“Using State Child Labor Laws to Identify the Effect of School-Year Work on High School Achievement”

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Abstract

This paper uses variation in the labor supply of 12th grade students created by interstate variation in child labor laws to estimate the effect of school-year work on 12th grade math achievement. The IV estimates in this paper indicate that an exogenous decrease in school-year hours worked of ten hours per week would result in a 0.2 standard deviation increase in math scores. Comparisons to OLS estimates suggest that failure to account for the endogeneity of the labor supply decisions of high school students will result in underestimates of the negative impact of school-year work on academic achievement.

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I. Introduction

Part-time work during the high school years has become common place among students in the United States. Students in the 1990s were twice as likely to be working part time as were students in 1950 (Singh 1998), and in 1997 ninety-two percent of males age 22-27 reported some work experience while they were in high school (Hotz et al. 1999). Not only do large numbers of high school students work in this country, but also many tend to work long hours during the school week. Ruhm (1995) finds that 12th graders who reported working in 1991 worked an average of 28 hours per week. The work intensity reported by students in our nation is unique in the developed world, and some analysts suggest this may be a major explanation of why U.S. students perform below students in many other countries on several measures of academic achievement (Steinberg 1996).

Student employment has not escaped public attention. Public awareness and concern about the amount of time students spend on the job has led some states to consider laws that would reduce the amount of time students could work during the school year. For example, the Massachusetts state legislature has recently considered a bill that would reduce the maximum workweek from the currently allowed 45 hours per week to 28 hours. As reported in the *Boston Globe*, “...the bill will be presented ... in an economic climate marked by labor shortages and increased demand for 14-to-17-year-olds to fill entry-level jobs.”(Lewis 2000) In recent years Colorado and other states have considered similar measures that would strengthen their child labor laws.

Concerns about the amount and intensity of student employment stem from a conviction that work and school do not go well together. For example the Massachusetts Teachers Association backed the Massachusetts child labor bill because of a belief that "...long work hours affect the quality of students' school work" (Lewis 2000). Those who hold these beliefs often cite academic research to back their case. Indeed the research generally shows that working long hours per week is related to poorer academic outcomes. With a few exceptions, however, research on the school-year work-achievement relationship suffers from two weaknesses that hinder its usefulness for public policy decisions. First, most of the empirical work on the subject is based on samples of students who were in school in the early to mid-1980s. These students attended school prior to the wave of school reform movements that followed the 1983 publication of *A Nation at Risk* by the National Commission on Excellence in Education. The samples of students used in earlier studies also worked in an economy that was much different than the current "information age" economy. Changes in either schools or the types of jobs in which adolescents work could affect the work-achievement relationship. Second, virtually none of the studies use a clear source of exogenous variation in school-year work. As a result, estimates from past work should be viewed with caution since unobserved heterogeneity associated with the labor supply decisions of students could lead to biased estimates of the effect of school-year work on the academic achievement of students.

This paper advances the literature in two important ways. First, the results update our understanding of the work-achievement relationship for students who were in school in the 1990s. Estimates in the paper are based on a sample of students from the *National*

Education Longitudinal Survey of 1988 (NELS88) who were high school 12th graders in 1992, a full decade later than students who were used in most of the earlier studies. Second, a model is developed that both illustrates the potential problems with Ordinary Least Squares (OLS) estimation of the effects of work on achievement and provides a framework for instrumental variables estimation. Specifically, the effect of school-year work on academic achievement is identified in the model through exclusion restrictions involving state child labor laws.

The OLS estimates of the work-achievement relationship presented in the paper are similar to those found for earlier cohorts of students. However, the paper will show that OLS estimates that fail to account for the endogenous labor supply decisions of students are substantial underestimates of the negative impact that school-year work has on academic achievement. OLS estimates in this paper indicate that an exogenous decrease of ten hours per week in work intensity would lead to only a 0.03 standard deviation increase in 12th grade math scores. IV estimates, on the other hand, indicate that the same ten-hour-per-week decrease in work intensity would lead to a 0.20 standard deviation increase in math scores.

There are two possible explanations for the larger IV estimates in the paper. The first is that there may be considerable measurement error in student self-reported hours of work that attenuates OLS estimates. The second explanation is that the labor supply decisions of 12th graders are related to unobservable factors that are themselves related to academic achievement. The model in the paper will show that if achievement-related shocks are positively related to school-year labor supply, then OLS estimates will understate the negative effects of school-year work on achievement.

The format of the paper is as follows. The next section of the paper reviews past research on the work-achievement relationship for high school students. This is followed by the presentation of a model relating school achievement and high school work. Sections that discuss the data and the empirical strategy follow. After a presentation of the results and a sensitivity analysis, the paper closes with a summary and a discussion of the implications for policy.

II. Past Research

Those who believe that work and schooling do not go together often posit a zero sum model where time spent working is time taken away from activities such as homework that could enhance academic achievement (Coleman 1961). Others believe in a developmental model where working, particularly in the "right" kinds of jobs, may have a positive effect on academic achievement. The belief is that increased involvement in the world of work leads to the transmission of academically-related skills and knowledge, and that work also teaches and reinforces desirable adult-like traits such as responsibility and maturity that enhance academic outcomes (Holland and Andre 1987).

Past examinations generally support a negative student work-achievement relationship, but the results are far from clear cut. While interest in the working patterns of students and the effects of student work on outcomes dates at least to Dewey (1938), all but a few of the empirical studies use students who were in school in the 1980s.

In one of the earliest studies, D'Amico (1984) used data from the *National Longitudinal Survey of Youth* (NLSY) to show that increased work time was associated with decreased study time and free time for some racial/ethnic and gender groups of students. However, he found no effects of student employment on his only measure of academic achievement, class rank. He found that some groups of students who worked over 20 hours per week had higher drop out rates than comparable students who did not work, but he also found that those who those worked 1-20 hours per week had lower drop out rates than non-workers.

Greenberger and Steinberg’s (1986) study of students in Orange County, California found that teenage employment, especially when it is over 20 hours per week, is accompanied by negative academic, psychological and socioeconomic outcomes for students. In particular, they found that working more hours was associated with a lower grade point average. Given the nature of their sample, however, their findings may not be generalizeable to a larger population.

Using *High School and Beyond* (HSB) data on students who were 12th graders in 1982, Marsh (1991) found a primarily linear and negative relationship between school-year hours worked and a series of academic outcomes, including test scores and secondary school completion. However, since Marsh gives little information on how the categorical work experience variable in HSB was parameterized, it is not possible to determine whether the effects he found were substantively large or not.

Lillydahl (1990) was the first to address the potential endogenous nature of the labor supply decisions of students. She used a unique sample of 1987 students from The National Assessment of Economic Education Survey and Two Stage Least Squares (2SLS) estimation to examine the effects of working on grade point average, SAT scores, and a score on a standardized test of economic literacy. She found a nonlinear effect of hours of work on grade point average that was positive up to 13.5 hours per week and declining after that. There are, however, sample selection issues in this work, since only about a third of the 3,000 students in the survey were used in the regressions due to selection criteria and missing data. Also, Lillydahl apparently employed no exclusion restrictions, but identified the 2SLS estimates using the non-linearities of a first stage Tobit model predicting hours worked.

Eckstein and Wolpin (1998) used NLSY data to estimate an explicit sequential decision model of high school attendance and work. They find that working full-time results in a 0.135 point reduction in cumulative grade point average (on a four-point scale). They cite this is being a negative but "quantitatively small" effect of working on academic performance. However, since they present no information on the standard deviation of cumulative grade point average, it is impossible to independently assess the effect size of school-year employment on grades.

There are two papers that have used more recent student data. Schoenhals, Tienda, and Schneider (1998) use NELS88 data to show that "much of the adverse effect of youth employment on academic outcomes...found in previous research is attributable to preexisting differences among youth who elect to work at various intensities" (p. 723). Their method is to use the rich NELS88 data to control for variables that might affect both student work intensity and academic achievement as measured by grade point average. In their preferred model, they show that they can drive the negative relationship between hours-worked-per-week and grade point average in the 10^{th} grade to statistical insignificance by including variables to account for "preexisting differences between workers and nonworkers." In particular they include variables indicating the type of school attended in the 10^{th} grade (Catholic, private nonreligious, private religious, or public) and type of 10^{th} grade curriculum (general, academic, vocational). While these variables may control for "preexisting" differences, another interpretation is that these variables are both endogenous and correlated with work intensity—students choose type of school (or curriculum), hours of work, and level of academic achievement simultaneously. Under this alternative interpretation these estimates should be viewed with some caution.

Using NELS88 data and employing a simultaneous equations model, Warren, LePore, and Mare (2000) find "...no compelling evidence that high school employment has either long- or short-term effects on grades in academic courses." Thus, theirs is one of three studies that find no relationship between grades and work intensity using NELS88 data.[1] However, as Warren et al point out, their study has nothing to say about how work intensity affects students' achievement test scores. In the conclusion to their paper they state that "[their finding] does not mean that working too much has no effect on how much students learn or on other educational outcomes." Also, Warren et al use state unemployment rates to instrument for school-year work, a strategy that is questionable if 12th graders adjust their school effort in response to the labor market opportunities they face.

Taken as a whole these studies do not offer consistent lessons about the relationship between school-year work and academic achievement. The reasons for the inconsistencies are likely related to some combination of different data sets, different age students, different dependent variables, and different empirical methods across the studies. In general, however, among earlier cohorts of students, increased work hours per week is associated with lower academic performance. The few studies examining later

[1] The other studies are Eckstein and Wolpin (1998) and Schoenhals, Tienda, and Schneider (1998).

cohorts of students tend to find no systematic relationship between working during the school week and grade point average, even when the time spent at work is over twenty hours per week.

This paper examines the effects of school-year work on the academic achievement of 12th grade students. The focus is on academic achievement because it is ultimately what students learn that matters most, at least in the labor market, and grade point average may be less tightly aligned with actual learning than achievement test results.[2] The study uses students who were 12th graders in 1992, and it focuses on the scores on 12th grade mathematics and reading tests as the measures of academic achievement.[3] The departure in this paper is the development of a model where student work is endogenous and the use of exclusion restrictions involving state child labor laws to identify the parameters of interest.

III. The Model

In the model families with a high school age child gain utility from the academic achievement of the student, *A*, and from consumption, *C*.

$$U=U(A,C)$$

The utility the family receives from student academic achievement may be related to future family consumption under the assumptions that students with higher academic achievement have higher lifetime earnings that in some way are shared with family members, or it may come from the pleasure the family receives from a high achieving

[2] For example when taken together, the work of Bishop (1990) and Murnane, Willett, and Levy (1995) support this view.

[3] Results based on the other available tests in the NELS data show qualitatively similar results. Full results available from the author upon request.

child. Total family income results from income generated by the parents, y, and income from the school year work of the student. The student works T_w total hours during the school year of at a wage of w. The student wage, w, is assumed to be inversely related to state child labor laws, L. As child labor laws become more restrictive, the "effective" wage offer for students declines. For example, an exogenous tightening of child labor laws in a state might mean fewer hours that a student could work in "formal jobs" such as retail sales, with the time replaced in lower paying "adolescent jobs" such as babysitting or lawn work.

The budget constraint faced by the family is

$$p_A A + p_C C = y + w(L)T_w \qquad \textbf{(1M)}$$

In this model p_A and p_C are the price of achievement and consumer goods, and $T_w = T - T_a$, where T is the total amount of time available for the student to work outside of school and T_a is the amount of time that the student spends in activities such as homework that will increase academic achievement.

The production of academic achievement is related to observed ability, X, unobserved shocks that affect achievement, d, and the amount of time spent working in accordance with some production function, g:

$$A=g(X, d, T_w). \qquad \textbf{(2M)}$$

Examples of d in this model are unobserved changes in motivation, family structure, family dynamics, or student peer-group that could affect academic achievement. I assume that

$cov(A,X)>0$ and,

$cov(A,d)>0.$

Maximization of $U(A,C)$ subject to (1M) and (2M) yields a decision rule on how much time the student should spend in academic activities, T_a, versus time working for pay, T_w. Time spent on academic activities is given by

$T_a = g^{-1}(A, X, d)$, and I assume that

$T_a < T$.

A and T_w are functions of the following arguments,

$A=A^*(y, p_A, p_C, X, d, T_w)$

$T_w=T_w^*(y, p_A, p_C, X, d, L)$,

where all variables are defined as before.

Several lessons emerge from the maximization problem. The first relates student achievement to child labor laws. Satisfaction of the first and second order conditions of the maximization problem imply that an exogenous change in child labor laws affects student achievement in the following manner:

$$\frac{\partial A^*}{\partial L} = \left[\frac{\partial A^*}{\partial y}(T_w) + _{compensated} \frac{\partial A^*}{\partial p_A}\frac{\partial g^{-1}}{\partial A}\right] \bullet \frac{\partial w}{\partial L}$$

The expression in brackets is the income effect on achievement weighted by student work time, T_w, plus the compensated price effect on achievement weighted by the effect that a change in achievement level has on student academic time. The result in the brackets is then weighted by the effect that a change in laws has on the student wage.

Assuming $\frac{\partial g^{-1}}{\partial A} > 0$, the second term in brackets is negative. Since $T_w > 0$ and $\frac{\partial w}{\partial L} < 0$, the sign of $\frac{\partial A^*}{\partial L}$ depends on $\frac{\partial A^*}{\partial y}$. If A is an inferior good then $\frac{\partial A^*}{\partial L}$ is unambiguously positive. In this case, a toughening of child labor laws leads to greater achievement as students spend more time on homework and related activities and less time working as a result of the lower wage offer.

If A is a normal good, $\frac{\partial A}{\partial L}$ is positive if the compensated price effect outweighs the income effect. The likelihood of this is decreasing in the amount of time the student currently spends working and increasing in the sensitivity of time spent on academic activities to changes in achievement. Thus it is more likely that an increase in child labor laws will increase A for students who already spend less time in the school year working for pay or for students whose time spent on homework and other academic activities is more closely connected to academic achievement.

The model also illustrates issues of identification. The analytical goal is to estimate the effect of school-year work during the 12th grade year on 12th grade student achievement. A naïve approach would estimate a linear approximation to the production function:

$$A = bT_w + pX + (d+e) \qquad \textbf{(3)}$$

where e is a standard mean zero error term uncorrelated with the regressors. However, in the model T_a, and hence T_w, is partly determined by d, and so estimates of b in (3) will be biased. Under an assumption that observed ability X is negatively related to student work time T_w,[4] the sign of the bias depends on the relationship between T_w and d. If, we assume that unobserved shocks to achievement, d, are also negatively related to student work time, then the bias in b is of an undetermined direction. If unobserved shocks are positively related to student work, however, then the bias in b is unambiguously positive.

This describes a situation where conditional on observed ability, students who experience a positive shock to achievement (e.g., increased motivation or an exogenous shift in peers that increases achievement) tend to work more. Regardless of the direction of bias, however, it is clear that except in particular circumstances,[5] OLS estimation of (3) yields a biased estimate of b.

The final lesson of the model is that it suggests possible instruments for consistent estimation of b. Child labor laws, L, determine T_w, but they do not enter directly into the production function. Thus, state child labor laws are potential instruments for endogenous school-year work.

IV. Data

The data for the empirical work come from the first and second follow-up surveys of NELS88. NELS88 is a multistage national longitudinal study developed and administered by the National Center for Education Statistics (NCES) at the U.S. Department of Education. There were about 27,800 eighth grade students interviewed in the baseline survey in 1988. The first and second follow up surveys were conducted in 1990 and 1992 when the initial respondents were 10th and 12th graders, respectively. Of the 18,241 students who were interviewed in both the 10th and 12th grades, 9,252 students who meet the minimum sample selection criteria are used in the analysis. The sample includes all observations with non-missing values on both a 10th and a 12th grade math test, on a

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[4] As shown in the next section, the aggregate data suggest that this is the case.

[5] The OLS bias could be zero in the case where d is negatively related to T_w if positive and negative bias terms cancel each other out.

variable that measure 12th grade work experience, and on a school identifier variable.[6] All respondents in the sample were still enrolled in school as of the survey date in the spring of 1992, a source of potential sample selection bias that is examined later in the paper.[7]

NELS88 is well suited for the purposes of this study for several reasons. Besides containing information on 12th grade achievement and work experience, NELS88 data contain information on observed ability, *X*, in the form of measures of 10th grade math and reading scores.[8] Second, members of the NELS88 survey were 12th grade students in 1992, a decade later than students in most of the earlier studies that have examined the effects of student employment.

The primary weakness of NELS88 data has to do with the 12th grade work experience variable. Ideal data for estimating the effect of working on achievement would allow the construction of a continuous variable that captured the entire work history of individuals between the 10th and 12th grades. Instead, the NELS88 data set contains a categorical variable giving hours worked per week on "the current or most recent" job held in the 12th grade.[9] Hours per week in this variable are coded in ten categories that represent five-hour increments ranging from 1-5 hours per week through "worked more than 40 hours per week." Students who did not work at all in the school

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[6] Similar sample selection criteria—replacing 10th and 12th grade reading tests for math tests—were employed for the sample used to estimate models where 12th grade reading score is the dependent variable. This sample contains 9,242 individuals.

[7] See the Data Appendix for more information on sample construction.

[8] Marsh (1991) claims that controlling for prior achievement leads to a causal interpretation of OLS estimates of the effect of working on 12th grade achievement. This assertion neglects any time varying shocks that may be correlated with both 12th grade work and 12th grade achievement. For example, it may be that conditional on 10th grade achievement, it is students who become more motivated *post* 10th grade who tend to both work more and have higher 12th grade achievement.

[9] The study does not count hours worked on summer jobs.

are also identified with this variable. The manner in which this variable is used in the analysis is explained in the next section.

Table 1 gives summary statistics for the sample. The first column displays statistics for the entire sample. That column shows that about half of the entire sample is female and three-quarters are white. Almost thirty percent of the sample have self-reported family income below $25,000 per year, while about 17 percent report having at least one parent who lacks having at least a high school education. 85 percent of the sample attended a public school in the 12^{th} grade and half of the sample reported being in the college preparatory track, as opposed to the general curriculum or vocational education tracks. The mean scores on the 12^{th} grade math and reading tests are about 53.0, with sample standard deviations of 9.5 and 9.3.

The four right-most columns in the table divide students into relatively coarse hours-per-week work categories. These columns indicate that there are observable differences between students who work different amounts during the school year. Females are less likely to work zero or over twenty hours per week than male 12^{th} graders, and more likely to work one to twenty hours per week during the school year.

Several notable racial/ethnic differences in student labor supply appear in the data. First, white students are most concentrated in the one to ten hours per week category, the range where it may be the easiest to combine work and schooling. Meanwhile, a relatively high percentage of black students are found in the zero hours category, indicating that black students are either choosing not to work or are less successful in finding work during the school year. Hispanic students are disproportionately represented in the over twenty hours per week category. Working many hours per week is also associated with a greater probability of coming from a family whose income is below $25,000, of having at least one parent who lacks a high school diploma, and of attending a public school in the 12th grade. Meanwhile, those who work over 20 hours per week during the school year are less likely to be enrolled in a college preparatory academic track.

There are only small differences in mean test scores between those who worked zero hours per week in the 12th grade and those who worked up to ten hours per week. Meanwhile, the scores in both math and reading are lower for those who work between 11 and 20 hours per week and lower still for those who work over 20 hours per week. The initial evidence is that working more hours is negatively related to academic achievement.

Estimates in this paper will control for observable, exogenous differences among students when estimating the effect of school-year work on academic achievement. However, care has been taken to not include as controls variables that may be correlated with work intensity and are also likely to be endogenous. As a result, the primary results in the paper are based on models that do not control for school sector, the type of academic track, percent of students in the school on free or reduced lunch, and the urbanicity of the school attended.[10]

Since the analyses will utilize observations that may be missing on some, but not all of the control variables, the last rows of the table give information on missing data.[11]

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[10] As I note later, estimates from models that include potentially endogenous control variables are similar to the main results in the paper.

[11] The method described by Cohen and Cohen (1983) was used to resolve the problem of missing values. With this strategy missing values for a particular variable are replaced by a constant arbitrary value and an additional dichotomous variable is included to distinguish, and hold separate, the replacement

Less than one percent of the sample have missing racial/ethnic data, while 8.5 percent of the sample lacks information on family income. It does not appear that missing data is systematically linked to 12th grade hours of work during the school year.

V. Empirical Strategy

As explained in the previous section, the key explanatory variable, hours of work during the 12th grade school year, is categorical in the NELS88 data set. To approximate a continuous variable, I assign everyone the midpoint value of the respective categorical range.[12] Past work has generally made assumptions that allowed the school-year work variable to be entered linearly into the model, sometimes along with its square (Marsh 1991; McNeal 1997; Warren, LePore, and Mare 2000). An examination of the relationship between hours worked per week and 12th grade math scores in the NELS88 data indicates the potential problems with a linear parameterization in this case.

Figure 1 graphs the unconditional relationship between hours worked per week in each of the ten categories and mean 12th grade math scores. Several facts emerge from the graph.[13] First, unlike some of the studies that use students who were in school in the 1980s, there does not appear to be an advantage for students who work 1-5 hours per week relative to students who do not work at all. Second, unlike studies of earlier cohorts of students that found non-linear negative effects after 20 hours of work per week, the

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values. As is reported in a later section, qualitatively similar results are obtained if only data containing no missing values are used.

[12] This method was used by Warren, LePore, and Mare (2000) in their paper on the effects of school year work on high school grades. I examined the possibility of assigning everyone the mean value of the relevant categorical range estimated from Current Population Survey data. The results in this paper are similar using this method.

[13] The graph in Figure 1 is constructed using all 10,445 respondents with non-missing information on the hours-worked-per-week variable and the 12th grade math test. A graph based on the 9,252 members of the analytic sample is very similar.

unconditional work-achievement relationship in Figure 1 appears to be quite linear in the range from 1 through about 33 hours per week. Finally, there appear to be no additional negative effects after 33 hours per week of work.

The relationship depicted in Figure 1 can be captured parsimoniously by a single variable that can be entered linearly if the following constraints are adopted:

1. the effect on achievement of working under five hours is constrained to be the same as the effect of working zero hours per week;

2. the effect on achievement of working between 1 and 33 hours per week is linear; and

3. the effect on achievement of working over 33 hours per week is constrained to be the same as working 33 hours per week.

Later in the paper specifications where these constraints are relaxed are discussed. In no case do any of the constraints appear to unfounded.[14] The empirical work involves consistent estimation of b in the following equation:

$$A_{ij} = a_0 + bT_{w_{ij}} + a_1 X_i + a_2 W_i + a_3 S_j + e_{ij}, \qquad e_{ij} = n_{ij} + d_i \qquad \textbf{(1)}$$

where i indexes student, j indexes state of residence during the 12th grade year, and T_w is the number of hours worked per week during the 12th grade school year. W is a vector of student background indicators for gender, race/ethnicity, occupation and education level of mother and father, category of family income, indicators for missing information, and in some specifications, residence in one of nine census regions.[15] S is a vector capturing state level factors that might influence academic achievement. Academic achievement, A,

[14] Furthermore, the basic results of this paper remain the same if the school-year work variable is simply entered linearly as has been done with past work.

is measured with 12th grade test scores and observed ability X is measured with 10th grade test scores.

The correlation between T_w and d necessitates IV estimation of (1) based on:

$$T_{w_{ij}} = a'_o + a'_1 W_{ij} + a'_2 X_i + a'_3 S_j + g_1 Laws_j + u_{ij} \quad \textbf{(2)}$$

where *Laws* is a vector of state child labor laws.

The model suggests the exclusion of *Laws* from (1). Explicitly, however, the validity of excluding the state child labor laws rests on the assumption that conditional on state level factors such as average per student expenditure, percentage of adults with at least 12 years of schooling, and state per capita income, the laws only affect academic achievement through their limitations on student work in "formal jobs" during the school year.

The primary child labor law instrument set contains one continuous and six binary variables that affect adolescents through age 17 differently in different states.[16] These variables were constructed using information from two sources: a survey of state departments of labor conducted by the Child Labor Coalition of the National Consumer's League and a document compiled by the Wage and Hour Division of the U.S. Department of Labor. The Child Labor Coalition survey gathered information on child labor laws and child labor law enforcement in each state, while the U.S. Department of Labor document summarizes selected state child labor standards that affected minors under 18 in 1992. Using these two documents I was able to construct seven state child labor law variables that contain non-missing information across the 50 states and the

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[15] Family income in NELS88 is categorized into 15 categories.

District of Columbia.[17] Taken together, these variables provide a measure of the child labor "regulatory climate" and the child labor activity in the state.

The continuous variable is the total number of dollars collected in the state in civil money penalties during 1992 divided by the state population. The binary variables, all relative to 1992, include:

- an indicator of whether or not the state had regulations that placed limits on adolescent work after 10:00 p.m. on a night before a school day,
- an indicator of whether or not the state labor department was required to publicize the names of employers who violate child labor laws,
- an indicator of whether or not the state imposed criminal penalties for child labor violations,
- an indicator of whether the state limited the maximum number of hours that adolescent minors could work during a school week, and
- an indicator for whether the state required work permits for minors employed in agriculture-related jobs and,
- an indicator for whether the state required work permits for minors employed in non-agriculture-related jobs.

Table 2 shows the distribution of the laws across the states and the number of observations in the analytic sample affected by each law. In the first row, there were 13 states that collected some civil money penalties for child labor law violations in 1992.

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[16] Specifically, laws that affect workers age 16-17 are the relevant laws. Ninety percent of the sample was still 17 years of age as of September 1st of their 12th grade year, and thus, directly affected by the child labor laws.

Among the states that collected any civil money penalties, the mean amount collected was $643 per 100,000 in population. The maximum amount collected was $1,319 per 100,000 in Illinois. Every state except for Tennessee was affected by at least one of the laws, and no state was affected by all of the laws.

The primary identifying assumption associated with the state child labor law instruments is that they are only associated with 12th grade academic achievement through the constraints they provide on the school-year labor supply of 16-17 year-old students in formal jobs. This raises questions regarding the processes that could generate variation in these laws across states. One plausible explanation is that states tend to tighten the child labor regulatory climate in the presence of heightened public awareness of issues related to working minor children. Public concern may escalate following either the publication of work-related accidents or homicides that involve adolescents or the reporting of negative child labor statistics.

For example, a 1993 Houston Chronicle series, "Kids on the Job; The Price of a Paying Job," is given substantial credit for the passage of tougher child labor legislation in Texas in the early 1990s. That series opened with a recounting of four teenage girls who were murdered two years earlier while working a late-night, school-night shift in a yogurt shop (Stancill 1993b). The series went on to report that there had been 13,328 injuries and 15 deaths in Texas among workers ages 14-18 in the four years just prior to the report. A bill to tighten Texas child labor laws was introduced two months after the newspaper series appeared. The sponsoring state senator cited the series in introducing

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[17] The Child Labor Coalition survey had missing information on seven states. Follow up phone calls to the state departments of labor in these states allowed me to fill in missing information on the variables that were constructed from that survey.

the bill saying that, “The press coverage this issue has gotten at the Houston Chronicle and other news outlets in the state has put this issue on the front burner” (Stancill 1993a). The bill was passed into law two months later.

In another example, the child labor regulatory environment in Mississippi was toughened considerably after a 16 year-old worker fell to his death while working on a roof in violation of federal child labor laws. This incident is credited with prompting the state to add six labor law violation investigators (up from only one investigator previous to the incident), as well as contributing to a push to establish a department of labor in that state, where there was none before (Adkins 2000).

Colorado provides another example. As reported in a Denver, Colorado newspaper, “…legislators, bureaucrats, industry representatives and employers…started taking a fresh look at state laws after two teen-agers were slain in February in a late-night shooting at a Subway Sandwich Shop near Columbine High School. One of the victims was a 15-year-old working alone…” (Simons 2000).

That stricter state child labor laws may often be passed in response to high profile work-related adolescent accidents and homicides suggests that the variation in these laws across states is largely random. To the extent that this is the case, state child labor laws satisfy the first condition required of valid instrumental variables: they would be unrelated to academic achievement except through their effects on adolescent labor supply. In a later section I will present results from sensitivity analyses that explore potential violations of this assumption. Meanwhile, evidence on whether the laws are, in fact, related to student labor supply—the second requirement for valid instruments—is provided in Table 3.

Table 3 shows estimates on the first stage relationship between the child labor law instrument set and the 12th grade school-year labor supply of students.[18] For comparison purposes, the first and third columns provide baseline results from a regression of the school-year labor supply variable on the exogenous control variables, including the 10th grade math score (first column) and the 10th grade reading score (third column). Estimates in the second and fourth columns are from models that contain the baseline variables plus the state child labor laws. The hypothesis that the coefficients on the child labor law instruments are jointly zero can easily be rejected in either the math or the reading test models, providing evidence that the laws are related to 12th grade labor supply.

The cautious reader may be unconvinced that state child labor laws are unrelated to unobserved state-level factors that may also affect student academic achievement, a correlation that would violate the necessary IV conditions. To address this concern, I first note that if the child labor laws are working as assumed, then they should be less related to the labor supply of 12th grade students who turn 18 earlier rather than later in the school year. This suggests the possible use of age by labor-law interactions as a second instrument set. The advantage of this instrument set is that it allows for the inclusion of state dummies to directly control for state fixed effects.[19] As a simple test of whether the laws differentially affect labor supply by age, the school-year work variable was regressed on the child labor law instruments and the variables in W using the 2,583 students who had all turned 18 by December 31st, 1991. In this regression the null

[18] All regressions in the paper correct the estimated standard errors to account for the fact that students in the NELS88 data are clustered in schools.

[19] I thank an anonymous reviewer and the editor for this suggestion.

hypothesis that instrument coefficients are jointly zero is not rejected (p=0.63). Alternatively, in a similar regression using the 6,669 younger students who had not turned 18 by 1992, the same null hypothesis is rejected (p=0.0000). Thus, the evidence is that the child labor law instruments are more strongly correlated with the labor supply of younger students than of older students.

Based on this evidence estimates from a second set of instruments formed from the interaction of each of the seven child labor laws with student age are presented. In addition to the variables in *W* and 10^{th} grade achievement, these additional IV estimates control for the linear effect of age along with state fixed effects. Estimates in this model are based on the sample of 7,973 students who had not yet turned 18 at the beginning of the 12^{th} grade school year. The reason for using this more refined sample is that, given the sample, age is endogenous. Age endogeneity in the sample arises from the fact that drop out decisions between the 10^{th} and 12^{th} grade are related to age. Students who are age 16 or older at the beginning of the 10^{th} grade will be 18 years of age at the beginning of the 12^{th} grade. These older, "behind grade level" students drop out between the 10^{th} and 12^{th} grades at substantially higher rates than do students who are younger and "on grade level." Among students who began the 10^{th} grade, 18 percent of those who will be 18 or older two years later drop out by the 12^{th} grade. Meanwhile, only 2 and 4 percent, respectively, of the students who will be 16 or 17 at the beginning of 12^{th} grade drop out between the 10^{th} and 12^{th} grades. To the extent that age-endogeneity in the sample is a problem in models that use age-law interactions as instruments, it is much less so in a sample of students who are "on grade level" than in a sample that includes over-age students.

VI. Results

A. OLS and IV Estimates

Table 4 gives OLS and IV estimates of b from equation (1) where different dependent variables (math and reading), specifications, and samples are used. The first three columns in the top row of Panel A give OLS estimates of the relationship between 12th grade labor supply and 12th grade math scores. The estimate in the first column is based on the entire analytic sample, and the model controls for state and regional factors that may influence academic achievement by including in the regression mean state expenditure per student, state per capita income, percent of adults in the state with at least a high school diploma, and census region of the country where the student attended 12th grade. For purposes of comparison, the estimate in the second column is based on the same specification, but uses the restricted sample of on-grade-level students who had not turned 18 when the 12th grade school year started. The third column uses the restricted sample, adds age of the student as an additional regressor, and controls for state level factors through the inclusion of a complete set of state dummies. All three estimates, along with the parallel OLS estimates in Panel B that use the 12th grade reading score as the dependent variable, indicate that every additional ten hours worked per week during the school year is associated with a 0.3 point reduction in 12th grade achievement test scores.[20]

These results are generally comparable with past estimates based on students who were in school a decade earlier. To examine whether specific regularities found for earlier

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[20] In all of the OLS and IV models, females have lower mean math scores and higher mean reading scores than males, blacks score lower than whites, and Hispanics have lower reading scores than do whites. Complete regression results from the models in Table 4. Available from the author upon request.

students are present in these data, the constraints embodied in the parameterization of the work intensity variable are relaxed. In a less constrained model based on the full sample and the regressors used in the first column, the effect of working on achievement is allowed to be different for each of the ten work intensity categories in the NELS88 work experience variable. In this model, using the math test score as the dependent variable, the null hypothesis that working 1-5 hours per week has the same effect as working zero hours cannot be rejected (p-value = 0.96). We also fail to reject the null that working 21-25 hours per week has the same effect on achievement as working 16-20 hours per week (p-value = 0.29). Thus, there is no compelling evidence of the non-linearities reported in previous research based on students who were in high school in the early 1980s.

While the OLS results are consistent across models and samples and show a statistically significant negative relationship between working during the school year and academic achievement, the estimated effects are substantively very small. For example, given the standard deviation of 12^{th} grade math scores in the sample (9.5), decreasing student work during the school year by ten hours per week would increase 12^{th} grade math scores by only slightly more than 0.03 of a standard deviation based on the OLS estimates. Effect sizes this small offer little support for policies that would further constrain the school-year work habits of adolescents.

As discussed earlier, however, OLS estimates of the effect of school year work on achievement may be biased. OLS estimates would underestimate the negative effect of working on achievement if, conditional on observed ability, students who experience positive achievement-related shocks tend to work more. OLS estimates would also understate a negative work-achievement relationship if school year work is measured with error.[21]

IV estimates that account for the endogeneity or mis-measurement of student labor supply are in the right-most three columns of the table. Estimates using the full analytic sample and the primary child labor law instrument set are in the first IV column. For both math and reading, the estimates are substantially more negative than the OLS estimates, and they are both well estimated. The IV estimates in this column indicate that a decrease of 10 hours per week in the amount of school-year work would lead to a 1.9 point increase in mean math scores and a 2.7 point increase in mean reading scores.[22]

Primarily for comparison purposes, the second IV column of the table presents results based on the same instrument set and specification, but in this case the restricted sample is used to estimate the parameters of the model. The estimated effects of school-year work on academic achievement are essentially the same across the two samples when math is the dependent variable, and the restricted sample estimates are somewhat smaller when reading is the dependent variable.

Estimates in the third IV column are formed over the restricted sample. The model in this column uses the student age by child-labor-law interactions as instruments, and it

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[21] While there is no direct evidence of the degree of error in student self-reported hours of work, Mellow and Sider (1983) use matched employee survey and employer administrative files to show that employees' self-reported hours of work exceed the administrative records by about 4 percent. Mellow and Sider present no information on the covariance between self-reported and administrative-record hours of work.

[22] In analyses not presented here, models were estimated that included controls for the urbanicity of the 12th grade school, the sector (public or private) of the school, and the county unemployment rate, all potentially endogenous. Results based on these models (available from the author upon request) are very similar to the main OLS and IV estimates in the paper.

includes a set of 50 state indicator dummy variables to control for state fixed effects.[23] The estimated effect of school-year work on math achievement remains unchanged from the other two IV estimates in Panel A, but it is less well estimated. When reading score is the dependent variable, the point estimate is substantially smaller in the state fixed effects model than in the other IV models, and it is poorly estimated. Even though this point estimate is smaller than the other IV estimates, it is still substantially more negative than the comparable OLS estimate.[24]

Both OLS and IV estimates indicate that school-year work negatively impacts academic achievement in the 12th grade. Unlike the OLS estimate, however, IV estimates in this paper are large enough to suggest a potential role for policy interventions. Policies that could substantially reduce the amount of time adolescents spend working during the school year could be expected to substantially raise academic achievement, all else remaining equal. Based on the math estimates, for example, an exogenous decrease in hours worked per week of ten hours would be expected to increase average math achievement scores by two tenths of a standard deviation (1.9/9.5), or about one-quarter of the black-white math test score gap in NELS88.

B. Sensitivity Analysis

In this section threats to the identifying assumptions underlying the IV estimates are examined. The primary assumption is that state child labor laws are related to 12th grade

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[23] A dummy variable indicating school attendance in the District of Columbia is the excluded variable in the regression.

[24] When the age by labor-law IV models are fit over the entire sample, the results are similar to the OLS results. It is straightforward to show that there are plausible conditions under which b_{IV} is biased upward if $\sigma_{age,\,\varepsilon} \neq 0$.

academic achievement only because of the constraints they place on the school-year labor supply of 12th graders in “formal jobs” rather than through some other mechanism. A fundamental concern for this identification strategy is that states may pass more or less restrictive laws based on political economy decisions. For example, some states may be willing to pass more restrictive child labor laws because there is a low demand for teen labor in that state. In this case the causal direction between school-year work and child labor laws is running in a direction that undermines the validity of state child labor laws as instruments. From the standpoint of the validity of the child labor law instruments in this paper, other equally problematic political economy scenarios explaining state variation in child labor laws may also be likely.[25] However, in all cases one would expect to see a systematic relationship between the variation in state child labor laws and the labor supply of all young workers. This includes not only 16-17 year-olds directly affected by the laws, but also 18-20 year-old workers.

To examine this possibility a sample of 18-20 year-olds from the 1992 merged outgoing rotation files of the Current Population Survey (CPS) data was employed. The usual hours worked per week in this sample was regressed on the child labor law instruments, along with controls for gender, race, region of the country, and state unemployment rate. The null hypothesis that the coefficients on the seven child labor law instruments are jointly zero in this regression cannot be rejected (p=0.60). Thus, there is no support in CPS data for a systematic political economy explanation of the passage of state child labor laws.

[25] Of course, these arguments are not problematic for the IV models that include state fixed effects.

Since the IV model is overidentified, a test of overidentifying restrictions can be conducted. Following Davidson and MacKinnon (1993) the residuals from the IV models were regressed on the respective instruments. In the math achievement models, p-values on the chi-squared statistic testing the null hypothesis that the coefficients on the instruments are jointly zero were 0.76 and 0.41 for, respectively, the primary IV set and the interaction IV set. Similar p-values resulted in models where reading achievement was used as the dependent variable.[26] Thus, in all cases we fail to reject the null hypothesis that the instruments are unrelated to the residuals from the IV model.

Since selection into the analytic sample is predicated on still being in school in the 12^{th} grade, an additional concern is that state child labor laws may be related to the probability of dropping out. For example, assume that dropouts are drawn from the left-hand tail of the achievement distribution and also assume that strict child labor laws reduce the dropout rate. Under these assumptions the sample of students who remain in school through the 12^{th} grade in states with stricter child labor laws will have lower average academic ability than 12^{th} grade students in states with less strict laws. The opposite sample selection problem would occur if stricter child labor laws caused dropout rates to be higher. To examine this potential problem the probability of dropping out between the 10^{th} and the 12^{th} grades was regressed on W, the 10^{th} grade achievement, and the child labor laws using logit regression. We fail to reject the hypothesis that the coefficients on the child labor laws are jointly zero in that regression ($p = 0.29$),

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[26] This standard overidentification test fails to take account of the fact that the instruments are at the state level, while the analysis is at the student level. Hoxby and Paserman (1998), demonstrate that failure to account for the state-level clustering of students in NELS88 will result in a larger test statistic, and hence, a greater chance of rejecting the null hypothesis that the instruments are jointly orthogonal to the IV residuals.

concluding that there is no evidence that the child labor laws are systematically related to the probability of dropping out.[27]

Finally, no solid evidence is found for either gender-labor supply or racial/ethnic-labor supply interactions. The only potential interaction that appeared was for black students in the OLS model. However, the black-labor supply interaction was statistically insignificant in the corresponding IV model.

VII. Summary

Like studies based on earlier cohorts of students, the OLS estimate in this paper finds a negative relationship between working more hours during the school year and academic outcomes. More important, however, the effect sizes associated with the OLS estimates in this paper are so small as be substantively uninteresting for policy formulation. Unfortunately, comparing effect sizes in this paper to earlier work is often impossible due to the lack of information in the earlier studies, but there are no earlier studies that make a strong case for the need for policy intervention.

The focus of this paper, however, is on the instrumental variables estimates, and these estimates present a quite different story. IV estimates that account for the endogeneity of the school-year labor supply decisions of high school students find a much larger negative effect of working on 12th grade math scores than the OLS estimates. The IV estimates are large enough to be of policy interest, and they suggest that if a primary policy objective is to maximize 12th grade academic achievement, then states should possibly consider more restrictive child labor laws for 16-17 year-olds. For

[27] When the 10th grade work variable is included in the regression we also fail to reject a test of

example, the estimates in the math model indicate that if more restrictive child labor laws reduced the average number of hours worked per week during the school year by ten hours per week, mean 12th grade math scores could be expected to rise by about 2.0 points, or about 0.2 of a standard deviation.

This impact can be put in economic terms. Using estimates of returns to test scores of 20 percent for each standard deviation increase, a discount rate of 3 percent, and a retirement age of 65, a 17 year-old student who experienced an exogenous decrease in school-year work of 10 hours per week in the 12th grade could expect an increase in lifetime earnings that has a present discounted value of almost $30,000.[28] Meanwhile, at a wage of $10 per hour, the decrease in hours worked per week would translate into lost school-year wages for the 17 year-old of $3,600. To the extent that higher academic achievement leads to better health, decreased criminal activity, or other benefits not directly measured by increased earnings, this calculation underestimates the private and social benefits of reducing the school year work of students. Meanwhile, the costs may be underestimated if, for example, lost work experience during the 12th grade year reduces later wages.

In a world where adolescents make well-informed calculations of the present discounted value of high school academic achievement, public policies that interfere with the free workings of the labor market may be counterproductive. On the other hand, if 16-17 year-old students are myopic and tend to over-value present consumption, the results

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whether the child labor law coefficients are jointly zero (p = 0.74).

[28] Neal and Johnson (1996) estimate a 20 percent return for each standard deviation increase in test scores, while Krueger (2000) suggests a more modest 8 percent return. However, the calculations here do not include potential future increases in the productivity growth rate that could increase income and hence the benefits associated with increased test scores.

in this paper suggest that it may be beneficial to consider policies that would constrain the amount of time students can spend working during the school year.

Table 1. Summary statistics for the analytic sample of 9,252 NELS88 members with both 10th grade math scores, 12th grade math scores, and 12th grade labor supply information.[a]

	Entire sample	Sample by 12th Grade Hours of Work Per Week Categories			
		Zero hours	1-10 hours per week	11-20 hours per week	Over 20 hours per week
Percent of sample	—	22.7	20.4	33.3	23.6
Percent …					
female	51.4	47.6	55.7	56.5	44.2
white	74.5	71.8	78.5	75.0	71.1
black	7.1	9.3	5.2	6.3	7.9
Hispanic	9.3	9.1	6.9	9.0	12.0
other race/ethnicity	8.9	9.3	9.1	9.0	8.1
with family income below $25,000	29.6	27.0	25.4	28.5	37.3
with at least one parent who lacks a high school diploma	17.3	15.4	12.0	17.6	23.7
who attend public school	85.1	76.1	81.0	89.8	90.8
who are in a college preparatory academic track	49.7	57.5	55.9	49.9	36.5
Mean and standard deviation of…					
12th grade math score	53.0	55.1	54.7	52.9	50.0
	(9.5)	(9.7)	(9.4)	(9.0)	(9.0)
12th grade reading score	52.9	54.3	54.6	52.9	50.0
	(9.3)	(9.4)	(9.1)	(8.8)	(9.1)
Percent missing information on…					
race/ethnicity indicator	0.65	0.62	0.27	0.68	0.96
mother's education level	0.63	0.52	0.53	0.72	0.69
father's education level	0.96	1.01	0.53	1.27	0.82
mother's occupation	0.66	0.52	0.48	0.72	0.87
father's occupation	2.78	3.14	2.12	2.63	3.20
family income	8.52	9.46	8.54	8.45	7.68

a. Reading score statistics are based on the 9,242 sample members who had both 10th and 12th grade reading scores and 12th grade labor supply information.

Figure 1. Mean 12th grade math test scores graphed against the ten hours-worked-per-week categories.

Table 2. Distribution of states and observations across the three child labor law instruments.

Law	Number of States Affected	States Affected	Number of Observations Potentially Affected
Dollars collected in the state in civil money penalties	13	*States with non-zero collections*: Arizona, California, Illinois, Indiana, Kentucky, Maine, North Carolina, New Jersey, New York, Oklahoma, Oregon, Virginia, Wisconsin	3,580
Limits on student work after 10 p.m. on a school night	5	Alabama, California, District of Columbia, Massachusetts, Maine	1,219
State department of labor publicizes employers who violate child labor laws	7	Iowa, Maine, New Jersey, New York, Oregon, Pennsylvania, South Carolina	1,891
Imposition of criminal penalties for child labor law violations	37	*All states except*: Arizona, Georgia, Hawaii, Indiana, Kentucky, Maine, North Carolina, New Mexico, Nevada, New York, Tennessee, Wisconsin, West Virginia, Wyoming	6,896
40 hour limit on the number of work hours per week while school is in session	13	California, Connecticut, Delaware, Florida, Indiana, Kentucky, Maryland, Maine, Michigan, New Hampshire, New York, Washington, Wisconsin	3,331
Required work permits for minors in agriculture-related jobs	17	Alaska, Arkansas, California, Connecticut, Hawaii, Iowa, Michigan, Missouri, North Dakota, New Jersey, New Mexico, Nevada, New York, Ohio, Oklahoma, Pennsylvania, Washington	4,220
Required work permits for minors in non-agriculture-related jobs	36	*All states except*: Arizona, Colorado, District of Columbia, Florida, Idaho, Kentucky, Minnesota, Missouri, Montana, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont	7,072

Table 3. First-stage regression of 12th grade school-year hours worked per week on the primary control variables plus the instruments (standard errors in parentheses).[a, b]

	Baseline Math Model	Baseline Math Model plus instruments	Baseline Reading Model	Baseline Reading Model plus instruments
Control variables				
female	–1.25**	–1.24**	–1.04**	–1.03**
	(0.20)	(0.20)	(0.20)	(0.20)
black	–1.16**	–1.07**	–0.92**	–0.84~
	(0.47)	(0.47)	(0.47)	(0.47)
Hispanic	–0.05	0.03	0.03	0.07
	(0.42)	(0.43)	(0.43)	(0.43)
other	–0.09	–0.21	–0.22	–0.33
	(0.35)	(0.35)	(0.36)	(0.35)
10th grade math or reading test score	–0.13**	–0.13**	–0.11**	–0.10**
	(0.01)	(0.01)	(0.01)	(0.01)
Instruments				
amount in civil money penalties		182**		176**
		(47)		(47)
laws restricting work after 10 p.m.		–0.70		–0.83
		(0.63)		(0.63)
laws requiring public notice of child labor violations		–0.94		–0.94
		(0.57)		(0.57)
criminal penalties allowed for child labor violations		–0.34		–0.36
		(0.38)		(0.38)
laws limiting maximum hours of work per week during school year		–0.60~		–0.65~
		(0.36)		(0.36)
work permit required for agriculture-related work		2.50**		2.52**
		(0.46)		(0.47)
work permit required for non-agriculture related work		–1.44**		–1.42**
		(0.42)		(0.43)
R^2	0.091	0.096	0.088	0.093
N	9,252	9,252	9,242	9,242
Test of H_0: the instruments are jointly zero	—	F = 5.69	—	F = 5.90
		p = 0.0000		p = 0.0000

a. All regressions control for parental education and occupation, family income, census region, state expenditure per student, state per capita income, percent of adults in the state with at least a high school education, and for whether or not there is missing information on race/ethnicity, parental education, parental occupation, or family income.

b. ** = significant at the 0.01 α-level, * = significant at the 0.05 α-level, ~ = significant at the 0.10 α-level.

Table 4. OLS and IV estimates of the effect of working during the school year on four different measures of 12th grade academic achievement

(standard errors in parentheses).[a, b]

	OLS			IV		
	Full sample	Restricted sample	Restricted sample	Full sample	Restricted sample	Restricted sample
Panel A: Dependent variable: 12th grade math scores						
Hours of work per week in 12th grade	–0.03**	–0.03**	–0.03**	–0.19**	–0.20**	–0.20
	(0.004)	(0.005)	(0.005)	(0.07)	(0.07)	(0.15)
Age	—	—	Yes	—	—	Yes
State-level controls	See below[c]	See below[c]	State dummies	See below[c]	See below[c]	State dummies
Instruments	—	—	—	Child labor laws	Child labor laws	Law*age interactions
R^2	0.85	0.84	0.84	0.83	0.81	0.81
N	9,252	7,973	7,973	9,252	7,973	7,973
Panel B: Dependent variable: 12th grade reading scores						
Hours of work per week in 12th grade	–0.03**	–0.03**	–0.03**	–0.27*	–0.22*	–0.12
	(0.007)	(0.007)	(0.005)	(0.11)	(0.11)	(0.21)
Age	—	—	Yes	—	—	Yes
State-level controls	See below[c]	See below[c]	State dummies	See below[c]	See below[c]	State dummies
Instruments	—	—	—	Child labor laws	Child labor laws	Law*age interactions
R^2	0.66	0.64	0.64	0.61	0.61	0.64
N	9,242	7,957	7,957	9,242	7,957	7,957

a. All regressions control for gender, race/ethnicity, 10th grade achievement, mother and father's education level and occupation, family income, and for whether or not there is missing information on race/ethnicity, parental education, parental occupation, or family income.

b. ** = significant at the 0.01 α-level, * = significant at the 0.05 α-level, ~ = significant at the 0.10 α-level.

c. State average expenditure per pupil, state per capita income, percent of adults with at least a high school diploma, and indicator for one of nine census regions.

Data Appendix

	Number of observations omitted	Resulting Sample Size
Total observations in NELS:88		27,805
Not in sample in the second follow up	6,202	21,603
Dropped out before the 12th grade	2,879	18,724
12th grade status unknown	1,072	17,652
12th grade math test score missing	4,414	13,238
12th grade work history missing	2,879	10,359
School id missing	647	9,712
Gender missing	62	9,650
Unable to identify state	1	9,649
10th grade math test score missing	397	9,252

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