No Pass No Drive: Education and Allocation of Time*

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Abstract

More than one-third of public high school students in the U.S., mostly boys and blacks, fail to graduate from their class each year. This has put the question of how to spend educational resources in a cost-effective way prominent on the research agenda. We study the effect of a low cost negative incentive policy, the No Pass No Drive (NPND) law, on educational outcomes. Since the late 1980s, several states have introduced these laws that set minimum academic requirements for teenagers to obtain driving licenses. Using data from the American Community Survey (ACS) we find that NPND laws have a positive and significant effect on probability of high school completion and educational attainment among males and blacks, but not females. Data from the Monitoring the Future (MTF) suggests that students

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who remained in school increased time allocated to school-work at the expense of leisure and work hours.

1 Introduction

Educators and policy makers are increasingly paying more attention towards one of America's most disturbing educational trend: more than one-third of all public high school students fail to graduate with their class.[1] Dropout rates are particularly high among males and blacks. This phenomenon has been termed the "silent epidemic" and has forced states to take several initiatives to keep students in school (Bridgeland, DiIulio & Morison, 2006). Among the different interventions that have been introduced, much attention has been paid to the use of performance-based cash or in-kind rewards to motivate students to stay in school and improve academic achievement. Large scale financial incentive programs have been evaluated in the U.S. and worldwide.[2] These studies advocate for financial incentives or *carrots* as a more direct and cost-effective way to improve student outcomes compared to traditional input-oriented initiatives (e.g., more teachers, higher teacher salaries, smaller class sizes or better school infrastructure).

In a summary of the literature relevant to the U.S., Gneezy, Meier and Rey-Biel (2011) point out that the effects of large scale financial incentive schemes are relatively small compared to the costs incurred. Moreover, most education policies have been unsuccessful in improving educational outcomes among the most vulnerable group, namely, black males. This group is particularly important as a lack of a high school diploma is found to have more severe consequences for black than for white students. For instance, African Ameri-

[1] Swanson, Christopher B. (2004). "Who Graduates? Who Doesn't? A Statistical Portrait of Public High School Graduation", Class of 2001. Washington, DC: The Urban Institute

[2] Some recent examples include Angrist and Lavy (2009), Angrist et al. (2009), Berry (2009), Bettinger (2010), Fryer (2011), Kremer, et al. (2009) and Leuven, Oosterbeek, and van der Klaauw (2010).

can dropouts aged 20–24 years are more than twice as likely to be unemployed compared to white dropouts (U.S. Department of Education, 2001).

In this paper, we study the effect of a large scale and low cost negative incentive policy, the No Pass No Drive (NPND) law, on educational outcomes.[3] We argue that negative incentives, when not too extreme and when targeted towards an activity that students have a preference for, might be an effective means to improve educational outcomes.

Since the late 1980s, many U.S. states have set restrictions for teenagers to have access to a drivers' license. Students must continually earn their driving privileges by staying in school and, in some states, passing their courses. The regulation is intended to motivate academically marginal students, who enjoy the freedom associated with driving, to study harder or, to stay, in school. These laws, commonly known as No Pass No Drive (NPND) laws, vary across states in their scope. While most states require the applicant to be enrolled or attending school, and/or condition license on courses passed, some states deny or revoke driving licenses to minors who are involved in unacceptable behavior such as possession of illegal substances or violent acts. The implementation of NPND laws imposes a minimal cost to the state. School attendance officers monitor truant students and send an electronic notification to the traffic authority, which then denies or revokes the students' driving licenses.

As an example, Kentucky implemented the NPND legislation in August 2007. According to its statute, *"When a sixteen or seventeen year old student drops out of school or is declared to be academically deficient, the schools will report electronically to the Division of Driver Licensing. The Division of Driver Licensing will suspend the student's privilege to drive and notify the driver of the suspension"* (KRS 159.051).

The aim of this paper is twofold. First, we study whether a negative incentive policy can affect the long-run educational outcomes of students by keeping them in school. On the one hand, imposing minimum academic requirements

[3] To our knowledge, no other study has rigorously evaluated the effect of this policy on education outcomes. Krimmel (2000) studies the effect of Kentucky's NPND laws on high school dropout rates. However, he is unable to infer causality as the Kentucky Department of Education also implemented a dropout prevention program around the same time.

can increase education by motivating students who want to gain driving privileges to do better in school. On the other hand, if a student drives to school or to work, taking away his driving privileges might in fact compel him to drop out from school. We use data from the U.S. American Community Survey (ACS) to compare the academic outcomes of treated cohorts who were young enough to have been affected by the NPND laws to older cohorts in the same state, relative to other control states in the sample. Our results indicate that NPND laws have a significantly large effect on educational outcomes among males and blacks, but not females. It led to a 0.14 years increase in average educational attainment among black males and a 5.1 percent increase in the probability of graduating from high school.

Second, we study whether, as a result of NPND laws, high school students change their allocation of time between studying, work and leisure. We use a differences-in-differences approach with repeated cross-sectional data of high school students from Monitoring the Future (MTF) survey. Our results suggest that NPND laws were effective in reducing truancy and increased time allocated to homework, mainly among black males and females, at the expense of leisure and employment activities. Moreover in states with NPND laws, students who are enrolled in school are more likely to hold a driver's license. Both ACS and MTF results are robust to several checks to internal validity threats.[4]

There are several policy implications of our results. First, NPND laws are effective among males, who seem to be unresponsive to positive incentive-policies. Second, in addition to having direct implications on the labor market through higher wages, the increase in education also generates positive externalities on the rest of the society. For example, Lochner and Moretti (2004) estimate that 23% of the difference in incarceration rates between blacks and whites could be eliminated by raising the average education levels of blacks

[4] A caveat for the MTF results is that we only observe allocation of time among individuals who are enrolled in school. As a result, the effects on males needs to be interpreted with caution since NPND constrains "marginal" male students to stay in school. Therefore, the coefficients are underestimating the possible positive effect on blacks and should be a lower bound on the actual estimates.

to the same level as that of whites. Comparably, if education increases one's patience or risk aversion, we might also expect more educated individuals to be safer drivers. Third this policy might be a relatively cheap and effective means to reduce the high school graduation gap. Fourth, this policy might also be effective in narrowing the college gender gap since our results suggest that NPND laws led to an increase in average educational attainment among males.

The rest of this paper is structured as follows. The next section discusses the background and literature pertinent to our study. Section 3 describes the data and presents the empirical strategy. In Section 4 we show the main results and conduct robustness checks for threats to internal validity. Finally, we conclude the discussion in Section 5.

2 Background

2.1 No Pass No Drive Laws

In 1988, West Virginia became the first state in North America to revoke or deny driving privileges to teenagers who did not show satisfactory progress in school. The novel law got considerable media attention after a significant decrease in dropout rates a year after implementation (Ayres, 1989). Following this, several other neighboring states implemented policies that link driver licenses to school attendance, academic performance, and/or behavior. We compiled data on NPND laws from state legal statutes for the period 1988 to 2008. As of 2008, 26 states have passed NPND laws and most states implemented it in the 1990's. The shaded states in Figure 1 had the law in place in the most recent year in our sample.

Advertisement of NPND is widespread. For example, schools with websites tend to have the information available online for both parents and students. Some schools send informatory notes to parents when the law is implemented. In other states, the Departments of Motor Vehicles (DMV) gives talks to the school principals briefing them on ways to report non-compliant students in a

timely manner.

Implementation of the law requires an integrated effort between the State Department of Education, the Department of Public Safety and the Division of Driver's Licensing. When an under-aged driver wants to obtain a driving permit, he needs to present a School Compliance Verification Form indicating that he is in compliance with the requirements of the law. The student may obtain the certificate from his schools' district of residence. In some states, such as Kentucky and Florida, schools electronically report changes to their students' statuses to the licensing authorities. The online service is provided free of charge and imposes minimal cost to either the state governments or the taxpayers.[5] In most other states, whenever a student withdraws from school, is found to be academically deficient, or has excessive absences from school, the law requires the attendance officer to notify the Department of Public Safety (or Department of Motor Vehicles and Transportation). Following the receipt of this notice, the relevant Department sends a notice to the licensee that he is at risk of losing his driving license unless documentation of compliance with the law is received.

Data collected by some of the state departments suggests that the law is strictly enforced and can affect a significant proportion of teenage drivers. While we cannot exactly identify the proportion of students who are affected by the law because they might try to meet the standards before their license gets revoked, data from the state of Florida gives us some insights. In Florida, in 2010, out of about 340,000 drivers under 18, the state suspended 5,389 student licenses for truancy, and sent warnings to another 24,090 students with a learner's permit who were at risk for a delay in getting their license.[6] Only 4% of those who got their licenses suspended did not meet the requirements to recover their driving privileges in the next period. The proportion of affected teenagers however varies across states. For example, the same year, Georgia suspended 16,000 licenses out of 90,684 drivers under 18, which accounted for

[5] Source: Kentucky Department of Education and Department of Transportation

[6] Source: Data tracked by the Florida Department of Motor Vehicles in cooperation with the Florida Department of Education.

approximately 17% of potential teenage drivers affected by NPND laws, while in Tennessee the proportion was around 3% (Southern Regional Education Board, 2011).

Historically, there is also some anecdotal evidence that these laws have been strictly enforced and have been effective in keeping students in school. For example, a 1990 newspaper article stated:

"West Virginia adopted the nation's first such law in 1988. So far, more than 1,000 licenses have been revoked. Of that number, 583 were reinstated-163 because youngsters returned to school, 172 because youths turned 18 and 77 because of "circumstances beyond the control" of the students.

In Florida, a report on the first four months of the law's use shows that 1,000 of the 4,200 dropouts who returned to school between October 1989 and February 1990 cited the law as the reason" (**Kentucky New Era– May 22, 1990**).

The intent of the law is unanimous across states: students who fail to meet mandatory attendance requirements cannot apply for a driver's license. However, they can earn the right again by returning to school, qualifying for an exemption related to personal or professional circumstances or attaining the eligible age, i.e. 18 in most states (see Table 1). Some states also require that students meet certain academic expectations in addition to attendance requirements. As shown in Table 1, among these 26 states, seventeen condition a student's driving privilege exclusively on compliance with attendance requirements. For the remaining states, other factors are also taken into account such as satisfactory academic progress and suspension or expulsion from school. Table 1 also shows that the minimum age at which the individual is bound by the law is 15 for a majority of the states. In most cases, the law is applicable until the individual turns 18.

2.2 Related Literature

The effect of *carrots* or positive incentives on educational outcomes has been well documented and debated. Among psychologists the popular view for over

40 years has been that cash incentives destroy intrinsic motivation to learn (Deci, Koestner, & Ryan, 1999; Frey and Jegen, 2001). Contrary to this extreme view, recent empirical work in economics has shown heterogeneity in the effect of rewards on individuals. While some students improve their outcomes in response to incentives, others are either not affected or are worse off.

Leuven, Oosterbeek, and Van Der Klaauw (2010) evaluate a randomized experiment on the effects of financial incentives on undergraduate students' achievement in University of Amsterdam. They find that high-ability students have larger pass rates and more credit points when assigned to reward groups. In contrast, the achievement of low-ability students drops when assigned to larger reward groups. Angrist, Lang, and Oreopolous (2009) evaluate the effects of financial rewards linked to Grade Point Average (GPA) performance in a Canadian university. They find that financial incentives improve performance among girls but not among boys. This is consistent with an Israeli study by Angrist and Lavy (2009) who find a positive effect on matriculation rates among girls, but not males, who were provided cash incentives to complete a matriculation certificate. Bettinger (2012) finds more direct evidence that incentives, where students could receive up to $100, did not lower measures of intrinsic motivation among elementary-school students in a low-income school district of Ohio. In large scale randomized trials done in four U.S. cities, Fryer (2011) shows that incentives that are linked to inputs (such as attendance, homework, good behavior, etc.) led to an improvement in student achievement. In comparison, incentives that are conditional on performance are much less effective. This study gave financial incentives worth $6.3 million to 38,000 students across 261 schools. Overall, the results suggest that the use of large scale financial incentives in education is not very cost-effective and does not seem to improve outcomes among males.

Research on non-cognitive abilities provides insight on the ineffectiveness of financial incentives in improving outcomes among males. Becker, Hubbard and Murphy (2010) develop a theoretical model and provide quantitative evidence showing that gender disparities in educational attainment can be explained

largely by gender differences in non-cognitive skills. Women, on an average, have higher non-cognitive skills (such as self-motivation, attention to instruction from teachers, organizational capabilities to do homework and prepare for exams) and lower variance in the distribution of non-cognitive skills than men. Thus, one can predict that women are more likely to respond to incentives to increase schooling due to their higher intrinsic motivation and that policies that improve non-cognitive skills in boys might lead to greater gender parity in educational outcomes.

Our study adds to this growing body of literature that evaluates incentive programs. We argue that if NPND laws decrease truancy rates, increase study time and decrease dropout rates without penalizing work activities, then this policy increases overall human capital at a low cost to the public. Moreover, the policy would be most effective if the benefits accrue to disadvantaged groups who are at a high risk of dropping out or being habitual truants.

This paper also contributes to the literature on the effect of *sticks* on student outcomes. Policies that impose a penalty on under performing students are not so popular among educators and policy makers because they decrease the set of choices available to children. Moreover, for researchers, there are ethical issues involved in conducting randomized controlled trials that would penalize one group of students. The existing literature on negative incentives at the secondary school level has mostly focussed on the effects of high school exit exams on dropout rates. However, the evidence is inconclusive and several of the studies find that exit exams causes some groups of students to drop out of school early.[7]

In a study of Canadian college students, Lindo, Sanders, and Oreopoulus (2009) find that being placed on academic probation – the student must earn a GPA above the campus-set standard in the next term or he will be suspended from the university for one year – more than doubles the probability that Canadian males drop out of college but no such discouragement effect is found for girls. Another typical form of negative incentive is to require students to improve their performance in order to gain a particular privi-

[7]See Lindo, Sanders and Oreopoulos (2010) for a brief review of this literature.

lege. Vidal-Fernández (2011) analyzes state interscholastic associations rules imposed during the 1970s in the U.S. that required student athletes to pass a certain number of subjects in order to be allowed to participate in school sports. Using women as a placebo group, she finds that a one-subject increase in the minimum academic standard is associated with a two-percentage-point increase in the probability of high school graduation among males.[8]

We contribute to the literature in at least two ways. First, we evaluate the effect of a policy that targets driving; an activity that is considered an integral aspect of maturation and socialization process among teenagers. If a student does not want to be in school in the first place, placing him/her on probation is only going to make it easier for him to drop out. On the other hand, if the stakes are related to an activity that students enjoy or consider important, the policy might just work.

Second, we show that NPND laws seem to have larger positive effects among black males than similar policies previously analyzed. In theory, penalizing students for not meeting academic standards can raise or lower graduation rates. For instance, consider the case of placing students on academic probation if they do not meet the minimum GPA requirement. Academically marginal students who want to stay in college may be motivated to work harder to remain in college. On the other hand, some students will simply "give-up" because the utility cost associated with the extra academic effort exceeds the benefits of staying in college. If the second effect dominates the first, graduation rates might actually decline as a state adds another requirement to the minimum academic standards – clearly opposite the regulations' intention. Therefore, the stricter is the minimum academic requirement, the less likely we are to find a positive impact on graduation rates.

However, unlike previous policies that have been evaluated in the literature, it is not so obvious why NPND laws should *increase* dropout rates. If a student has a preference for driving, he can obtain a license only by staying in school.

Though, theoretically, the effect of the NPND law on education outcomes

[8] A limitation of this paper is that if men and women are affected differentially by unobserved factors in states with sports restrictions, the identification strategy is questionable.

is unambiguous, there still might be at least two reasons why the law could make some students drop out from school. First, students, who generally drive to school, may no longer be able to attend school if they lose their driving privileges because of being academically deficient under the NPND laws. Second, some students may drop out if the law imposes financial burdens if they can no longer drive to work.

We do not expect that NPND laws will increase dropout rates through these channels because, in most states, students can appeal for an exemption based on personal reasons (Table 1). For instance, students in Kentucky, Louisiana and West Virginia can apply for economic hardship exemptions if they need to drive to jobs that support their families. Similarly, Mississippi allows students under 18 to be exempt if they are married. A few states also allow students to drive if they are enrolled in job training or need to drive to GED certificate programs.

Finally, it is worth noting that the mechanism through which NPND laws affects educational outcomes makes this policy different from other incentive schemes that have been evaluated in the literature. In those studies outcomes were measured while the incentive was in place but before the punishments or rewards were administered. The effect that we identify may result either from the state carrying a stick (i.e. the threat of losing a drivers license) or from actually hitting teenagers with it (i.e. suspending their license). However, we provide evidence that the behavioral response to a threat of losing a license is large.

3 Data and Empirical Framework

3.1 American Community Survey

To study the effect of NPND laws on educational outcomes, we use the three most recent rounds of the U.S. Census Bureau's American Community Survey

(ACS), 2009 to 2011.[9] We match data from the ACS with state level minimum and maximum age requirements to identify cohorts that were affected by the NPND law in the year in which the law was enacted. For the analysis on high school graduation rates we include individuals who are at least 21 years old so that almost everyone should have completed high school. Similarly, for the analysis on completed years of schooling, we include individuals who are at least 24 years old. The oldest cohort is restricted to be 30 years old when the first law was passed in 1988 (i.e. someone born in 1958). Thus, the data spans cohorts born between 1958 and 1990.

Our baseline specification to study the effect of NPND laws on educational outcomes is given by:

$$E_{isc} = \beta_1 Treatment_{sc} + \beta_2 X_{isc} + \beta_3 R_{sc} + SOB_s + YOB_c + T + \varepsilon_{isc} \quad (1)$$

where E is either years of completed education or high school graduation. $Treatment_{sc}$ is a dummy variable indicating whether the individual belongs to the treated cohort c in state of birth s. $Treatment_{sc}$ is equal to 1 for all individuals who were 13 or younger in the year the law was passed. We chose 13 because it is the youngest age at which teenagers are eligible for a drivers' license in our data (see Table 1). The control group ($Treatment_{sc} = 0$) comprises individuals who were older than 18 when a law was passed in their state. Individuals between 14 and 18 in the year the law was passed are excluded from the sample because we cannot identify to what extent they would have been affected by the law.[10] SOB_s and YOB_c refer to state of birth and year of birth fixed effects and T is a linear and/or quadratic state-specific time trend. X_{isc} includes controls for gender, race and Standard Metropolitan Statistical Area (SMSA). R_{sc} includes a set of state-specific demographic (log

[9] Note that most states passed NPND laws in the 1990's. Thus, we use data from the most recent rounds of the ACS in order to include as many individuals as possible who were affected by the laws and at the same time are at least 21 years old in the ACS.

[10] The treatment status of individuals between the age of 14 to 18 cannot be clearly ascertained as some of these individuals may already have licenses when the law was passed (since most states allow licenses by the time the student turned 14). In the robustness section, we will exploit this heterogenous treatment in a dynamic specification.

population), economic (log per capita income and unemployment rate), and education controls (log of per pupil expenditure, pupil teacher ratio and log of teacher salary)[11] associated with the birth cohort at age 13. All income and expenditure variables are inflation-adjusted. Regressions also control for the survey year.

As we can see in Table 1, West Virginia and Illinois were the first and last states passing the law in 1988 and 2005, respectively. Since the treated individuals were less than 13 years old in the year the law was passed, the oldest treated cohort was born in West Virginia in 1975. As mentioned earlier, we restrict the sample to cohorts born between 1958 and 1990 (including both treatment and control). Thus, the youngest cohort affected by the NPND laws comprises of those born in 1990.

We merge data on NPND laws with the ACS data using state of birth identifier. Using state of birth instead of state of residence avoids any bias that may be introduced due to career-induced migration.[12] Standard errors are clustered at the state level (Bertrand, Duflo & Mullainathan, 2004). The entire sample consists of 3,307,742 observations including states that never passed NPND laws.

Our coefficient of interest, β_1, is identified using cross-state and cross-cohort variation. For instance, in California, where law was adopted in 1991, $Treatment_{sc}$=1 for those born between 1978 and 1990 and $Treatment_{sc}$=0 for those born before 1972. The second difference is to individuals of the same birth cohort in other states in the sample that did not have NPND laws at that time.

The crucial identifying assumption is that educational outcomes do not vary systematically across cohorts in the treatment and control states over time. There could be potential internal validity threats to this conventional identification assumption. First, if educational outcomes were reacting to other laws that were being implemented around the same time, our estimates would

[11] All state level education data has been obtained from the National Center for Education Statistics (NCES).

[12] To address concerns of migration-induced bias, our results are also robust to restricting the sample to those individuals whose state of residence is the same as their state of birth.

be biased. Second, there could be mean reversion if there was a downward trend in educational attainment in treatment states at the time of the enactment of the NPND laws but not in control states. Third, the intervention could be a response to another factor that simultaneously influences both the policy and outcome. Besley and Case (2000) point out the importance of controlling for such policy endogeneity. For instance, the sudden increase in teen accident fatality rates or traffic violations could lead to states passing NPND laws. One could argue that due to the increased accident rates, parents forbid their children from driving to school and that in turn influences their allocation of time and educational outcomes.

To account for these factors, we check for threats to internal validity in several ways. First, we present evidence on the robustness of our key results to introducing a rich set of time-varying state-specific demographic, economic, and education controls. To address the issue of policy endogeneity caused by traffic related outcomes, we run a version of the baseline regressions controlling for two additional state-level traffic control variables: the log of vehicle miles traveled and the log of total motor vehicle fatalities among 15-17 year olds. Third, given the long time span of our data, we include state-specific linear and quadratic time trends in the regressions. Fourth, we directly test if our results are being driven by other laws that were being passed in states around the same time as NPND laws such as minimum school entrance age and compulsory attendance laws. Next, we restrict the analysis to states that had the NPND law in place at some point and exclude states that never passed the law. Finally, we run placebo regressions among older cohorts who were not directly affected by the NPND laws. If the identification strategy is valid, we should find that NPND laws have no effect on educational outcomes of older cohorts.

If some states allow exemptions to students with special circumstances, this may present a problem if those who qualify for exemptions are different and changing over time within a state. For example, students might be encouraged to get a GED if that is one of the possible exemptions. However, the results that we present in Section 4 are robust to considering GED graduates as either dropouts or high school graduates. Second, in disadvantaged

areas students might be more likely to work and get exemptions or alternatively high-income parents may be more successful in getting exemptions for their children. However, these concerns are alleviated by including state-specific macroeconomic time-varying controls, state-specific time trends, and additional individual characteristics.

Table 2 presents the descriptive statistics for key variables used in the baseline specification with standard deviations in parenthesis. The average educational attainment in the sample is 13.4 years with a high school graduation rate of 87 percent.[13] As expected, girls have higher education levels and high school graduation rates compared to males. State expenditures per pupil have increased over time whereas pupil teacher ratios have decreased. Teacher salaries have not changed much since the 1960's.

If teenage students allocate their time between attending school, working and leisure, an increase in time spent on attending school or studying should be accompanied by a decrease either in work hours, leisure or both. To support and complement the ACS findings, we use data from the 1993-2008 rounds of Monitoring the Future (MTF) surveys to analyze how NPND laws affect young adults' allocation of time and driving outcomes. The next subsection describes this data in detail.

3.2 Monitoring the Future (MTF)

The MTF surveys approximately 50,000 12^{th} graders across 135 schools every year since 1975 and 8^{th} and 10^{th} graders since 1991.[14] The survey is meant to identify changes in young adults' views, attitudes, and behaviors overtime. Though the primary purpose of MTF is to gather information on substance abuse by teens, the data also contain useful information on teens' allocation of time. In addition, it includes basic demographic information such as age, sex, race, and parents' education.

[13] We treat GEDs as high school dropouts following Heckman and LaFontaine (2010).

[14] The MTF survey is self-administered and information that can be used to identify individuals is held confidential. We came to an agreement with the Survey Research Center at the University of Michigan who ran our programs on site and provided us with the output tables.

The MTF collects data on the average time per week or per day spent on a range of activities including work, going out with friends, watching TV, sports or exercise, reading books and homework. We focus on questions that indicate the channels through which NPND laws might affect the allocation of time between educational investment, work and leisure. Our outcome variables include hours spent working, watching TV, doing homework and the probability of skipping school. Finally, to further support our results, we also look at the effect of NPND on driving outcomes in the MTF.

Table 3 presents the outcome variables and demographic characteristics by gender and race. Standard deviations are in parenthesis. There are no statistically significant differences by race or gender in the background characteristics. However, we can see some interesting differences by gender and race in the outcome variables. For instance, white females are the least likely to skip school while black males are most likely to be habitual truants. Similarly, white females spend about 8 hours per week doing homework while black females and males spend, on an average, 6 and 5.4 hours respectively. Interestingly, there are no significant differences across the groups in leisure activities such as going out on dates. However, blacks spend more time watching TV on weekdays. The average for accidents ("probability of having an accident in the last 12 months") are large mainly because this includes accidents regardless of whether the respondent was the driver or a passenger and also includes accidents that resulted in minor bumps and scratches.

For the MTF, we estimate the following differences-in-differences specification for respondents who were 15-17 years old at the time of the survey (10^{th} and 12^{th} graders) and were, therefore, in the age group directly affected by NPND laws.

$$Y_{ist} = \alpha_0 + \alpha_1 NPND_{st} + \alpha_2 X_{ist} + \alpha_3 Z_{st} + S + Y + \epsilon_{ist} \tag{2}$$

where i denotes individual, s denotes state, and t denotes time. Y is the outcome of interest (studying, work, leisure and driving). $NPND$ is a dummy variable that takes the value of 1 if state s has the NPND law in place at time t. X is a vector of individual student characteristics that includes age, maximum

parental education, race dummies, a dummy equal to one if the student lives in a Standard Metropolitan Statistical Area (SMSA), and a male dummy in the full sample models. Z_{st} includes potentially relevant time-varying state-level controls. These include macroeconomic variables (log of per-capita income, log of population and unemployment rate), education controls (log of per-pupil expenditures in education, log of teacher's salary and the ratio of pupils per teachers) and traffic-related variables (log of vehicle miles traveled and log of total motor vehicle fatalities among 15-17 year olds).[15] All income and expenditure variables are inflation-adjusted. S and Y are state and year fixed effects, respectively. Standard errors are clustered at the state levels (Bertrand, Duflo & Mullainathan, 2004).

Our coefficient of interest, α_1, captures within-state changes in students' outcomes in states where a NPND is enacted with respect to the associated changes in outcomes of students in states where a law has not yet been enacted. The identifying assumption is that there are no unobserved changes in variables related to both student outcomes and NPND laws that are differentially affecting treatment and control states. We carry out robustness checks to ensure internal validity of our estimates. We introduce education control variables that affect education and might have changed during the time when the laws were being enacted. Similarly, we include state-specific linear time trends to capture time-varying unobserved characteristics at the state level.

The estimated coefficients for the effect of NPND laws on allocation of time has to be interpreted carefully for two reasons. First, time spent on leisure or work may decrease not because individuals choose to devote more time to study, but because they might not be able to drive to work or to a party. However, we circumvent this problem by also studying outcomes that do not require driving, such as, time spent watching TV or doing homework. Regardless of the reason behind changes to time allocation, if high school graduation rates increase as a result of the NPND laws, it should be at the

[15] Data on vehicle miles travelled was obtained from the Highway Statistics Series of the Federal Highway Administration. Motor vehicle fatality data are from the Fatality Analysis Reporting System (FARS).

expense of either leisure, work or both.

Second, the MTF consists of a sample of teenagers who are enrolled in school. If NPND constrains "marginal" students to stay in school, the coefficients are underestimating the possible positive effect on student outcomes and should be a lower bound on the actual estimates.

4 Results

4.1 American Community Survey

Before presenting the main results, we provide some visual evidence in support of the identification assumptions. Consider the following relation between high school graduation, H, and years of exposure to NPND laws:

$$H_{isc} = \sum_{j=7}^{24} \alpha_j D_{ij} + \beta X_{isc} + \gamma R_{sc} + S_s + B_c + \varepsilon_{isc} \tag{3}$$

D_{ij} is a dummy that indicates whether individual i was age j when the law was passed. All other control variables are the same as in our main specification given by equation (1). Since identification in this model relies mainly on post-NPND trends, we do not include state-specific time trends. The above specification is restricted to individuals between 7 and 25 years old, where 25 year olds are the omitted category. We assume that everyone is in school by age 7. The sample is restricted to states that passed the NPND law at some point.

The coefficients, α_j, have a testable restriction for increasing values of j. The younger the individual is when the law is passed (i.e. smaller is j) i.e. the longer he is exposed to the policy, the larger should be the effect on high school graduation rates. Moreover, we should find no effect for individuals who were aged 18 years and above at the time of enactment.[16]

[16] This specification can be compared to a dynamic differences-in-differences setting with dummies for pre-treatment and post-treatment years. Duflo (2001) uses a similar methodology.

Figure 2(a) plots the coefficients and confidence intervals for the $\alpha_j's$ for the entire sample. Each dot represents the coefficient on the dummy variable for age at the time of enactment of the law. The vertical lines are the corresponding 95-percent confidence intervals. Reassuringly, the coefficients are close to zero for individuals who are 18 years or older at the time of passing of NPND laws. The younger the individual is when the law is passed, the larger is the coefficient. Note that NPND laws were passed relatively recently. Thus, the sample size is larger for older cohorts and that explains the noisier confidence intervals for younger cohorts. Nevertheless, the confidence intervals do not cross the zero line until age 14 after which the coefficient becomes statistically insignificant. As pointed out earlier, for individuals between the age of 14 to 18 we cannot identify to what extent they would have been affected by the law.

Figure 2(b) through (e) plots coefficients for $\alpha_j's$ for gender and race regressions. A similar pattern is observed across all sub-groups; the younger the individual is when the law is passed, the larger is the coefficient. The only exception is Blacks where the estimates are mostly negative, insignificant and often switches sign.[17]

The effect that we identify may result either from the state carrying a stick (i.e. the threat of losing a drivers' license) or from actually hitting teenagers with it (i.e. suspending their license). Figure 2 suggests that the behavioral response to a threat of losing a license is large and this is evident from the larger coefficients on cohorts who are affected at a younger age.

Table 4 presents results from the baseline specification given by Equation (1), which is less restrictive and provides us with more precise estimates. To explore whether NPND laws are more effective for males and disadvantaged

[17]When we control for state-specific time trends in the above model, the event study graph for Blacks looks similar to the other subgroups (with coeffcients ranging between 0.02 to 0.05 for ages 7 to 14). Thus the unstable pattern observed in 2(e) could be due to secular trends in high school graduation rates among Blacks that occur monotonically within states over time. However, as mentioned before, controlling for state-specific time trends is inappropritae in this model as we do not have sufficient sample period available before the laws were passed. We explore a more complete model that allows for inclusion of time trends in the next section.

students, we estimate separate models by gender and race. Results for high school graduation rates and educational attainment are presented in panel A and B, respectively. In the specification with state-specific linear time trends (Column 1), males affected by NPND laws are 1.3 percent more likely to graduate from high school and have about 0.064 additional years of education. The mean graduation rates among males is 84%. Thus, as a result of NPND laws, affected male cohorts are completing 0.064 additional years of education and are 1.5 percent more likely to graduate from high school. As expected, the effect on average educational attainment is smaller relative to high school graduation rates. This is because the law should have the largest effect on marginal students who are at the risk of dropping out, and these students are anyway unlikely to attend college.

Since blacks constitute a disproportionately large proportion of the dropout population and NPND targets teens at risk of dropping out, we should expect a larger effect for this subgroup. This is confirmed by looking at Table 4. Black males affected by the NPND law have 0.14 additional years of education and are 3.7% more likely to graduate from high school. This is a large effect that translates into a 1.2 percent increase in average educational attainment among black males (the mean education for this group is 12.49 years) and a 5.1 percent increase in the probability of graduating from high school (mean graduation is 0.72). All estimates are significant at the 5% level.

Because our data spans across several cohorts and graduation rates may not follow a linear trend, Column (2) includes state-specific quadratic time trends. Results are robust and in fact, the coefficients almost double and are more precise. The coefficients for high school graduation and educational attainment among black males increases to 6.7% and 0.26 years, respectively. Column (2) also shows that graduation rates for females and white males increased by 0.6% and 0.7% respectively, but the estimates are relatively imprecisely estimated.

Taken together, the results suggest that NPND laws had the largest effect on males and in particular among blacks. This is an interesting result because several studies on financial incentives find that girls react to positive incentives while males do not. However, the results from Table 4 show that the effect of

a *stick* on educational attainment seems larger among males. One explanation for this result is that females demonstrate superior non-cognitive ability, thus they are less affected by an incentive that is designed to improve achievements they already have (Becker, Hubbard, and Murphy 2010). Another explanation could be that males have a preference for driving while females do not.

4.2 Robustness Checks and Placebo

4.2.1 Minimum School Entryage Laws

A new strand of literature finds that children who are older when they start school tend to perform better and complete more years of schooling (Barua and Lang, 2010).[18] If school entry age laws changed around the same time as NPND laws, our results would not correctly capture the effect of NPND laws.

To address this concern, we estimate the regressions controlling for the minimum age, in months, at which the state allows the child to enroll in kindergarten. For instance, if a state law requires that the child must turn 5 by 1^{st} September, the youngest child in Kindergarten in that state would be 60 months old (assuming school starts on 1^{st} September). Using state of birth as the identifier, we merge census data with school entry age laws that were in place in the year all individuals in our sample turned 5.

Table 5, columns (3) and (4), shows results controlling for the minimum school entry age. We only present estimates among males (panel A) and black males (panel B). Columns (1) and (2) reproduce results from Table 4 for educational outcomes. The inclusion of the entry age variable does not change the coefficients for males and the coefficient for black males is larger. The coefficient on entry age variable is close to zero and statistically insignificant. In models not shown here, we also include as an additional variable the Com-

[18] We have also explored a possible link between school accountability programs and NPND laws. Unlike the NPND laws which were passed in the 1990's, most school accountability programs were first implemented during the 70's and early 80's. As far as we know, only Dallas and Chicago school district implemented school accountability policies during the 1990s (Jacob, 2005). While Illinois has not passed an NPND to date, Texas passed it in 1995. Our results are robust to excluding Texas from the sample.

pulsory Attendance Law (CAL) that was in place in the year the individual turned 14.[19] Reassuringly, controlling for CALs does not change either the magnitude nor the significance of estimates.

4.2.2 Traffic-Related Confounding Factors

Due to the nature of NPND laws, we should worry if the laws were being enacted as a response to teenage traffic trends that were also related to educational outcomes. For example, a sudden increase in teen traffic fatality rates or traffic violations could lead to states passing NPND laws. At the same time, due to the increased accident rates or violations, parents might forbid their children from driving to school which might ultimately influence their time spent studying.

We estimated all models including two state-level traffic control variables: the log of vehicle miles traveled and the log of total motor vehicle fatalities among 15-17 year olds.[20] The data on vehicle miles and traffic fatalities were collected from the Federal Highway Administration (FHWA) and the Fatality Analysis Reporting System (FARS), respectively. To be consistent with the other control variables, we match this information to the year the individual turns 13. Upon inclusion of traffic variables, the coefficients are even larger in magnitude, especially for black males and are more precisely estimated.[21]

4.2.3 NPND States versus Control States

An underlying assumption of our model is that in the absence of NPND laws, educational outcomes would have evolved similarly between states that implemented and those that never implemented NPND laws. The last two columns in Table 5 ignores non-NPND states and only exploits variation across cohorts

[19] We kindly thank Philip Oreopoulos for providing us with the data on CALs.

[20] Tables available upon request.

[21] Cross-state comparisons might also suffer from fundamental differences in location related needs to drive. The effects should be larger in rural areas where public transport is not largely available. Though all our regressions control for SMSA, we also estimated separate regressions by SMSA status. The results are comforting and show that coefficients are larger in magnitude in rural areas and often insignificant in urban regions.

and timing of implementation within the sub-sample of NPND states. The results are consistent and confirm that NPND laws caused high school graduation rates among males and black males to significantly increase within the group of adopting states. For black males, the coefficients are larger in magnitude and do not lose significance even with the relatively smaller sample sizes. Thus, the effect of NPND laws on educational outcomes is not driven purely by unobservable differences across implementing and non-implementing states.

4.2.4 Placebo Tests

As an additional internal validity test, we use a "fake" treatment group to see if educational outcomes are reacting to any other factors that affect different cohorts in a particular way. Individuals who were older than 19 years of age at the time of the enactment should not be affected by these laws. Not only are most of these individuals out of school but also they are above the maximum age at which the NPND law is applicable. We estimate a placebo model in which the "treatment" group includes individuals who are between 19 and 24 years of age and the "control" group comprises of individuals between ages 25 and 30. The results for this control experiment are shown in Table 6. If the regression estimate is significantly different from 0 for the placebo groups, the trends are not parallel, and the identification assumption may be challenged. As we can see from columns (1) and (2) in Table 6, for both males and blacks, the effect of NPND on educational outcomes for the placebo groups is close to zero and statistically insignificant.

4.2.5 Effect at Different Levels of Education

Though NPND laws constrain teenagers to remain in school from the age of 13, the effect of the law should be most pronounced after individuals attain the minimum dropout age, which is 16 years in most states. Thus, we should expect teenagers in grade 10 and above to be the most affected by this policy (assuming that a 16 year old is in grade 10). Moreover, since NPND laws primarily targets "marginal" students, the effect of these laws on post sec-

ondary education is anticipated to be smaller than the effects on high school graduation.

Table 7 shows at what level of education the laws were effective. We present results for the effect of NPND laws on different levels of education among males who were at least 21 years old in the ACS. The regression framework is the same as Equation (1) but now the outcome variable is the probability of completing at least the n^{th} grade, where $n = 8$ to 12, some college and a college degree.[22]

Among males, cohorts affected by NPND laws are more likely to complete $10th$, $11th$ and $12th$ grade. The coefficients are almost equal in magnitude with a slight increase from grade 10 to grade 12. We also find that NPND increases the probability of attending one or more years of college by 1.1%, however, there is no significant effect on college completion rates. Among black males, the effects are concentrated in $11th$ and $12th$ grade with the magnitudes increasing from grade 8 until grade 12 and then declining thereafter. There is a small positive effect in grade 8, but the coefficients are less precisely estimated. Black males affected by NPND laws are also 1.2 percent more likely to complete college which is a third of the effect on graduation rates. Thus, the estimated effect of the NPND laws are largest in grades 10 through 12. Moreover, the effect on college completion is positive but much smaller in magnitude. This provides additional evidence in favor of the assumption underlying the identification strategy.

Overall, the results strongly suggest that NPND laws did indeed increase educational attainment and graduation rates among males and blacks in the U.S. Given this observed shift in time invested in education, how do NPND laws affect work-leisure time allocation for those students who stayed in school? To address this question and further support the ACS estimates, in the next section, we show results using data from Monitoring the Future (MTF) survey.

[22]For these regressions, we continue to treat GEDs as highschool dropouts. We also assume that because of minimum dropout age laws, those with a GED had to stay in school at least until grade 9. Thus, we consider GEDs as having completed $8th$ and $9th$ grade but not $10th$, $11th$ or $12th$ grade.

4.3 Monitoring the Future

In this section, we present results for 15 to 17 year olds from the differences-in-differences specification given in Equation (2). To be consistent with the ACS data, in all the MTF models, we include the same set of control variables as in the census estimates. However, in tables not shown in the paper (available upon request), we have estimated versions of the baseline model including recent state laws related to driving. Our results are robust to including Graduated Driver Licensing (GDL) laws and Seatbelt use laws. All regression estimates shown also include state effects, year effects and state-specific linear time trends.

Table 8, columns (1) and (2) report estimates for two school-related outcomes as the dependent variable, namely, probability of skipping school and weekly hours spent doing homework. In most states the law not only requires teenagers to be enrolled in school but also enforces a minimum attendance requirement. Column (1) shows that school-enrolled teens in states with an NPND law are 7.5 percent less likely to be truants and the effect is significant at 5% (the coefficient is 0.018 while the mean for days skipped is 0.24). Interestingly, when we compare coefficients across the different sub-groups, we find that the effect is larger for girls and insignificant for males. Girls may be less likely to dropout of school than males but that does not necessarily imply that they are also less likely to be truants. This is also clear from Table 3 which shows no gender differences in the average rates of truancy.

Another possible explanation for the insignificant result for males is sample selection. The MTF only records information for non-dropouts. In states with NPND laws, the sample includes individuals who were at the margin of dropping out but decided not to because of the fear of losing their driving privileges. We would expect these "marginal" students to have a higher truancy rate. Note that, due to having only non-dropouts in the MTF, selection is likely to be most pronounced for blacks since results from the ACS suggest that blacks had the largest increase in graduation rates. Therefore, the coefficients are underestimating the possible positive effect on blacks and should be a lower bound on the actual estimates.

In Column (2), we see that black students are spending more time doing homework. In particular, black males and females spend 1.12 and 0.84 more hours, respectively, doing homework each week and the result is significant at 1%. This is a large effect relative to the average and translates to a 20% increase in the average weekly time spent doing homework among black males and 14% increase among black females. There is a small negative effect, though significant only at the 10% level, on homework time for white females.

Columns (3) reports estimates for the effect of NPND on leisure activities, where leisure is proxied by hours spent watching TV on an average weekday.[23] We observe that black students in states with NPND laws are spending less time watching television on an average weekday. They spend 0.2 hours fewer hours each week watching TV and the effect is also highly statistically significant at 1%. The negative effect is larger and more precisely estimated among black females.

Finally in Column (4) we present results for weekly hours spent working. There is a decrease in hours spent on the job each week for most of the subgroups. The results are strongest for males and blacks. In particular, males work about 0.15 hours less each week while blacks reduce hours of work by 0.2 hours in states with NPND laws.[24] Both estimates are highly statistically significant.

To sum up, NPND laws led to a redistribution in allocation of time with respect to work, study and leisure among high school students. In particular, blacks males and females are spending more time doing school work and less time working and watching TV. Among females, there is also an increase in school attendance. The results for black females is reassuring as sample selection should not be a problem for this group, and yet we observe substantial reallocation of time. There are at least three possible explanations for this. First, there may be peer effects. Second, we observe a decrease in probability of skipping school among females and that leads to significant reallocation of

[23] The MTF also reports a broad range of other leisure activities. We did not find any effect of NPND on hours spent playing sports, going to the movies, playing videogames, going out with friends or going to a mall.

[24] We find no effect on the probability of being employed.

time away from work and leisure. Third, female siblings or girlfriends may be compelled to get a license if some black males lose their driving privileges because they dropped out of school. Thus, even if earlier they did not care for driving, now they are compelled to adhere to the minimum attendance requirements of NPND laws.

4.4 Driving Outcomes

To further support our results, we also study the effect of NPND laws on driving outcomes in the MTF. In these regressions, we also include 18 year olds in the sample because driving-related questions are only asked to 12^{th} graders.

Table 9 presents estimates for the effect of NPND laws on the "probability of holding a driving license" and "probability of having an accident in the last 12 months", respectively. The results indicate that all sub-groups except white females have a high likelihood of holding a driving license with the effect being largest among blacks. In states with NPND laws, both black males and females are more likely to hold a drivers' license and the effects are highly statistically significant. In fact, the probability of holding a license is slightly higher among black females (2.1%) compared to black males (1.7%).

It is not surprising that the effect of NPND laws on driving licenses is positive. If the law makes individuals stay in school, it is precisely because they have a preference for driving. Thus, in states with NPND, those who are enrolled in school have a strong preference for driving and are more likely to hold a license.

We also find that in states with NPND laws, black males are 4 percent less likely to have traffic accidents. We also find a negative coefficient on accidents for black females, however, the coefficient is much smaller in magnitude and is relatively imprecisely estimated.

The interpretation of coefficient on accidents is not straightforward. On the one hand, the effect on accidents can be through an increase in education. For instance, Lochner and Moretti (2004) show that an additional year of

schooling is associated with a 0.37 percentage point reduction in incarceration for blacks. Comparably, if education increases one's patience or risk aversion, we should expect more educated individuals to be safer drivers.

On the other hand, NPND laws led to a decrease in the number of risky drivers on the roads. Individuals who were not enrolled in school and/or were habitual truants would have lost their driving privileges. Thus, the negative effect on accidents could simply reflect the change in age composition of drivers due to the smaller number of teen drivers on the roads. We are not aware of any nationally representative data set that has individual level data on accidents, education and state-level identifiers that would allow us to test these different interpretation of our driving results. We leave that for future research.

5 Discussion

Parents and educators use *carrots* to tempt a child to cooperate and behave well or *sticks* or threats to shape certain behavior. The theoretical rationale behind using such approaches is that low-achieving individuals have high discount rates and the use of *carrots* and *sticks* motivates them to change their behavior. While social psychologists have long debated the effect of incentives on intrinsic and extrinsic motivation, economists have recently begun evaluating numerous positive incentive policies. The main advantage of positive incentive policies is that they are fairly easy to implement and they increase the set of choices a child has and therefore it should not decrease their utility. However, they are costly to administer and do not always seem to work for males.

Negative incentive policies are not so popular among policy makers because they decrease the choices available to children and the benefits might be short-run. Moreover, they are only effective if they target something that individuals have a preference for. Also, there are ethical issues with conducting randomized controlled trials that involve negative incentives. Nevertheless, parents and educators continue to use *sticks* to discipline and motivate low performing children. For instance, grounding and time-out are common approaches

used by parents. Policy-makers across the world are also increasingly making use of negative incentives to keep students from dropping out of school. A recent Australian policy requires that teen mothers be enrolled in school to receive welfare payments.[25] In the U.S., high school students who do not pass a certain number of subjects are not allowed to play sports.

In this paper, we show that the No Pass No Drive (NPND) law, a U.S. state level negative incentive policy, has positive and significant effect on educational outcomes among affected cohorts and the effect is mainly driven by males and blacks. Further, we show that NPND laws were effective in reducing truancy and increased time allocated to school-work at the expense of leisure and work.

While a formal cost-benefit analysis is beyond the scope of this paper, it is easy to argue that the benefits of this policy far exceeds the costs. We find that NPND laws increase educational attainment among blacks by 0.14 years and high school graduation rates by 5.1%. Many estimates suggest that the private rate of return to a year of education is 10 percent, or approximately $80,000 in present value over the course of a lifetime. Using these numbers, 0.14 years more education would increases lifetime earnings by $ 11,200. This is the direct private benefit of the policy and does not take into account the social benefits in terms of reduced traffic accidents, less teenagers who are drinking and driving etc. Moreover, decreased truancy and more time spent doing school work implies keeping students off the streets which can also have a more direct effect on crime.

In terms of the costs to the government or taxpayers, the marginal cost of electronic reporting is nearly zero and the fixed cost of setting up the system is minimal. However, for students who are enrolled in school, we find a decrease in work hours in the MTF data. Due to NPND laws, blacks are working 0.2 hours less each week. Assuming that an average high school student works up to 20 hours per week and using the Federal minimum wage of $7.25 as a reference, this implies a $1.45 decrease in average weekly wages for the

[25] http://www.abc.net.au/news/2011-05-05/teen-parents-targeted-in-welfare-crackdown/2704204

remaining years in school. This figure is clearly small relative to the potential increase in lifetime earnings due to an increase in education. At the same time, some students who lose their driving privileges because of poor attendance may no longer be able to drive to work. In this case, their parents may drive them to work (which would cost the same except the time cost to parents) or they might be compelled to take public transportation (which would be cheaper than driving). However, we expect such costs to be negligible as most states allow exemption for students with special circumstances.

Finally, an important negative consequence of the law could be that it may encourage teenagers to drive with or without licenses. The positive license effects for subgroups (Table 9) might be due to the law making licenses and driving more salient for students. This could have high social costs if teenage drivers are riskier and under insured. Using state level data on number of licenses from the Departments of Motor Vehicles (for the period 1988 to 2008), we ran several regressions to study the effect of NPND laws on age and gender specific licenses among 16-18 year olds. In these regressions we controlled only for state and year fixed effects. Coefficients for both males and females generated insignificant and unstable estimates. This gives us some reassurance that the law didn't increase number of teenage drivers with valid licenses.

Unfortunately, there is no data on non-licensed drivers available. We can, however, look at the effect on state-level teen accident rates as a potential negative outcome of NPND laws. Note that the effect could even be positive, as discussed earlier, if education has positive externality effects on accident rates. We have tested this theory using the Fatality Analysis Reporting System (FARS) that maintains data on fatal injuries suffered in motor vehicle traffic crashes in the US. Negative binomial regression models of the effect of NPND law on state level accident fatalities among teenagers yielded negative but statistically insignificant results.[26]

To conclude, it is worth taking advantage of natural experiments to evaluate the intended and unintended consequences of low-cost negative incentive

[26] However, this data is at the state level and only includes accidents that led to a fatal outcome.

policies. Negative incentives, when not too extreme and when targeted towards an activity that students have a preference for, might be an effective means to improve educational outcomes, especially among the disadvantaged groups.

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Figure 1: States With NPND Laws (2008)

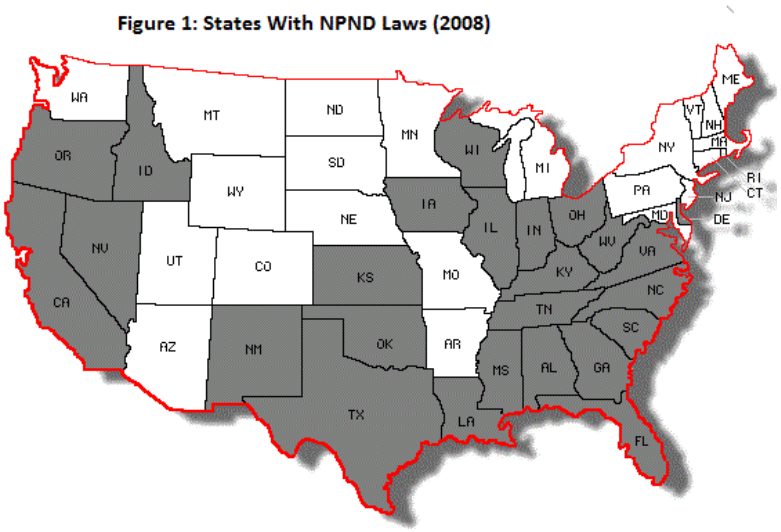

Table 1: Summary of No Pass No Drive Laws

State	Year of Enactment	Attendance Requirement	Satisfactory Progress in School	Student Behavior	Minimum Age	Maximum Age	Exemption
Alabama	1993	Yes			13	19	Employment, children, caretaker
Arkansas	1991	Yes			14	18	Employment
California	1991	Yes			13	18	No
Delaware	2000	Yes			-	-	No
Florida	1997	Yes			15	18	Hardship
Georgia	1998	Yes			15	18	Caretaker
Idaho	1996	Yes			15	18	No
Illinois	2005	Yes		Yes	-	18	No
Indiana	1991	Yes		Yes	15	18	No
Iowa	1994	Yes			-	18	No
Kansas	1999			Yes	13	-	No
Kentucky	1990		Yes		16	18	Economic hardship
Louisiana	2004			Yes	15	18	Exceptional circumstances
Mississippi	1994		Yes		15	18	No
Nevada	2003	Yes			14	-	No
New Mexico	2004	Yes			-	-	No
North Carolina	1997		Yes		15	18	Exceptional circumstances
Ohio	1992	Yes			-	18	No
Oklahoma	1996	Yes			14	18	Exceptional circumstances
Oregon	1995			Yes	15	21	Exceptional circumstances
South carolina	1998	Yes			15	17	No
Tennessee	1990	Yes	Yes	Yes	15	18	Exceptional circumstances
Texas	1995	Yes			15	18	No
Virginia	1993	Yes			16	18	Emancipation or marriage
West Virginia	1988		Yes		15	18	Exceptional circumstances
Wisconsin	1988	Yes			16	18	No

Table 2: ACS Descriptive Statistics by Cohort

Individual Characteristics					
Cohort		1960	1970	1980	All
	Males	0.49	0.49	0.50	0.50
		(0.50)	(0.50)	(0.50)	(0.50)
	Blacks	0.11	0.12	0.12	0.12
		(0.31)	(0.32)	(0.33)	(0.33)
Years of Education		13.45	13.67	13.42	13.42
		(2.14)	(2.09)	(1.96)	(2.06)
	Males	13.30	13.45	13.18	13.26
		(2.23)	(2.16)	(2.04)	(2.13)
	Females	13.60	13.89	13.67	13.64
		(2.05)	(1.99)	(1.89)	(1.98)
	Blacks	12.87	13.05	12.78	12.84
		(2.32)	(2.24)	(2.14)	(2.22)
	Whites	13.55	13.79	13.55	13.57
		(2.09)	(2.02)	(1.90)	(2.01)
	White Males	13.42	13.59	13.33	13.39
		(2.18)	(2.09)	(1.93)	(2.07)
	White Females	13.68	13.99	13.78	13.74
		(1.99)	(1.93)	(1.84)	(1.92)
	Black Males	12.56	12.64	12.39	12.49
		(2.36)	(2.28)	(2.20)	(2.27)
	Black Females	13.14	13.41	13.16	13.18
		(2.26)	(2.15)	(2.00)	(2.13)
High school graduation		0.87	0.87	0.86	0.87
		(0.34)	(0.33)	(0.35)	(0.34)
	Males	0.85	0.84	0.83	0.84
		(0.36)	(0.36)	(0.38)	(0.37)
	Females	0.89	0.90	0.89	0.89
		(0.31)	(0.30)	(0.32)	(0.31)
	Blacks	0.80	0.79	0.77	0.78
		(0.40)	(0.41)	(0.42)	(0.41)
	Whites	0.88	0.89	0.88	0.88
		(0.32)	(0.31)	(0.33)	(0.32)
	White Males	0.86	0.87	0.85	0.86
		(0.34)	(0.34)	(0.35)	(0.35)
	White Females	0.90	0.91	0.90	0.91
		(0.30)	(0.28)	(0.29)	(0.29)
	Black Males	0.75	0.72	0.69	0.72
		(0.43)	(0.45)	(0.46)	(0.45)
	Black Females	0.84	0.86	0.84	0.84
		(0.36)	(0.35)	(0.36)	(0.36)
Observations		1,071,487	866,263	901,689	3,307,742*

* Includes cohorts born between 1959-1990.

Table 2 Continued: ACS Descriptive Statistics by Cohort

State-specific Economic Variables				
Cohort	1960	1970	1980	All
Log (Per capita income)	8.94	9.70	10.17	9.53
	(0.30)	(0.21)	(0.18)	(0.66)
Log (Population)	15.69	15.78	15.94	15.80
	(0.88)	(0.89)	(0.92)	(0.91)
Unemployment rate	6.98	6.98	5.24	6.31
	(2.18)	(2.05)	(1.31)	(1.99)
State-specific Education Variables				
Cohort	1960	1970	1980	All
Log (Expenditure/pupil)	7.98	8.21	8.78	8.33
	(0.23)	(0.25)	(0.39)	(0.51)
Pupil teacher ratio	19.84	17.80	16.88	18.41
	(1.99)	(2.47)	(2.63)	(2.87)
Log (Teacher salary)	10.03	10.09	10.12	10.08
	(0.15)	(0.16)	(0.16)	(0.16)

Salaries, income, and expenditures are inflation-adjusted. Standard deviations in parentheses.

Table 3: Monitoring the Future (MTF) Descriptive Statistics

	All	Males	Females	Blacks	Whites	Black Males	Black Females	White Males	White Females
Education									
Skips school	0.24	0.24	0.23	0.27	0.22	0.28	0.26	0.23	0.21
	(0.43)	(0.43)	(0.42)	(0.44)	(0.41)	(0.45)	(0.44)	(0.42)	(0.41)
Weekly hours doing homework	6.89	6.26	7.45	5.75	7.24	5.43	6.01	6.52	7.92
	(6.31)	(6.05)	(6.64)	(5.99)	(6.47)	(5.84)	(6.09)	(6.09)	(6.74)
Employment									
Weekly working hours	2.56	2.67	2.46	2.75	2.54	2.83	2.68	2.65	2.44
	(2.08)	(2.15)	(2.0)	(2.26)	(2.02)	(2.3)	(2.23)	(2.11)	(1.93)
Leisure									
Going out on dates	2.58	2.60	2.57	2.57	2.59	2.66	2.49	2.57	2.61
	(1.59)	(1.55)	(1.62)	(1.59)	(1.59)	(1.57)	(1.6)	(1.55)	(1.63)
Weekly hours watching TV	2.51	2.59	2.43	3.35	2.27	3.37	3.34	2.39	2.16
	(1.53)	(1.52)	(1.53)	(1.57)	(1.4)	(1.56)	(1.58)	(1.45)	(1.42)
Traffic variables									
Has a driving license	0.14	0.14	0.13	0.14	0.14	0.15	0.12	0.15	0.14
	(0.34)	(0.34)	(0.3)	(0.34)	(0.35)	(0.36)	(0.32)	(0.35)	(0.35)
Had a traffic accident	0.29	0.26	0.27	0.26	0.3	0.28	0.24	0.3	0.29
	(0.46)	(0.43)	(0.44)	(0.44)	(0.46)	(0.44)	(0.43)	(0.47)	(0.45)
Age	16.02	16.08	15.97	16.46	15.94	16.52	16.40	16.00	15.88
	(1.69)	(1.69)	(1.69)	(1.77)	(1.65)	(1.77)	(1.77)	(1.65)	(1.65)
Statistical Metropolitan Area	0.76	0.76	0.76	0.80	0.73	0.80	0.80	0.73	0.73
	(0.43)	(0.43)	(0.43)	(0.4)	(0.44)	(0.4)	(0.4)	(0.44)	(0.45)
Parent's education									
Less than High school	0.06	0.05	0.06	0.06	0.04	0.05	0.07	0.03	0.05
	(0.23)	(0.22)	(0.25)	(0.24)	(0.2)	(0.22)	(0.25)	(0.18)	(0.21)
High school graduate	0.22	0.22	0.23	0.24	0.22	0.23	0.25	0.21	0.22
	(0.42)	(0.41)	(0.42)	(0.43)	(0.42)	(0.42)	(0.43)	(0.41)	(0.42)
Some college	0.19	0.19	0.19	0.22	0.18	0.22	0.23	0.18	0.19
	(0.39)	(0.39)	(0.4)	(0.41)	(0.39)	(0.41)	(0.42)	(0.38)	(0.39)
College graduate	0.32	0.33	0.30	0.30	0.34	0.33	0.28	0.35	0.33
	(0.46)	(0.47)	(0.46)	(0.46)	(0.47)	(0.47)	(0.45)	(0.48)	(0.47)
More than college	0.19	0.20	0.19	0.17	0.21	0.17	0.16	0.22	0.21
	(0.4)	(0.4)	(0.39)	(0.37)	(0.41)	(0.38)	(0.37)	(0.41)	(0.41)
Observations	670,459	316,256	354,203	129,913	470,186	57,831	72,082	225,810	244,376

Means are weighted to account for oversampling of minorities. Standard deviations in parentheses.
Date 1=never goes out on a date, 2=once a month, 3=2 a month, 4=1 or 2 a week, 5=2 or 3 times a week, and 6=more than 3 times a week.

Figure 2: Age-Specific Effects on Highschool Graduation (Coefficient & Confidence Intervals)

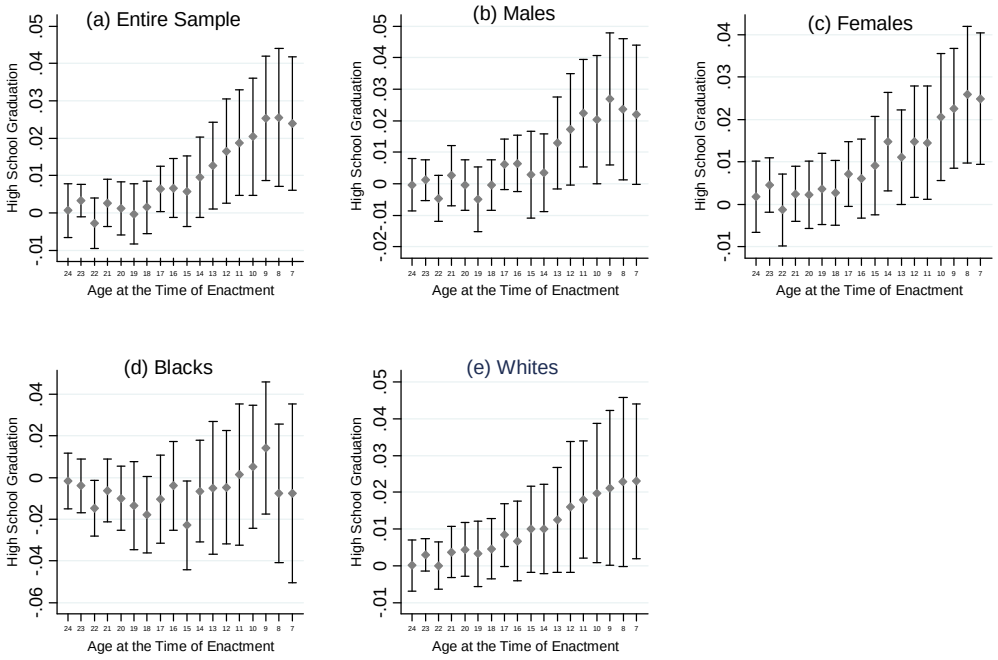

Table 4: The effect of NPND Laws on Education

	(1)	(2)	Observations
	Panel A: High School Graduation		
All	0.008	0.012***	2,550,027
	(0.005)	(0.004)	
Males	0.013**	0.018***	1,256,203
	(0.005)	(0.005)	
Females	0.003	0.006*	1,293,824
	(0.005)	(0.004)	
Blacks	0.016	0.039***	306,809
	(0.012)	(0.012)	
Whites	0.003	0.006*	2,080,771
	(0.004)	(0.003)	
Black Males	0.037**	0.067***	145,220
	(0.017)	(0.020)	
Black Females	-0.002	0.014	161,589
	(0.010)	(0.011)	
White Males	0.004	0.007*	1,030,255
	(0.005)	(0.004)	
White Females	0.003	0.004	1,050,516
	(0.004)	(0.004)	
	Panel B: Completed Years of Education		
All	0.046**	0.024	2,314,035
	(0.022)	(0.015)	
Males	0.064***	0.060***	1,137,609
	(0.022)	(0.018)	
Females	0.029	-0.010	1,176,426
	(0.025)	(0.020)	
Blacks	0.054	0.100*	276,399
	(0.052)	(0.052)	
Whites	0.029	0.005	1,900,353
	(0.022)	(0.017)	
Black Males	0.144**	0.263***	129,855
	(0.067)	(0.078)	
Black Females	-0.026	-0.056	146,544
	(0.045)	(0.066)	
White Males	0.035	0.013	939,750
	(0.024)	(0.022)	
White Females	0.022	-0.005	960,603
	(0.025)	(0.025)	
State-specific Time Trends	Linear	Quadratic	

Regressions include controls for gender, race, and SMSA. Also includes cohort, state and census year fixed effects, unemployment rate, log (per capita income), log (population), log (teacher salaries), log (expenditures per pupil) and pupil per teacher ratio.
Expenditures, salaries, and income are inflation-adjusted.
Standard errors clustered at the state level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of NPND Laws on Education: Robustness Checks for Males and Black Males

	(1) Attainment: Baseline	(2) Graduation: Baseline	(3) Attainment: Entry Age	(4) Graduation: Entry Age	(5) Attainment: NPND States	(6) Graduation: NPND States
			***Panel A** : Males Only*			
Treatment	0.064***	0.013**	0.066***	0.016***	0.048*	0.013**
	(0.022)	(0.005)	(0.022)	(0.005)	(0.026)	(0.005)
Entry Age Laws			-0.004	-0.001		
			(0.006)	(0.001)		
Observations	1,137,609	1,256,203	910,999	1,006,240	512058	571201
			***Panel B:** Black Males*			
Treatment	0.144**	0.037**	0.174**	0.051***	0.161**	0.051**
	(0.067)	(0.017)	(0.070)	(0.017)	(0.064)	(0.020)
Entry Age Laws			-0.004	0.000		
			(0.015)	(0.004)		
Observations	129,855	145,220	108,010	121,244	71146	79350

Regressions include controls for gender, race, SMSA, cohort, state & census year fixed effects and state specific linear time trends Also controls for unemployment rate, log (per capita income), log (population), log (teacher salaries), log (expenditures per pupil) and pupil per teacher ratio. Expenditures, salaries, and income are inflation-adjusted.

Standard errors clustered at the state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Placebo Experiment (Individuals Between 19 to 30 Years of Age)

	(1) Attainment	(2) Graduation
Males	-0.003	0.003
	(0.017)	(0.003)
Observations	241,581	243,939
Black Males	0.029	0.000
	(0.063)	(0.009)
Observations	32,871	33,233

Regressions include controls for gender, race, SMSA. Also includes cohort, state & census year fixed effects and state specific linear time trends, unemployment rate, log (per capita income) log (population), log (teacher salaries), log (expenditures per pupil) and pupil per teacher ratio. Expenditures, salaries, and income are inflation-adjusted.

Standard errors clustered at the state level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 7: Effect of NPND at Different Levels of Education (Males and Blacks Only)

	(1) Grade 8	(2) Grade 9	(3) Grade 10	(4) Grade 11	(5) Grade 12	(6) Some College	(7) College
Males	0.001	0.001	0.011***	0.012***	0.013**	0.011**	0.006
	(0.001)	(0.001)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Black Males	0.007*	0.008	0.015	0.025*	0.037**	0.021	0.012**
	(0.004)	(0.005)	(0.011)	(0.014)	(0.017)	(0.013)	(0.006)

N= 1256203 (Males) and 145220 (Black Males). Regressions include controls for gender, race, and living in a SMSA.
All regressions include cohort, state & census year fixed effects and state specific linear time trends
Also includes unemployment rate, log (per capita income), log (population), log (teacher salaries), log (expenditures per pupil) and pupil per teacher ratio
Expenditures, salaries, and income are inflation-adjusted.
Standard errors clustered at the state level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 8: NPND Laws and Time Allocation Among Teenagers

	(1) Probability of Skipping School	(2) Weekly Hours of Homework	(3) Weekly Hours Watching TV	(4) Weekly Working Hours
All	-0.018**	0.197**	-0.030	-0.105**
	(0.007)	(0.082)	(0.038)	(0.047)
Observations	362,458	261,414	265,405	178,840
Males	-0.015	0.359**	-0.017	-0.145***
	(0.011)	(0.168)	(0.036)	(0.054)
Observations	169,479	126,462	128,303	84,783
Females	-0.020**	0.062	-0.043	-0.077
	(0.008)	(0.215)	(0.046)	(0.069)
Observations	192,979	134,952	137,102	94,057
Blacks	-0.020	0.964***	-0.194***	-0.180***
	(0.013)	(0.258)	-0.063	(0.064)
Observations	69,269	40,773	42,182	31,689
Whites	-0.016	-0.112	-0.004	-0.072
	(0.011)	(0.142)	(0.043)	(0.052)
Observations	256,087	192,574	194,368	132,505
Black Males	-0.016	1.123***	-0.137*	-0.203*
	(0.020)	(0.293)	(0.074)	(0.113)
Observations	30,509	18,906	19,583	13,853
Black Females	-0.025	0.838**	-0.244**	-0.152**
	(0.021)	(0.375)	(0.102)	(0.071)
Observations	38,760	21,867	22,599	63,536
White Males	-0.017	0.060	-0.027	-0.153
	(0.015)	(0.232)	(0.047)	(0.113)
Observations	121,694	94,118	94,938	17,836
White Females	-0.015	-0.286*	0.016	0.002
	(0.010)	(0.169)	(0.047)	(0.078)
Observations	134,393	98,456	99,430	68,969

Regressions include dummies for race, sex, age, SMSA and highest parental education.
All include year and state fixed effects, state-specific time trends,unemployment rate, log (per capita income), log (population), log (teacher salaries), log (expenditures per pupil), pupil per teacher ratio, log (traffic fatalities), and log (vehicle miles).
Expenditures, salaries, and income are inflation-adjusted. Regressions are weighted to adjust for oversampling of minorities.
Standard errors clustered at the state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Effect of NPND on Driving Outomes Among Teenagers (MTF)

	(1) License	(2) Accidents	Observations
All	0.007**	-0.016	731,960
	(0.003)	(0.012)	
Males	0.009***	-0.014	347,990
	(0.003)	(0.012)	
Females	0.005	-0.018	383,970
	(0.004)	(0.013)	
Blacks	0.019***	-0.041**	144,997
	(0.003)	(0.020)	
Whites	0.005	-0.017	509,679
	(0.004)	(0.015)	
Black Males	0.017***	-0.042**	64,978
	(0.005)	(0.020)	
Black Females	0.021***	-0.040*	80,019
	(0.003)	(0.024)	
White Males	0.008**	-0.016	246,553
	(0.004)	(0.014)	
White Females	0.002	-0.019	263,126
	(0.005)	(0.017)	

Regressions control for race, sex, age, SMSA and highest parental education.
Also includes year and state fixed effects, state-specific time trends, unemployment rate, log (per capita income), log (population), log (teacher salaries), log (expenditures per pupil), pupil per teacher ratio, log (traffic fatalities), and log (vehicle miles).
Expenditures, salaries, and income are inflation-adjusted.
Weighted to adjust for oversampling of minorities.
Standard errors clustered at the state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.