	Std. Dev.	N	Mean	Std. Dev.	
Health Behaviors						
did binge drink in last 30 days (1 if yes, 0 if no)	0.266	0.442	27,782	0.275	0.446	-3.026
did drink in last 30 days (1 if yes, 0 if no)	0.447	0.497	21,495	0.505	0.500	-15.636
did smoke in last 30 days (1 if yes, 0 if no)	0.243	0.429	24,426	0.269	0.443	-8.383
did use marijuana in last 30 days (1 if yes, 0 if no)	0.211	0.408	28,248	0.272	0.445	-20.991
number of days binge drinking in past 30 days	1.108	2.976	27,782	1.141	3.062	-1.622
number of days drinking in past 30 days	2.432	4.903	21,495	3.021	5.681	-14.249
number of days smoking in past 30 days	3.276	8.129	24,426	3.712	8.636	-7.244
times marijuana used in past 30 days	2.776	8.352	28,248	3.912	9.911	-17.788
High School Graduation Requirements (HSGR)						
minimum total required units on math and sciences	5.152	1.271	16,790	5.308	1.323	-14.351
Individual Controls						
male	0.483	0.500	30,815	0.530	0.499	-14.691
age	16.18	1.224	30,901	16.16	1.247	2.515
9th grade	0.239	0.426	30,754	0.262	0.440	-8.204
10th grade	0.245	0.430	30,754	0.245	0.430	0.000
11th grade	0.256	0.437	30,754	0.250	0.433	2.156
12th grade	0.259	0.438	30,754	0.241	0.428	6.525
white	0.423	0.494	29,499	0.367	0.482	17.728
black	0.223	0.416	29,499	0.276	0.447	-18.436
other races	0.354	0.478	29,499	0.357	0.479	-0.961
Hispanic	0.269	0.444	29,729	0.273	0.445	-1.383
State Controls						
median income (USD)	22,638	2,902	31,311	23,680	2,825	-57.584
public school per pupil spending (USD)	4,262	1,046	31,311	4,616	986.3	-55.629
unemployment rate (%)	6.338	2.102	31,311	5.936	2.159	29.402
medical marijuana legalization (1 if legal, 0 if illegal)	0.176	0.381	31,311	0.146	0.353	13.118
cigarette tax (cents per pack)	33.83	27.89	31,311	41.90	27.96	-45.347
beer tax (cents per gallon)	14.88	10.15	31,311	11.63	8.620	56.915
state high school exit exam	0.322	0.467	31,311	0.265	0.441	20.040
pupil-teacher ratio	16.65	2.791	31,311	16.25	2.589	23.854

Notes: Observations are unweighted in order to obtain comparability. Monetary figures are deflated (CPI1982-1984=100). T-tests are performed using $T - statistics = \frac{mean_1 - mean_2}{\left(\frac{std_1^2}{N_1} + \frac{std_2^2}{N_2}\right)^{\frac{1}{2}}}$, while the subscripts "1"s refer to the sample we used, and "2"s refer to the sample dropped.

Appendix 5: The Impact of Total Math and Science HSGR on Cocaine Use

Dependent Variables		(1)	(2)	(3)
did use cocaine in last 30 days (1 if yes, 0 if no)				
Total Sample		0.000	-0.002	-0.004
		(0.003)	(0.003)	(0.003)
by Gender	Male Only	-0.000	-0.003	-0.004
		(0.004)	(0.003)	(0.003)
	Female Only	0.002	0.003	0.000
		(0.003)	(0.002)	(0.003)
by Race	White Only	0.003	0.004**	0.003
		(0.002)	(0.002)	(0.002)
	Non-White Only	-0.000	-0.004*	-0.006**
		(0.004)	(0.002)	(0.003)
times cocaine used in last 30 days				
Total Sample		-0.028	-0.118	-0.127
		(0.066)	(0.079)	(0.078)
by Gender	Male Only	0.013	-0.073	-0.095
		(0.070)	(0.068)	(0.071)
	Female Only	-0.011	0.018	-0.014
		(0.037)	(0.028)	(0.036)
by Race	White Only	0.041	0.056*	0.037
		(0.030)	(0.028)	(0.032)
	Non-White Only	-0.024	-0.138**	-0.125**
		(0.057)	(0.051)	(0.060)
State Dummies		YES	YES	YES
Year Dummies		YES	YES	YES
Individual Controls		YES	YES	YES
State Level Controls		NO	YES	YES
State Specific Linear Time Trends		NO	NO	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. Estimates are obtained using weighted OLS with standard errors (in parentheses) clustered by state. Weighting method is provided in the empirical method section. Individual controls include age, grade, gender, and race dummies. State level controls include median income, unemployment rate, per pupil education spending, pupil-teacher ratio, beer tax, cigarette tax, state high school exit exam dummy, and medical marijuana legalization dummy. The total number of observations is 126,741, including 62,206 males, 64,535 females. 51,973 are whites, and 74,768 are non-whites.