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No Pass No Drive: Education and Allocation of Time

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Abstract

Since the late 1980s, several states in the US have introduced No Pass No Drive (NPND) laws that set minimum academic requirements for teenagers under 18 to obtain a driving license. Using data from the U.S. Census and Monitoring the Future (MTF) survey, we exploit cross-state, cross-cohort and cross-time variation in NPND laws to study their effect on educational outcomes and allocation of time among teenagers.

Estimates using the Census show that NPND laws have a positive and significant effect on both years of completed schooling and the probability of high school completion among boys and blacks, but not girls. Our results are robust with respect to several internal validity checks. Using the MTF, we show that NPND laws were effective in reducing truancy. We also find evidence of increased time allocated to school-work at the expense of leisure and employment activities.

Keywords : Education Incentives; Allocation of Time; Dropout; No Pass No Drive Laws

JEL Classification : *J*08 ; *I*2

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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 boys and blacks. This phenomenon has been termed the "silent epidemic" and has forced states to take several initiatives to keep students in school. Among the different interventions that have been introduced, much attention has been paid recently 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] Most of 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, improving school infrastructure etc.). Furthermore, many of these studies find that girls respond better to financial incentives compared to boys.

Though a surprisingly large number of studies have evaluated the effect of performance-based incentives, not much is known about how negative incentives or "sticks" affect education outcomes. In a recent survey of high school dropouts, 38 percent of respondents cited "too much freedom" and too many distractions as a factor in their decision to drop out from high school.[3] In the same survey, 68 percent felt that their respective schools should have tried to stop students from skipping classes. This suggests that a policy that addresses both school attendance requirements and out of school distractions might be an effective way to

[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), Dee (2011), Fryer (2010) and Kremer, et al. (2009).

[3] Bridgeland, J.M., Dilulio, Jr., J.J., and Morison, K.B. (2006, March). The silent epidemic: Perspectives of high school dropouts.

keep students in school.

In this paper, we study the effect of one such policy, the No Pass No Drive (NPND) law, on education outcomes. We take advantage of a natural experiment to answer whether "sticks" might be more cost-effective than "carrots" to increase educational attainment among teenagers, and if so, through what channels.

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 work 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 in, 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 behavior. For instance, Kentucky implemented the NPND legislation in August 2007. According to the state 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). Similarly, Florida passed NPND law in 1997 in an attempt to reduce truancy and improve academic performance. In 2010, the state suspended 5,389 students' licenses for truancy, and sent warnings to another 24,090 students with learner's permit who were at risk for a delay in getting their license.[4]

The aim of this paper is twofold. First, we study the effect of a negative incentive policy on long run education outcomes. On the one hand, imposing minimum academic requirements 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

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

his driving privileges might in fact increase dropout rates and decrease lifetime income. We use data from the 2009 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 education outcomes among boys and blacks, but not girls. In particular, it led to a 2.8 percentage point increase in average educational attainment among black males and a 6.4 percentage point increase in the probability of graduating from high school. This result is particularly relevant because dropout rates are alarmingly high among blacks and males. Moreover, several studies suggest that males are less likely to respond to financial incentives.

Second, we study the channel through which NPND policy has an effect on education. In particular, if this policy changes time invested in education, it should also affect allocation of time between leisure and work. We use a differences-in-differences approach with repeated cross-sectional data of high school students from the Monitoring the Future (MTF) survey to confirm changes in time allocation that led to an increase in education. We find that NPND laws were effective in reducing truancy, including increased school attendance among females. We also find evidence of increased time allocated to homework, mainly among blacks, at the expense of leisure and employment activities.

These results reassure that the laws were effective in increasing educational attainment among youth and shifted time invested in education in expense of leisure and work. Further, the largest gains in education outcomes are for males and disadvantaged groups. Results for both sets of data are robust to including state-specific education, traffic and economic controls. The results are also robust to several checks to internal validity threats.

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 used in the analysis and presents the empirical strategy. In Section 4 we show the main results from the study. Finally, we conclude the discussion in Section 5.

2 Background and Related Literature

The effect of *carrots* or positive incentives on education outcomes have been well documented and debated. Among social scientists the popular view for over 30 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 of education 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 female students but not among males. This is consistent with an Israeli study by Angrist and Lavy (2009) who find a positive effect on matriculation rates among girls, but not boys, who were provided cash incentives to complete a matriculation certificate. Bettinger (2010) finds more direct evidence that incentives did not lower measures of intrinsic motivation among elementary-school students in a low-income section of Ohio. In large scale randomized trials done in four U.S. cities, Fryer (2010) shows that incentives that are linked to inputs (such as attendance, homework, good behavior, etc.) lead to an improvement in student achievement. In comparison, incentives that are conditional on performance are much less effective.

So far, there has been little discussion 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. As a result, empirical work on the effect of nega-

tive incentives on outcomes has been largely ignored even though such means are commonly used by parents and teachers.

To our knowledge, only two papers have addressed this issue in the literature. 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 female college students. Another typical form of negative incentive is to require students to improve their performance in order to gain a particular privilege. 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.[5]

The results of these two papers seem to be opposite, but in theory, penalizing students for not meeting academic standards might actually raise or lower high school graduation rates. On the one hand, academically marginal students who want to stay in college (or, play high school sports) may be motivated to work harder to remain in college (or, on the school sports team). 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 (or, getting to play high school sports). If the second effect dominates the first, graduation rates might actually decline as a state adds another course 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.

Unlike *carrots*, negative incentives would be most effective if they target an activity that

[5] A key 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.

students have a preference for. 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 be effective.

NPND laws differ from the policies analyzed in Lindo et. at (2009) and Vidal-Fernández (2011) in a relevant aspect. While these laws limit access to driving privileges and therefore are also a negative incentive, they do not affect the utility of staying in school, but instead, they make the outside option of dropping out less attractive if students have a preference for driving. Therefore, it is not so obvious why NPND laws should negatively affect dropout rates.[6]

Our contribution to the literature is two-fold. This is the first paper that studies the effect of a negative incentive policy, that links driving privileges to school attendance, on long run education outcomes in the U.S..[7] Second, using a unique dataset of 8^{th} and 12^{th} graders, we also study the effect of the law on time allocation in order to understand the channel through which it effects education. To our knowledge, this is the first study that has used individual level data from the MTF to look at time use outcomes among students.

It is important to note that NPND laws should directly effect only those individuals who have a strong preference for driving. Driving among teenagers is considered an integral aspect of maturation and socialization processes. If we assume that males have a stronger preference for driving, we should expect a larger effect of NPND for them. Moreover, the law would have the largest impact on marginal students who are at a high risk of dropping out. Thus, intuitively, this law should be most effective in addressing attendance issues among males and disadvantaged groups.

[6] We could potentially find a negative effect on education if students who drove to school are no longer able to attend school because they lose their driving license.

[7] Lee (2011) exploits state variation in the repeal of Sunday closing laws to study the effect of policies outside of school on education outcomes. She argues that these laws affected education through their effect on time-competing options among youth.

3 Data and Empirical Framework

3.1 NPND Laws

Over the past two decades, 26 states have implemented policies that link driver licenses to school attendance, academic performance, or behavior. In 1988, West Virginia became the first state in North America to revoke or deny driving privileges to teenagers who do not show satisfactory progress in school. Wisconsin followed suit. We compiled data on NPND laws from state legal statutes for the period 1988 to 2008. As of 2008, 26 states have implemented NPND laws. The highlighted area in figure 1 refer to those states that had the law in place in the most recent year in our sample.

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 most states. In most cases, the law is applicable until the individual turns 18.

3.2 American Community Survey

To study the effect of NPND laws on education outcomes, we use data from the 2009 round of the U.S. Census Bureau's American Community Survey (ACS). We match data from the ACS with information on 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, the birth cohorts examined span from 1958 to 1990. We do this to ensure that the youngest individual in our sample is at least 19 years old and has completed high school (i.e. someone born in 1990). This also ensures that the oldest individual was 30 years old when the first law was passed in 1988 (i.e. someone born in 1958). For the analysis on completed years of schooling, we restrict the sample to those

individuals who are at least 24 years old. Therefore, the sample consists of cohorts born between 1957 and 1985.

Our baseline specification to study the effect of NPND laws on education outcomes is given by,

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

Where, $Treatment_{sc}$ 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 younger than 13 in the year the law was passed. We chose age 13 because it is the youngest age at which teenagers are eligible for drivers license in our data (see Table 1). The control group ($Treatment_{sc} = 0$) are those individuals who were older than 18 when a law was passed in their state. Individuals between the ages of 14 to 18 in the year the law was passed are omitted from the sample because we cannot identify to what extent they would have been affected by the law. S and B refer to state of birth and year of birth fixed effects, X_{isc} includes controls for gender and race. R_{sc} includes a set of state-specific demographic (log population), economic (log per capita income and unemployment rate), and education controls (log of per pupil expenditure, the pupil teacher ratio and log of teacher salary)[8] associated with the birth cohort at age 13. All income and expenditure variables are inflation adjusted.

We merge data on NPND laws with the census 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. Standard errors are clustered by state and year (Bertrand, Duflo & Mullainathan, 2004). The entire sample consists of 1,059,305 observations including states that never passed NPND laws.

Our coefficient of interest, β_1, in this specification is identified using cross-state and cross-cohort variation. For instance, in California, where law was adopted in 1991, $Treatment_{sc}$=1

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

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 the time.

The crucial identifying assumption is that education 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 education outcomes were reacting to other laws that were being implemented around the same time, our estimates would 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 education 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 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, we include state-specific linear time trends in the regressions. Fourth, we directly test if our results are being driven by minimum school entrance age laws that were being passed in states around the same time as NPND laws. 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 education outcomes of older cohorts.

Table 2 presents the descriptive statistics for key variables used in the baseline specifi-

cation. The average educational attainment in the sample is 13.5 years with a high school graduation rate of 87 percent.[9] As expected, females have higher education levels than 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. The next subsection describes this data in detail.

3.3 Monitoring the Future (MTF)

The MTF surveys approximately 50,000 12^{th} graders every year since 1975 and 8^{th} and 10^{th} graders since 1991.[10] 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. A multistage random sampling procedure is used to draw a nationally representative sample of students from approximately 135 schools. First, the geographical areas are randomly selected, then schools within areas are chosen with the probability being proportional to the size of senior school classes. In the final step, full classes are randomly selected within schools.

Since the MTF has been surveying $8^{th}$and 10^{th} since 1991, our sample comprises data on 15-17 year olds from 1991 onwards. We study 15-17 years olds because NPND laws only affect minors who are eligible for a driver's license, and in most states minimum age for a

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

[10] 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 kindly agreed on running our programs on site and provided us with the output tables.

learner's permit is 15 years.

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. For our study, we focus on survey questions that indicate the channels through which NPND laws might affect the allocation of time between educational investment, work, and leisure. For instance, the MTF asks respondents whether they work and the number of hours they work. We use this variable to study the effect of NPND laws on allocation of time towards work. The survey also asks respondents how often they go out for parties or on dates, play sports, watch TV etc. We use these variables to proxy for leisure activities. We also test if NPND laws have an effect on time invested in education activities. We use the time spent doing homework and the probability of skipping school as proxies for investment in education. Finally, as an extension to our basic framework, 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. There are no statistically significant differences by race or gender in the background characteristics. However, we can see some interesting differences in the choices made. For instance, consistent with a broad literature on gender differences in academic achievement, in our sample, females have a higher Grade Point Average (GPA) than boys. Also, blacks have lower average GPA than non-blacks. We also find truancy to be most prominent among blacks. Among the different groups, females spend the highest amount of time per week doing homework while blacks have the lowest hours. When we look at employment, the raw data suggests that boys are slightly more likely to work than girls. Interestingly, there are no significant differences across the groups in leisure activities such as going out on dates or to parties. However, blacks spend much more time, on an average, watching television.

For the MTF, we estimate the following model:

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

where i denotes individual, s denotes state, and t refers to time. Y is the outcome of interest (education, 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, 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). All income and expenditure variables are inflation-adjusted. S and T are state and time fixed effects, respectively. Standard errors are clustered both at the year and state levels.

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, as shown in Section 4, our results suggest that high schools students in NPND states were more likely to get license and were driving more. We also show an effect on work hours. 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 expense of either leisure, work or both.

Second, the MTF is a selected sample of teenagers who have not dropped out from school. This would be a problem if we want to measure the effect of NPND laws on allocation of time among teenagers who drop out of school before the law is passed. If some of them drive to work, we would expect a drop in their work hours after the policy is implemented. We acknowledge this data constraint and interpret the MTF results as the effect of the law on allocation of time among the selected sample of individuals who are attending school.

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. For example if NPND laws were enacted together with other traffic laws affecting teenagers, we would find a decrease in leisure and in probability of driving for teenagers which may not entirely be attributed to NPND laws. 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. Our results are robust to addition of these variables.

4 Results

4.1 American Community Survey (2009)

Table 4 shows results for the effect of NPND laws on high school graduation. Column (1) reports estimates from the sparest specification without any control variables. As expected, the sign on the treatment variable is negative and should be interpreted as NPND laws being enacted in states with low graduation rates. However the coefficient switches signs upon including state and cohort fixed effects in column (2). The complete model in column (3) suggests that NPND laws had a positive and significant effect on high school graduation rates. In particular, NPND laws are associated with an increase of 1.1 percent in graduation rates. The effect is slightly larger for boys than for girls. However, given that the graduation rate for girls is higher than for boys (Table 2), these numbers translate to almost similar percentage changes in graduation rates for both groups. The specifications in Columns (3)-(5) include state level macroeconomic controls for unemployment rate, log of per capita income and log population. As can be seen from comparing columns (2) and (3), the estimates are robust

to inclusion of state macroeconomic and education controls.

Table 5 shows analogous regression estimates with education attainment as the outcome variable. Once again, NPND laws led to an increase in education attainment among cohorts affected. However, unlike the results for high school completion rates, the effect on educational attainment is only visible for boys. This is an interesting result because several studies on financial incentives find that girls react to positive incentives or *carrots* while boys do not. However, the results from Table 5 show that boys are reacting to *sticks* more than girls. We cannot determine if that is because boys react more than girls to carrots or because boys have a preference for driving and girls do not.

One of the main concerns in these estimates is that the results may be influenced by underlying state-specific trends. More importantly, education attainment among girls has been increasing during the period of study and this could be biasing the results in Table 5. Since NPND laws vary both by cohorts and by state, we cannot include state-cohort interactions. Nonetheless, in Table 6 we include state-specific linear time trends to address this concern.

Columns (1), (2) and (3) show results for educational attainment as the dependent variable while columns (4) to (6) show results for graduation rates. As suspected, upon inclusion of state-specific time trends, Table 6 shows that NPND laws have no significant effect on education outcomes for females. The coefficients in columns (3) and (6) are close to zero and statistically insignificant. Among boys, the treated cohorts have 0.1 more years of education and are 1.5 percentage point more likely to graduate from high school. Moreover, both effects are statistically significant at 1%. The mean attainment and graduation rates among males is 13.29 years and 0.84 respectively. Thus, as a result of NPND laws, males have 0.8 percentage points higher educational attainment and are 1.8 percentage point 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 would have the largest effect on marginal students who are at the risk of dropping out, and these students

are least likely to invest in higher levels of education.

Since black youths constitute a disproportionately large proportion of dropout population, and NPND targets teens at risk of dropping out, we should expect a larger effect for this subgroup.[11] Table 7 presents the estimates from separate regressions by race among males.

Black cohorts affected by the NPND law have 0.35 more years of education and are almost 5% more likely to graduate from high school. This is a large effect and translates to a 2.8 percentage point increase in average educational attainment among black males (the mean education for this group is 12.53 years) and a 6.4 percentage point increase in the probability of graduating from high school (mean graduation is 0.73). We also find a marginal positive effect on educational attainment among white males though the estimated coefficient is measured with imprecision and is significant only at the 10% level. Taken together, the results suggests that the law had the largest effect on males and in particular among disadvantaged groups.

To put things in perspective, the results seem to be consistent with the idea that the effect of NPND laws should be the largest for students who have a preference for driving and are disadvantaged. Thus, assuming that males have a greater preference for driving, it is not surprising that our results suggest that women are least likely to be affected by NPND laws and black males are most affected.

4.2 Robustness Checks

A potential concern with our identification strategy is that education outcome may be affected by other unobserved education policies that were also changing around the same time as NPND laws. One policy that has received a lot of attention recently is the minimum school entry age laws. In the 1960s children were allowed to start kindergarten when they

[11] We only report results for males by race. In similar regressions for females, as expected from results shown in Table 6, none of the estimates were significant at conventional levels. These tables are available upon request.

were considerably less than five years old. However, over the last four decades, there has been a shift in policy and most states have increased the minimum entrance age. If school entry age laws changed around the same time as NPND laws, our results would not correctly capture the effect of NPND laws. This is even more relevant because the literature finds that older children tend to perform better in school and complete more years of schooling (Barua and Lang, 2010).

To address this concern, we estimate the regressions controlling for the minimum age 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). Similarly, if the state law requires the child to turn 5 by December 1^{st}, the youngest entrant to kindergarten would be 4 years and 9 months old (i.e. 57 months). 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 8, Columns (3) and (4), show results for the effect of NPND laws on educational attainment and graduation rates, respectively, controlling for the minimum school entry age (in months). We only present estimates among males (shown in panel A) and black males (shown in panel B), the group that is most affected by the policy.[12] Columns (1) and (2) reproduce results from table 6 and table 7 for education outcomes among males and black males. The inclusion of the entry age variable has a negligible effect on the NPND coefficient for both education outcome variables for males. Moreover, the coefficient on entry age variable is close to zero and statistically insignificant. Including minimum entry age makes the coefficient on black males (panel B) larger and the estimates are still highly significant. The effect on graduation almost doubles and is now 0.074 which translates to a 10 percentage point increase in graduation rates among black males.

As an additional internal validity test, we use a "fake" treatment group to see if education

[12]Regressions for women and the entire sample also yield estimates that do not change with inclusion of entry age variable.

outcomes are reacting to any other factors that affect different cohorts in a particular way. Individuals who were more than 19 years old at the time of the enactment of the NPND law 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 where the "treatment" group includes individuals who are between 19 and 21 years of age and the "control" group comprises of individuals between ages 22 to 24. If the regression estimate is significantly different from 0 for the placebo groups, the trends are not parallel, and our original estimate is likely to be biased. As we can see in columns (5) and (6) in table 8, for both males and blacks, the coefficient on NPND for the placebo groups is close to zero and statistically insignificant. However, the coefficient in column (6) on black males is non trivial, though the standard errors are larger possibly due to the small sample size.

Even if other policies are not confounding our estimates, the policy intervention could be a response to a third factor that simultaneously influences the policy implementation and education 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 forbid their children from driving to school and that influences their allocation of time and education outcomes. Moreover, one can argue that NPND laws will be more effective in states where vehicle usage is higher due to geographic factors or cultural reasons. Therefore, only states in which the policy would have been effective apply NPND laws and the outcome depends on who is being treated.

Table 9 presents results from the ACS with 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. The data on vehicle miles is collected from the Federal Highway Administration (FHWA). The traffic fatalities data is collected from the Fatality Analysis Reporting System (FARS). To be consistent with the other variables, we merge this data corresponding to the year the individual turns 13. However, FARS data is only available since 1975,

whereas the oldest cohort in our sample turns 13 in 1971 (i.e. those born in 1958). Thus, table 9 excludes data from 1971 until 1974 and that explains the difference in number of observations from previous tables.

We show results for the entire sample, males and black males. Upon inclusion of traffic variables, the coefficients are even larger in magnitude, especially for black males in column 3 and 6, and are still estimated with a lot of precision. Moreover, in column 5, the coefficient on teen traffic fatalities is negative and significant for the graduation regression.

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

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). Results are shown for the complete specification outlined in section 3.3 and includes all individual level control variables, state/year level education and macroeconomic controls and two traffic control variables.[13] All regressions also include state effects, year effects and state-specific time trends. For all tables, column (1) shows results for the entire sample, columns (2) and (3) estimates the model separately by gender while columns (4) and (5) present estimates by race for blacks and non-blacks respectively.

Table 10 reports estimates with school-related outcomes as the dependent variable. Panel A shows results for grades, panel B reports estimates for probability of skipping school and in panel C the outcome variable is hours spent doing homework. Though the effect of NPND

[13]To be consistent with the census data, in all the MTF tables shown below, we include the same set of control variables that we used in the census estimates. However, in tables not shown in the paper (but 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.

on grades is positive for all groups except females, none of the coefficients are statistically significant. Thus it seems that NPND laws have no effect on academic performance.

In most states the law not only requires that the teenager be enrolled in school but also enforces a minimum attendance requirement. Panel B shows results for equation (2) where the outcome of interest is likelihood of skipping school. Teens who are in states with the NPND law are 7 percentage point less likely to be truants and the effect is significant at 5% (the coefficient is 0.018 while the mean for days skipped is 0.26). Interestingly, when we compare columns (2) and (3), we find that the effect is larger for females and insignificant for males.

A possible explanation for this result could be sample selection. The MTF only records information for non-dropouts. In states with NPND laws, the sample includes individuals who were at the margin for 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 census suggests 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. If the bias due to selection is large enough, we might find that there is no effect (or even negative) of NPND. This would be more true for blacks, who are more disadvantaged, than for girls and that might be an explanation why the coefficients are positive for girls and not for blacks.

Finally, in panel C, we study the effect on hours spent in doing homework. Blacks spend about 1 more hour doing homework each week and the result is significant at 1%. This is a large effect relative to the average (5.74 hours) and translates to a 17% increase in the average daily time spent doing HK. We also find that in a state with an NPND law, the average male spend more hours doing homework.

In Table 11, we present results where the outcome variable is employment. Panel A reports estimates for probability of working while panel B shows results on hours spent

working as the dependent variable. The coefficient for each of the groups is close to zero in panel A suggesting that NPND has no effect on a teenagers probability of employment. However, we do find a decrease in hours spent on the job each week. 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. There seems to be substitution going on between work and study, however, we explore this further by looking at the effect on leisure activities.

Table 12 reports estimates for the effect of NPND on leisure activities where leisure is proxied by the number of times a teenager goes out every week on dates, parties and hours spent watching television.[14]

MTF asks students "how often do you go out with a date?" The response categories are: never; once a month or less; 2 or 3 times a month; once a week; 2 or 3 times a week; over three times a week. The estimated coefficients in Panel A are from an ordered probit regression that take into account this count nature of the variable.

The sign of the coefficients suggests that students in NPND states decreased the frequency of going out on dates. The effect is largest for non-blacks and females and significant at 1%, while blacks are not decreasing their frequency of going out on dates. Panel B shows ordered probit estimates for the frequency of going out for parties. The results suggest that women are reducing time invested in leisure activities but there is no strong evidence for males. This points towards the selection of non-dropouts in the sample. Since NPND does not have a significant effect on the dropout rates for women, we observe a clear decline in leisure activities for them. On the other hand, for boys and blacks, NPND let to an increase in graduation rates. However, the law would have the largest effect on individuals who were at the margin of dropping out. These students would also be most likely to be heavy consumers of leisure activities. Thus, including them in the MTF sample makes the results less clear

[14]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.

for this group.

Finally, from Panel C, we observe that blacks in states with the NPND laws are spending less time watching television. They spend about 6 percent less time watching TV and the effect is also highly statistically significant at 1%.

To sum up, NPND laws led to a redistribution in allocation of time with respect to work, study and leisure. In particular, blacks and males are spending more time doing school work and less time working. Moreover, blacks, in states with NPND laws, are spending less time watching television. Among women, there is an increase in school attendance at the expense of leisure activities but not work. For the entire sample, we find less truancy, fewer hours spent on the job and decrease in frequency of going out on dates. It is important to keep in mind that, due to having only non-dropouts in the sample, the MTF results are underestimating the true effects of the law. However, it is reassuring that women, for whom we find no significant effect on education outcomes in the ACS, are also reallocating their time as a result of NPND laws.

4.4 Driving Outcomes

As an extension of our basic framework, we also studied 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 12th graders.

Table 13, panel A, B and C, presents estimates for the effect of NPND laws on "probability of holding a driving license", "miles driven in a car per week" and "probability of having an accident in the last 12 months" respectively.[15] The coefficients in Panel B for driving are from an ordered probit model. The results indicate that all groups except non-blacks have a high likelihood of holding a driving license with the largest effect, 2 percent, among blacks. Moreover, in states with NPND laws, blacks are driving more miles per week and are

[15] We also studied the effect of NPND on some other driving related outcome variables in the MTF dataset: driving under the influence of alcohol/drugs and seatbelt use. We do not find any significant effects on these outcome variables. Tables are available upon request.

4 percentage points less likely to have traffic accidents. We also find a negative coefficient on accidents for females, however, the coefficient is much smaller in magnitude and is imprecisely estimated.

It is not surprising that the effect of NPND laws on driving licenses and miles driven 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.

What is not clear is whether the effect on accidents can be interpreted as causal. Theoretically, the mechanism through which education affects accidents can be compared to the literature that measures the effect of education on negative externalities with large social costs, such as crime. 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. Thus, one interpretation of these results is that NPND laws, indirectly through its effect on education, could also have externality effects on accidents.[16]

However, another interpretation that is consistent with the results is that 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 dataset that has individual level data on accidents, education and state level identifiers that allows us to test these different interpretation of our driving results. We leave that for future research.

[16] We have also attempted to test this theory using the Fatality Analysis Reporting System (FARS) that maintains data regarding 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 using the data yielded negative but statistically insignificant results. However, this data is at the state level and only includes accidents that led to a fatal outcome.

5 Discussion

Parents and educators use many discipline methods that involve *carrots* to tempt a child to cooperate and behave well or alternately use *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 boys. Moreover, though the effect of positive incentives on *performance* is well researched, their effect on long run outcomes such as educational *attainment* is less clear.

Negative incentive policies are not so popular among policy makers because they decrease choices available to children and the benefits might be short-run. Moreover empirical researchers would confront ethical conflicts when running randomized experiments with 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 parents be enrolled in school to receive welfare payments.[17] In the U.S., high school students who do not pass a certain number of subjects are not allowed to play sports. Thus, it is surprising that not much research has evaluated the effect of negative incentive policies on educational achievement.

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 attainment among

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

affected cohorts and the effect is mainly driven by boys and blacks. This has several policy implications. First, in addition to having direct implications on the labor market, this could also have externality effects. 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 to the same level as that of whites. Second, the increase in years of completed education is especially striking when one considers that the NPND policy is almost costless to states. On the other hand, financial incentive programs are costly to implement and their effect on *long term* education outcomes has not been well researched, at least in the United States. Moreover, for developing countries, conditional cash transfers end up occupying significant portions of total education budgets. Third, the dropout rates are alarmingly high among disadvantaged groups. Thus the optimal policy must target groups such as blacks and hispanics in particular.

The results from this paper therefore suggest that it is worth taking advantage of natural experiments to evaluate the intended and unintended consequences of low-cost negative incentive 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 among individuals, especially the disadvantaged groups.

6 References

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Figure 1: States with No Pass No Drive Laws (2008)

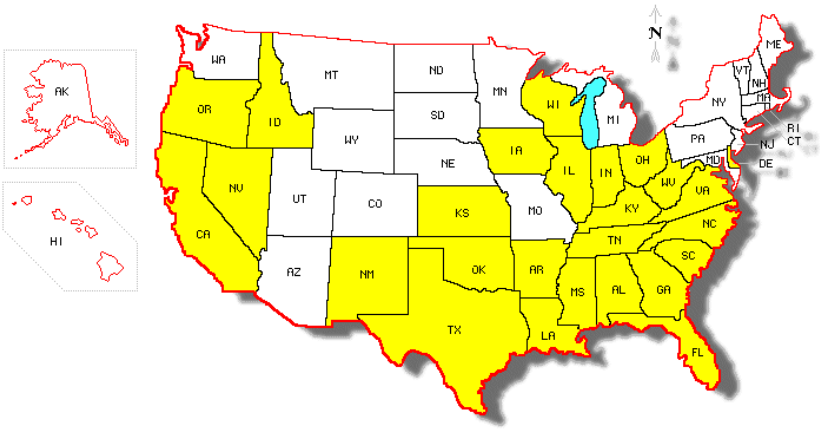

Table 1: Summary of No Pass No Drive Laws in 2008

NPND (2008)	Attendance Requirement	Progress in School	Student Behavior (Suspensions, expulsions etc)	Min Age	Max Age
Alabama	Yes			15	18
Arkansas	Yes			14	18
California	Yes			13	18
Delaware	Yes			-	-
Florida	Yes			15	18
Georgia	Yes			15	18
Idaho	Yes			15	18
Illinois	Yes		Yes	-	18
Indiana	Yes		Yes	15	18
Iowa	Yes			-	18
Kansas			Yes	13	-
Kentucky		Yes		16	18
Louisiana			Yes	15	18
Mississippi		Yes		15	18
Nevada	Yes			14	-
New Mexico	Yes			-	-
North Carolina		Yes		15	18
Ohio	Yes			-	18
Oklahoma	Yes			14	18
Oregon			Yes	15	18
South Carolina	Yes			15	17
Tennessee	Yes	Yes	Yes	15	18
Texas	Yes			15	18
Virginia	Yes			16	18
West Virginia		Yes		15	18
Wisconsin	Yes			16	18

Table 2: ACS 2009 Descriptive Statistics by Cohort

Individual Level Variables					
Cohort		1960	1970	1980	All
Educational attainment		13.49	13.71	13.37	13.48
		(2.10)	(2.05)	(1.91)	(2.03)
	Males	13.36	13.49	13.14	13.29
		(2.18)	(2.12)	(1.96)	(2.10)
	Females	13.62	13.91	13.60	13.66
		(2.02)	(1.95)	(1.84)	(1.94)
High school graduation		0.87	0.88	0.86	0.87
		(0.33)	(0.33)	(0.35)	(0.34)
	Males	0.85	0.85	0.83	0.84
		(0.35)	(0.36)	(0.38)	(0.36)
	Females	0.90	0.91	0.89	0.90
		(0.31)	(0.29)	(0.31)	(0.30)
Males		0.49	0.49	0.50	0.49
		(0.50)	(0.50)	(0.50)	(0.50)
Blacks		0.11	0.11	0.12	0.11
		(0.31)	(0.32)	(0.33)	(0.32)
Observations		356,371	287,352	297,661	1.059,305
State-specific Economic Variables					
Cohort		1960	1970	1980	All
Log (Per capita income)		8.94	9.70	10.12	9.50
		(0.30)	(0.21)	(0.18)	(0.65)
Log (Population)		15.69	15.78	15.94	15.79
		(0.88)	(0.89)	(0.92)	(0.90)
Unemployment rate		6.99	6.99	5.24	6.36
		(2.18)	(2.05)	(1.31)	(2.02)
State-specific Education Variables					
Cohort		1960	1970	1980	All
Log (Expenditure/pupil)		7.98	8.21	8.79	8.30
		(0.23)	(0.25)	(0.39)	(0.48)
Pupil teacher ratio		19.84	17.80	16.87	18.50
		(1.99)	(2.47)	(2.63)	(2.84)
Log (Teacher salary)		10.03	10.09	10.12	10.08
		(0.15)	(0.16)	(0.16)	(0.16)

Salaries and expenditures are inflation-adjusted.
Educational attainment is in years.

Table 3: Monitoring the Future Descriptive Statistics

	All	Males	Females	Blacks	Non-blacks
Education Outcomes					
Grade Point Average (GPA)	6.03	5.78	6.30	5.80	6.1
	(2.18)	(2.21)	(2.11)	(2.13)	(2.1)
Truancy	0.25	0.25	0.23	0.33	0.23
	(0.44)	(0.43)	(0.42)	(0.47)	(0.42)
Hours doing homework	6.85	6.25	7.45	5.74	7.06
	(6.40)	(6.08)	(6.65)	(6.03)	(6.45)
Employment Outcomes					
Work	0.50	0.51	0.47	0.52	0.48
	(0.50)	(0.50)	(0.50)	(0.81)	(0.50)
Hours worked a day	2.50	2.61	2.39	2.71	2.44
	(2.05)	(2.13)	(1.97)	(2.24)	(2.00)
Leisure Activities					
Going out on dates	2.57	2.59	2.55	2.58	2.57
	(1.59)	(1.56)	(1.62)	(1.60)	(1.59)
Going out for parties	3.11	3.08	3.13	3.09	3.11
	(0.95)	(0.98)	(0.91)	(1.01)	(0.94)
Hours watching television per day	2.54	2.62	2.46	3.56	2.38
	(1.54)	(1.53)	(1.54)	(1.50)	(1.48)
Age	16.5	16.48	16.51	16.65	15.91
	(0.85)	(0.83)	(0.86)	(1.76)	(1.67)
Lives in a Statistical Metropolitan Area	0.77	0.76	0.77	0.80	0.75
	(0.42)	(0.42)	(0.42)	(0.4)	(0.43)
Max. parent's education some high school	0.06	0.05	0.06	0.05	0.05
	(0.23)	(0.22)	(0.25)	(0.22)	(0.22)
High school graduate	0.22	0.21	0.22	0.20	0.21
	(0.41)	(0.41)	(0.42)	(0.4)	(0.41)
Some college	0.20	0.19	0.20	0.18	0.17
	(0.4)	(0.4)	(0.4)	(0.4)	(0.38)
College graduate	0.32	0.33	0.30	0.25	0.3
	(0.47)	(0.47)	(0.49)	(0.43)	(0.46)
More than college	0.19	0.20	0.19	0.26	0.18
	(0.39)	(0.39)	(0.39)	(0.43)	(0.39)
Observations	914,910	425,509	452,250	215,455	699,455

Weighted statistics

The GPA variable is recoded as D=1, C - =2, and so on up to A=9.

Going out for dates & party are the average nights a week a student goes out at night/party and ranges from zero to three or more

Table 4: The Effect of NPND Law on High School Graduation (ACS 2009)

	(1)	(2)	(3)	(4)	(5)
			All	**Males Only**	**Females Only**
Treatment	-0.030***	0.011***	0.011***	0.012***	0.010***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)
Male			-0.053***		
			(0.001)		
White			0.063***	0.070***	0.057***
			(0.003)	(0.003)	(0.004)
Black			-0.019***	-0.049***	0.007*
			(0.003)	(0.004)	(0.004)
Pupil teacher ratio			-0.001	-0.001	-0.001
			(0.001)	(0.001)	(0.001)
Log (Teacher Salary)			0.006	0.008	0.004
			(0.011)	(0.015)	(0.014)
Log (Expenditure/pupil)			0.013	0.008	0.017
			(0.009)	(0.013)	(0.011)
State of birth		✓	✓	✓	✓
Year of birth		✓	✓	✓	✓
Observations	987,654	987,654	987,536	486,488	501,048

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
Columns (3)-(5) include unemployment rate, log (per capita income) and log (population)
Expenditures, salaries, and income are inflation-adjusted.

Table 5: The Effect of NPND Law on Educational Attainment (ACS 2009)

	(1)	(2)	(3)	(4)	(5)
			All	**Males Only**	**Females Only**
Treatment	-0.127***	0.001	0.045***	0.071***	0.019
	(0.008)	(0.021)	(0.015)	(0.020)	(0.018)
Male			-0.357***		
			(0.006)		
White			0.414***	0.437***	0.392***
			(0.019)	(0.022)	(0.021)
Black			-0.249***	-0.395***	-0.121***
			(0.022)	(0.026)	(0.025)
Pupil teacher ratio			-0.014***	-0.014***	-0.014***
			(0.003)	(0.005)	(0.004)
Log (Teacher Salary)			0.155**	0.104	0.198**
			(0.076)	(0.100)	(0.098)
Log (Expenditure/pupil)			0.095	0.103	0.093
			(0.065)	(0.085)	(0.079)
State of birth		✓	✓	✓	✓
Year of birth		✓	✓	✓	✓
Observations	839,643	839,643	839,643	411,734	427,909

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
Columns (3)-(5) include unemployment rate, log (per capita income) and log (population)
Expenditures, salaries, and income are inflation-adjusted.
Educational attainment is in years.

Table 6: Effect of NPND Laws on Educational Attainment and High School Graduation with State-specific Time Trends (ACS 2009)

	(1) Attainment All	(2) Attainment Males	(3) Attainment Females	(4) Graduation All	(5) Graduation Males	(6) Graduation Females
Treatment	0.062***	0.100***	0.027	0.009***	0.015***	0.004
	(0.023)	(0.031)	(0.030)	(0.004)	(0.005)	(0.004)
Male	-0.357***			-0.053***		
	(0.006)			(0.001)		
White	0.409***	0.432***	0.388***	0.063***	0.070***	0.057***
	(0.019)	(0.022)	(0.022)	(0.003)	(0.003)	(0.004)
Black	-0.251***	-0.398***	-0.123***	-0.019***	-0.049***	0.008*
	(0.022)	(0.026)	(0.025)	(0.003)	(0.004)	(0.004)
Pupil teacher ratio	-0.008**	-0.007	-0.009*	-0.001	-0.001	-0.001
	(0.004)	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)
Log (Teacher Salary)	0.285***	0.240**	0.330***	0.024**	0.027	0.021
	(0.078)	(0.114)	(0.097)	(0.011)	(0.016)	(0.014)
Log (Expenditure/pupil)	-0.068	-0.083	-0.053	-0.008	-0.007	-0.008
	(0.065)	(0.091)	(0.083)	(0.009)	(0.014)	(0.012)
Observations	839,643	411,734	427,909	987,536	486,488	501,048

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include state and cohort dummies, state-specific linear time trends, unemployment rate, log (per capita income) and log (population)
Expenditures, salaries, and income are inflation-adjusted.
Educational attainment is in years.

Table 7: Effect of NPND Laws on Education by Race (Males Only)

	(1) Attainment Blacks	(2) Attainment Whites	(3) Graduation Blacks	(4) Graduation Whites
Treatment	0.346***	0.055*	0.047**	0.004
	(0.093)	(0.032)	(0.019)	(0.006)
Observations	42,881	345,126	52,329	402,457

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include state and cohort dummies, 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.
Educational attainment is in years.

Table 8: Effect of NPND Laws on Education: Robustness Checks for Males and Blacks (ACS 2009)

	(1)	(2)	(3)	(4)	(5)	(6)
	Attainment: Baseline	Graduation: Baseline	Attainment: Minimum Entry Age	Graduation: Minimum Entry Age	Attainment: Placebo	Graduation: Placebo
			***Panel A**: Males Only*			
Treatment	0.100***	0.015***	0.088***	0.018***	-0.003	0.010
	(0.031)	(0.005)	(0.032)	(0.006)	(0.040)	(0.007)
Entry age			-0.013	0.000		
			(0.009)	(0.001)		
Observations	411,734	486,488	331,142	391,186	43,166	43,166
			***Panel B:** Black Males*			
Treatment	0.346***	0.047**	0.419***	0.074***	0.031	0.032
	(0.093)	(0.019)	(0.102)	(0.020)	(0.095)	(0.021)
Entry Age			0.014	0.005		
			(0.024)	(0.005)		
Observations	42,881	52,329	35,653	43,933	5,372	5,372

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include state and cohort dummies, 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.
Educational attainment is in years.

Table 9: Effect of NPND Laws on Education Controlling for Traffic Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Attainment All	Attainment Males	Attainment Blacks	Graduation All	Graduation Males	Graduation Black
Treatment	0.064**	0.112***	0.445***	0.012***	0.023***	0.073***
	(0.025)	(0.035)	(0.105)	(0.004)	(0.006)	(0.020)
Log (Vehicle miles)	0.015	0.038	0.011	-0.000	-0.001	-0.009
	(0.017)	(0.025)	(0.087)	(0.002)	(0.003)	(0.012)
Log (Traffic fatalities)	-0.033	-0.047	-0.018	-0.003	-0.011**	-0.019
	(0.022)	(0.032)	(0.101)	(0.003)	(0.005)	(0.017)
Observations	678,503	333,301	35,274	826,396	408,055	44,722

Standard errors clustered at the state and cohort levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include state and cohort dummies, 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.
Educational attainment is in years.

Table 10: Effect of NPND Laws on Grades, Skipping Classes, and Homework

	(1) All	(2) Male	(3) Female	(4) Black	(5) Non-Blacks
		Panel A: *Grades*			
NPND	0.024	0.077	-0.022	0.028	0.011
	(0.047)	(0.060)	(0.053)	(0.093)	(0.054)
Observations	355,541	165,814	189,727	66,636	288,905
		Panel B: *Probability of Skipping School*			
NPND	-0.018**	-0.015	-0.020**	-0.020	-0.016*
	(0.009)	(0.011)	(0.009)	(0.017)	(0.010)
Observations	362,458	169,479	192,979	69,269	293,189
		Panel C: *Hours Spent Doing Homework a Day*			
NPND	0.197	0.359**	0.062	0.964***	-0.070
	(0.158)	(0.161)	(0.215)	(0.223)	(0.181)
Observations	261,414	126,462	134,952	40,773	220,641

Standard errors clustered at the state and year levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include unemployment rate, log(per capita income), log (population), log(teacher salaries), log(expenditures per pupil), pupil per teacher ratio, log(traffic fatalities), log(vehicle miles), year & state dummies, and state-specific time trends
Expenditures, salaries, and income are inflation-adjusted.
The grade variable is recoded as D=1, C - =2, and so on up to A=9.

Table 11: Effect of NPND Laws on Employment Outcomes

	(1) All	(2) Male	(3) Female	(4) Black	(5) Non-Blacks
		Panel A: *Probability of Working*			
NPND	-0.001	-0.002	-0.002	0.024	-0.008
	(0.009)	(0.009)	(0.012)	(0.017)	(0.010)
Observations	362,458	169,479	192,979	69,269	293,189
		Panel B: *Hours Worked a Day*			
NPND	-0.105*	-0.145**	-0.077	-0.180**	-0.074
	(0.054)	(0.063)	(0.065)	(0.078)	(0.059)
Observations	178,840	84,783	94,057	31,689	147,151

Standard errors clustered at the state and year levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include unemployment rate, log(per capita income), log (population), log(teacher salaries), log(expenditures per pupil), pupil per teacher ratio, log(traffic fatalities), log(vehicle miles), year & state dummies, and state-specific time trends
Expenditures, salaries, and income are inflation-adjusted.

Table 12: Effect of NPND Laws on Leisure Activities

	(1) All	(2) Male	(3) Female	(4) Black	(5) Non-Blacks
		Panel A: *Going out on a Date*			
NPND	-0.067***	-0.052*	-0.079***	-0.027	-0.081***
	(0.021)	(0.027)	(0.023)	(0.025)	(0.025)
Observations	356,314	165,122	191,192	65,343	290,971
		Panel B: *Going out for Parties*			
NPND	-0.023	0.031	-0.063**	-0.015	-0.025
	(0.023)	(0.026)	(0.026)	(0.031)	(0.023)
Observations	291,930	140,297	151,633	47,974	243,956
		Panel C: *Watching Television*			
NPND	-0.030	-0.017	-0.043	-0.194***	0.022
	(0.033)	(0.039)	(0.037)	(0.063)	(0.034)
Observations	265,405	128,303	137,102	42,182	223,223

Standard errors clustered at the state and year levels.
*** p<0.01, ** p<0.05, * p<0.1
All regressions include unemployment rate, log(per capita income), log (population), log(teacher salaries), log(expenditures per pupil), pupil per teacher ratio, log(traffic fatalities), log(vehicle miles), year & state dummies, and state-specific time trends
Expenditures, salaries, and income are inflation-adjusted.

Table 13: Effect of NPND Laws on Driving and Accidents

	(1) All	(2) Male	(3) Female	(4) Black	(5) Non-Blacks
			Panel A: *License*		
NPND	0.007***	0.009***	0.005**	0.019***	0.004
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Observations	731,960	347,990	383,970	144,997	586,963
			Panel B: *Driving*		
NPND	0.018	0.025	0.009	0.108***	-0.009
	(0.023)	(0.026)	(0.026)	(0.036)	(0.027)
Observations	234,998	106,625	128,373	47,363	187,635
			Panel C: *Accidents*		
NPND	-0.016	-0.014	-0.018*	-0.041**	-0.007
	(0.010)	(0.010)	(0.011)	(0.018)	(0.011)
Observations	731,960	347,990	383,970	144,997	586,963

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include unemployment rate, log(per capita income), log (population), log(teacher salaries), log(expenditures per pupil), pupil per teacher ratio, log(traffic fatalities), log(vehicle miles), year & state dummies, and state-specific time trends
Expenditures, salaries, and income are inflation-adjusted.