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Using State Child Labor Laws to Identify the Causal Effect of Youth Employment on Deviant Behavior and Academic Achievement

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Abstract

On the basis of prior research findings that employed youth, and especially intensively employed youth, have higher rates of delinquent behavior and lower academic achievement, scholars have called for limits on the maximum number of hours per week that teenagers are allowed to work. We use the National Longitudinal Survey of Youth 1997 to assess the claim that employment and work hours are causally related to adolescent problem behavior. We utilize a change model with age-graded child labor laws governing the number of hours per week allowed during the school year as instrumental variables. We find that these work laws lead to additional number of hours worked by youth, which then lead to increased high school dropout but decreased delinquency. Although counterintuitive, this result is consistent with existing evidence about the effect of employment on crime for adults and the impact of dropout on youth crime.

Keywords

Youth employment; Crime and deviance; School performance; Longitudinal data; Instrumental variables

Introduction

The classroom and the workplace are two of the most important developmental contexts for youth in American society. Until well into the early 20th century, adolescents were likely to mix school attendance and employment on the family farm, inside the home, or in factories, with employment generally taking up at least as much time as their schooling (Bremner et al. 1971; Kett 1977, 1978). Even children whose primary “occupation” was attending school were likely to have jobs in close proximity to the home, for example, running errands, selling papers, shining shoes, or other forms of informal work. Although the relative importance of education in the lives of America’s youth grew with compulsory attendance laws, employment and schooling were always closely interwoven and have historically competed for the time and energy of young people. It is interesting to see too that while concern was expressed about the working conditions of children, there was little doubt that, like schooling, employment was good for them.

Beginning in the mid-1800s, however, criticism grew concerning the practice of allowing adolescents to be gainfully employed while still attending school. Such criticism was part of a growing “sentimentalization of childhood” in which adolescence was increasingly viewed as a critical period for healthy development (Kett 1978). Adolescence, it was argued by psychologists, social workers, and reformers, was a time when youth should be spared the *sturm and drang* of adult responsibilities so that they could spend time exercising, reading, contemplating the future, and exploring their talents and interests. It was widely thought that youth should generally be sequestered from adults and adult-like activities such as employment. Child reformers at the time were concerned with “precocity”—the premature assumption of adult roles and responsibilities. One of the leading critics of the precocity of youth and the need for adolescence to be a distinctive time for reflection was the psychologist G. Stanley Hall:

[O]ur young people leap rather than grow into maturity.... [O]ur vast and complex business organization...absorbs ever more and earlier the best talent and muscle of youth ... but we are progressively forgetting that for the complete apprenticeship to life, youth needs repose, leisure, art, legends, romance, idealization, and in a word humanism, if it is to enter the kingdom of man well equipped for man’s highest work in the world. (Hall 1904, pp. xvi–xvii)

Doubts about the benefits of adolescent employment were not the idle speculation of academics and child reformers.¹ The U.S. Congress passed laws restricting child labor in 1918 and again in 1922, but this legislation was both times struck down as unconstitutional by the U.S. Supreme Court². In 1938, Congress finally passed the Fair Labor Standards Act (FLSA) which was ultimately upheld by the Supreme Court in 1941 in *United States vs. Darby Lumber Company* (312 U.S. 100). The FLSA established a minimum age of 16 years for non-agricultural employment but did allow 14 and 15 year olds to work so long as their employment did not interfere with their schooling and was not under conditions that proved detrimental to their health and wellbeing. Motivated primarily by the pressure of labor unions to remove a source of cheap and competitive labor as well as a genuine concern for

¹By 1913 all but nine states had child labor laws which fixed age 14 as the minimum age for factory work, while a majority of states had 14 years as the minimum age for employment in stores and other workplaces.

²Powerless to affect labor practices within states, Congress passed the Keating-Owen Act in 1916 which prohibited the interstate transportation of goods produced by factories or shops that employed children under 14 years of age, or children under the age of 16 who worked at night or for more than 8 h per day. The Supreme Court struck down this law 2 years later in 1918 in *Hammer vs. Dagenhart* (247 U.S. 251). In 1919, Congress passed the Child Labor Tax Law which placed a 10% excise tax on the net profits of factories and mines employing children. The Supreme Court struck down this law as unconstitutional in 1922 in *Bailey vs. Drexel Furniture Company* (259 U.S. 20). In 1924, Congress then passed a constitutional amendment giving the federal government the power to regulate child labor, but too few states ratified the proposed amendment and it consequently did not take effect.

the sometimes horrendous working conditions of children, federal child labor laws began to restrict the number of hours that school-aged youth could work.

Efforts to further restrict adolescent access to work characterized both federal and state legislation until the 1970s. In 1974, however, the Panel on Youth of the President's Science Advisory Committee (Coleman et al. 1974) signaled a change in thinking about adolescent employment. This and other blue-ribbon commissions such as the National Commission on the Reform of Secondary Education (1973), the National Panel on High School and Adolescent Education (1976), the Work-Education Consortium of the National Manpower Institute (1978), the Carnegie Council on Policy Studies in Higher Education (1979), and the National Commission on Youth (1980) were highly critical of the age-segregated nature of American high schools and noted that youth in school were too far removed from work. With minor variations, each of these commissions maintained that working would enhance youths' education rather than detract from it, would better prepare them for the future, and therefore urged the relaxation of rules imposing limits on youth entering the workforce while still in school.³

Beginning in the mid-1980s the pendulum shifted again. Critics of the youth work movement, now armed with empirical data to support their position, advocated careful consideration of the dangers and risks of adolescent employment. The first empirical studies of the effect of adolescent work indicated that employment during the school year, particularly what was called "intensive work" (an average of more than 20 h per week), was related to poor school performance and involvement in a host of antisocial and "pseudo-adult" behaviors. Renewed skepticism about adolescent employment was given full expression in 1986 with the publication of Ellen Greenberger and Laurence Steinberg's *When Teenagers Work: The Psychological and Social Costs of Adolescent Employment*. Based on their extensive research on youth who worked during the school year, especially those who worked intensively, Greenberger and Steinberg found that adolescents often paid a high developmental price for working. The areas where youth were at greatest risk were their performance in school as well as participation in antisocial and "pseudo-mature" behaviors (e.g., smoking, drinking, using drugs, early sexuality). In rejecting the notion that high-school employment should be encouraged, Greenberger and Steinberg used language reminiscent of G. Stanley Hall some 70 years earlier:

The sort of psychosocial moratorium that is beneficial both psychologically and socially requires an environment that challenges the young person to develop more advanced cognitive and emotional equipment, yet at the same time permits and encourages such luxuries as daydreaming, fantasy, and harmless irresponsibility. (Greenberger and Steinberg 1986, pp. 167–173)

A voluminous amount of research published from the 1980s onward, which we review in detail in the next section, only confirmed the conclusion of these developmental scholars that adolescent employment comes with high psychological and social costs and should be discouraged. Working too much during the school year appeared to lead to higher dropout rates, poorer school performance, lower educational aspirations, and an elevated risk for delinquency and other problem behaviors such as early smoking, drinking, and illicit drug use. It was in large measure due to this growing consensus in the social science literature that intensive working during the school year was potentially harmful for adolescents that the National Research Council (NRC) proposed stricter limits on the hours that high-school students should be allowed to work during the school year. Specifically, the NRC recommended (1998, p. 226) that the federal government limit school-year work for young

³It is noteworthy that virtually all of these commissions made their recommendation for increased involvement of school-going youth in the workplace with little or no empirical evidence on the beneficial effect of adolescent employment (see Ruhm 1995).

people ages 16 and 17—a group presently allowed to work as many hours as they choose under the federal child labor law—to no more than 20 h per week. The NRC's recommendation formed the basis for the Youth Worker Protection Act (H.R. 3193), which was introduced into the 108th Congress (2003–2004). If approved as drafted, this bill would amend the child labor provisions of the Fair Labor Standards Act of 1938 to limit the employment of 16 and 17 year olds to no more than 20 h per week during the school year.⁴

Although extant research suggests that working intensively is detrimental across a variety of domains, none of this research directly studies the impact of current youth employment rules on behavior. The federal government already greatly restricts employment until age 16, at which point the federal child labor law governing work hours expires, and less restrictive state child labor laws prevail. This implies that there is a discrete jump in allowable work hours at age 16. After that age, there is considerable state-to-state variation in the level of work allowed for adolescents. Some research examines the impact of first-time employment at age 16 on a variety of developmental outcomes (Apel et al. 2007; Steinberg et al. 1993), which should at least be partially dependent on the work rules. However, this work does not exploit the variation across states in the amount of legal work allowed. Tyler (2003) used cross-sectional variation in work intensity that corresponds to state child labor laws to find that there appeared to be a relationship between lenient work rules and performance on standardized tests. Rothstein (2007), however, examined the impact of the work rules on GPA and found that the increased work due to the work rules was not negatively correlated with changes in the GPA. In this paper, we build on the work of Tyler (2003) and Rothstein (2007) and study the impact of state laws governing the hours of teenage employment on deviant behavior and academic achievement.

Focusing on changes produced by state child labor laws has two major advantages. First, we are studying the exact policy change being recommended by the National Research Council, and proposed in the past as a bill in the U.S. Congress. Our research provides evidence that can help us learn what might happen if we embark on major policy changes for youth, employers, and policymakers who currently spend billions of dollars trying to attach youth to the labor market. Second, this quasi-experiment provides an innovative way to deal with the vexing issue of sample selection bias in this area. It is well established that youth who choose to work, and particularly youth who choose to work intensively, are very different from youth who do not work in a number of important ways before they even enter the labor force. This means that, on average, youth who choose to work are not comparable to typical non-working youth. Because state-level variation in youth work rules should be uncorrelated with these individual characteristics, our analytical approach described in a later section provides us with more convincing estimates of the causal impact of employment and intensive employment on adolescent behavior.

In the next section of our paper, we review extant research on adolescent work (work intensity in particular) and deviant behavior and academic achievement (for more extensive treatments, we recommend Mortimer 2003; Steinberg and Cauffman 1995). We then develop our case that a change in the prevailing child labor law provides a compelling avenue by which to study the effects of intensive adolescent work on performance in and attachment to school as well as antisocial conduct. Then we discuss the data set we employ in our research, our measure of adolescent work intensity, two classes of outcome variables, and our cluster of control variables. After we develop our analytical model, which forms the basis for causal inference regarding adolescent employment experience, we present our empirical findings.

⁴The bill died in committee but was resubmitted in the 109th Congress (2005–2006) as H.R. 2870, where it was also tabled without resolution. As of this writing (May 2008), the bill has not yet been resubmitted in the 110th Congress.

Prior Empirical Research on Adolescent Employment

A vast empirical literature has investigated the correlates of school-year employment, including health-related behavior, psychological adjustment, performance in school, family relationships, deviant and antisocial behavior, and subsequent labor market experiences. With the exception of adult labor market outcomes, this research has demonstrated conclusively that “the correlates of school-year employment are generally negative” (Steinberg and Dornbusch 1991, p. 309). In spite of this apparent consensus, however, there are compelling reasons to exercise restraint in drawing strong causal conclusions from this research. In this section, we review the evidence relating to the two domains of interest in this paper—deviant behavior and academic achievement.

Deviant Behavior

The most consistent evidence pertaining to the possibly detrimental effect of adolescent employment is in the area of antisocial behavior. Virtually without exception, empirical research has indicated that youth who work during the school year, particularly those who work intensively, are at higher risk of delinquent behavior (Agnew 1986; Bachman and Schulenberg 1993; Bachman et al. 2003; Cullen et al. 1997; Mihalic and Elliott 1997; Ploeger 1997; Steinberg and Dornbusch 1991; Steinberg et al. 1993; Wright and Cullen 2004), work-place deviance (Ruggiero et al. 1982; Wright and Cullen 2000), and substance use (Bachman et al. 1981, 2003; Greenberger et al. 1981; Johnson 2004; Longest and Shanahan 2007; McMorris and Uggen 2000; Mortimer 2003; Safron et al. 2001; Steinberg and Dornbusch 1991; Wu et al. 2003) than their peers who work moderately or not at all. Even longitudinal research with controls for pre-employment differences has found that intensive employment puts students at risk for alcohol and drug use (Johnson 2004; Longest and Shanahan 2007; McMorris and Uggen 2000; Mortimer 2003; Mortimer et al. 1996; Staff and Uggen 2003). Moreover, although selection controls diminish the relationship between adolescent employment and deviance, the relationship fails to disappear entirely.

Recent empirical work has focused on the identification of pre-employment differences between workers and nonworkers. This research indicates that intensive work has no effect on the risk of delinquent behavior and substance use (Apel et al. 2006, 2007; Paternoster et al. 2003), and that working a long number of hours while still in school might actually be beneficial for some at-risk youth (Apel et al. 2007). The analysis by Paternoster et al. (2003) is particularly instructive. They first estimated a random-intercept model and found that the effect of intensive work on antisocial behavior (e.g., delinquency, substance use, problem behavior) was positive and statistically significant, a result wholly consistent with prior research. They then estimated a fixed-intercept model and found that within-individual change in intensive work had no impact on antisocial behavior. They concluded that the consistently reported positive correlation between intensive work and antisocial behavior was driven by a process of *selection* rather than *causation*. The possibility that the adolescent work effect on antisocial behavior may actually be spurious is strengthened in light of research consistently demonstrating that intensive workers tend to engage in more delinquency and substance use prior to labor market entry (Gottfredson 1985; Mihalic and Elliott 1997; Mortimer 2003; Ploeger 1997; Steinberg et al. 1993).

Academic Achievement

In the domain of academic achievement, most studies indicate that youth who work more than 20 h per week during the school year tend to have lower grades and are more likely to drop out of school than moderate or non-workers (Bachman et al. 2003; Bachman and Schulenberg 1993; Carr et al. 1996; D’Amico 1984; Greenberger et al. 1981; Lillydahl 1990; Marsh 1991; McNeal 1997; Mortimer and Finch 1986; Steinberg et al. 1982, 1993).

These studies form the empirical basis for the widespread belief that intensive school-year work has detrimental effects on youths' school performance and attendance and hence should be restricted. There has, however, been just as compelling evidence that even before they begin working in the formal labor market, intensive workers-to-be do not perform as well in school as their counterparts who ultimately refrain from working or who work only moderately (Bachman and Schulenberg 1993; Entwisle et al. 1999; Mortimer 2003; Schoenhals et al. 1998). While researchers have generally been mindful of possible selection effects, controls for selection have not been particularly rigorous. Even so, when modest controls for selection are introduced, they substantially reduce the reported relationship between intensive work and achievement.

Relatively recent research with more rigorous selection controls has not consistently found an inverse relationship between intensive work and school performance. For example, Schoenhals et al. (1998) reported that the adverse effect of work intensity on measures of school performance was largely due to pre-existing differences among youth who work at varying levels of intensity. Using the National Education Longitudinal Study (NELS), they found that once these differences were taken into account, youth who worked more than 20 h per week had no lower grades than those who worked moderately or not at all, nor did they spend less time on homework or on reading outside of class. Moreover, in what may be considered a healthy reallocation of their available time, intensively working youth spent significantly less time watching television (see also Osgood 1999; Warren 2002). Mortimer (2003; also Mortimer et al. 1996) found no relationship between hours spent working and school grades, time spent on homework, or taking difficult subjects in school. Similarly, Warren et al. (2000) found that those who were poorer students initially tended to work intensively, but that working a long number of hours had no effect on school grades in academic classes.

In the one exception to these findings of no adverse effect of intensive work on school success, Tyler (2003) instrumented for work intensity using state child labor laws and penalties assessed against employers for violation of these laws, and reported that students who worked longer hours had significantly lower math achievement scores.⁵ More recently, Rothstein (2007) showed that while the number of hours worked during school was weakly related to lower grades for both males and females, the relationship declined to non-significance in a fixed-effects model with hours instrumented by unemployment rates and state work rules on teenage employment.

While these findings with respect to the relationship between school-time employment and grades are informative, the theoretical model linking work involvement to delinquency operates through decreased school attachment rather than decreased school performance (Hirschi 1969). That is, working during the school year is thought to be harmful because, by putting youth at risk for dropping out, it moves them away from a source of conventional bonding. There is very little research on the impact of work intensity on school dropout, but that which does exist generally suggests that intensive employment may not be so benign. McNeal (1997), for example, found that while informal jobs such as babysitting and lawn mowing reduced the risk of dropping out of school, formal or "paycheck" work increased it. Tienda and Ahituv (1996) reported that youth from more advantaged families who worked were more likely to dropout out, but those from disadvantaged families were unaffected. Entwisle et al. (2005) reported that, while working at a paycheck job increased the risk of dropout among 15 year olds, working youth who weathered that transition and remained in school at age 16 were subsequently less likely to drop out than non-working youth. Using

⁵These findings led Tyler (2003, p. 405) to recommend that "if a primary policy objective is to maximize twelve-grade academic achievement, then states should possibly consider more restrictive child labor laws for 16–17-year-olds."

NELS, Warren and Lee (2004) found that school-year work intensity was positively related to dropout after controlling for covariates that captured possible pre-employment differences. Comparable findings were reported by Warren and Cataldi (2006) with five different longitudinal data sets.

To summarize, literally dozens of existing studies leave little doubt that work itself, and more conclusively intensive work during the school year, is positively correlated with a wide swath of negative developmental outcomes for adolescents, with the most consistent findings for antisocial behavior and dropping out of school. Such findings led the National Research Council (1998) to make a policy recommendation that was focused squarely on the number of hours that adolescents spend in the workplace, with the intended goal of disallowing young people from working intensively while they are still in school and thus protecting them from the negative consequences that would obviously ensue. At first glance, the NRC's recommendation and the resulting Youth Worker Protection Act seem to be based on solid empirical footing. However, despite the breadth of evidence about the *presence* of a correlation between intensive employment and developmental maladjustment, we are far less confident about its *causal significance*.

More importantly, most existing studies do not actually study the change in behavior associated with the laws governing youth work. The one study that was done by Rothstein (2007) used work rules along with county unemployment rates and average wage rates by region and gender as instrumental variables for work intensity. She found that these instruments affected work intensity, but that work intensity was no longer correlated with GPA when these instruments were used. We extend her work in this paper with a focus on the work rules alone, and their impact on both delinquency and dropout. If the consensus about these laws is correct, we should see both an increase in school dropout and delinquency and a decline in school performance as youth take advantage of more liberal state laws regarding the number of hours they are permitted to work while in school. If the concerns about selection demonstrated by earlier work are correct (Apel et al. 2007; Paternoster et al. 2003), there should be no relationship between work intensity and school success, dropout, or delinquency.

Data

We use the first seven waves of the geocoded National Longitudinal Survey of Youth 1997 (hereafter NLSY97). The NLSY97 is a nationally representative sample of 8,984 youth born during the years 1980 through 1984 and living in the United States during the initial survey year in 1997. It consists of a cross-sectional sample of respondents representative of all youth and an oversample of black and Hispanic youths.⁶ The NLSY97 has distinct advantages to recommend its use for the current study. For one, it is a nationally representative sample, providing generalizability to the population of all youth in the United States who were 12–16 years of age in yearend 1996. In addition, the NLSY97 gathers information relevant to the school-to-work transition, and administers a series of self-report modules related to health and wellbeing, antisocial behavior, and school performance. Finally, the respondents are surveyed annually, providing a unique opportunity to examine changes in employment and behavior as youth mature. The appendix provides a list of the variables (and definitions) used in this analysis.

Of special interest is the transition from age 15 to age 16 because of the easing of child labor restrictions that occurs during this period. To avoid confounding change in the age-

⁶We present our empirical results from unweighted analyses. However, we hasten to add that we reestimated all of our models using normed sampling weights to adjust for the minority oversample, and our findings were virtually identical.

eligibility of the child labor statute with change in the statute itself, either because of legislative activity or cross-state mobility, we exclude respondents who reside in the four states that impose statutory changes concerning the employment of 16 and 17 year olds during the interview years that concern this analysis (CT, KY, VT, WI), as well as respondents who change their state of residence during the 15-to-16 transition. Our sample of interest includes 2,224 youth who participated in two consecutive interviews while ages 15 and 16, and who were not employed in a formal job when they were 15 years old. For this sample, we are interested in the causal effect of first-time formal work involvement at age 16 on behavior. Our decision to restrict our sample to non-workers at age 15 was based on two substantive considerations. First, we wanted to map our results as closely as possible to previous research in this area, and a great deal of that research has examined the effect of intensive work on those who had not worked in the past. Second, Figs. 1 and 2 shows that there is a substantial increase in the percent of youth working for the first time in the transition from 15 to 16 years of age. In restricting our sample we are, then, estimating a local average treatment effect. Table 1 provides descriptive statistics on our key variables, which we describe next.

Adolescent Work Involvement

The key independent variables are Formal Employment, whether a respondent worked in a formal job, and *Work Intensity*, the number of hours worked per week in a formal job. Recall that since we condition the sample on 15-year-old non-workers, these variables measure first-time work involvement at age 16. The NLSY97 distinguishes between two types of employment—informal work and formal work. Formal jobs are defined as “a situation in which the respondent has an ongoing relationship with a specific employer” (a “paycheck job”), while informal jobs are defined as “jobs for which the respondent performed one or a few tasks for several people without a specific boss, or in which the respondent worked for himself or herself” such as babysitting, yard work or paper routes (Center for Human Resource Research 2002, p. 96). Because child labor laws regulate formal but not informal employment, this analysis focuses explicitly on the former type of work. Beginning with a youth’s 14th birthday, the NLSY97 creates a week-by-week work history of all formal jobs, denoting the youth’s work status (employed, unemployed, out of the labor force, in the military) and work intensity during each calendar week if employed, accounting for within-job gaps in employment due to layoff, pregnancy, leave of absence, etc. Our measure of formal employment is a binary work status measure coded “1” for youth who were employed in a formal job for the first time at age 16. Our measure of work intensity is a continuous measure of the total number of hours worked at age 16, divided by the total number of weeks worked. To minimize the impact of outliers, we censor work intensity at 60 h per week, which represents the 99th percentile.

Dependent Variables

We are interested in six different outcomes that encompass deviant behavior and academic achievement. Definitions for each of these variables are provided in the Appendix. Our three measures of deviant behavior include delinquency, arrest, and substance use. *Delinquent Behavior* is a composite of seven self-report offenses: (1) intentionally destroying someone else’s property; (2) stealing something worth less than 50 dollars; (3) stealing something worth more than 50 dollars, including a car; (4) fencing, receiving, or selling stolen property; (5) attacking someone with the intent of inflicting serious harm; (6) selling marijuana or hard drugs; and (7) carrying a handgun. We dummy code each behavior, so that youth who report engaging in the behavior on at least one occasion since the last interview are coded “1,” and all non-participating youth are coded “0.” The measure of delinquent behavior is, then, a variety score constructed as the sum of all seven binary indicators of participation. It measures the number of different criminal behaviors

committed since the last interview, and has a range of possible values from zero to seven. *Arrest* is a dummy variable for self-report arrest since the last interview. *Substance Use* is a composite of three behaviors: (1) smoking cigarettes; (2) drinking alcohol; and (3) using marijuana. Similar to delinquency, we construct a three-item variety score that represents the number of different substances used since the last interview.⁷

Our three measures of academic achievement include suspension, grades, and dropout. *School Suspension* is the self-reported number of days suspended from school since the last interview. To minimize the influence of outliers, we censor this variable at 30 days. With this response variable, we also restrict our attention to those youth who are enrolled in school at both interviews during the transition.⁸ *Transcript Grades* is the grade point average (based on a 4.0 scale) that we create from the school transcript file. We include grades from all coursework during all school terms that overlap with the time elapsed since the last interview, but we weight each term's grade by the degree to which it overlaps with the reference period.⁹ *School Dropout* is a binary indicator coded "1" if the respondent reports that he or she is not currently enrolled in school and does not have a high school diploma. Note that we classify GED recipients as dropouts because of research showing that they more closely resemble dropouts as opposed to graduates (Cameron and Heckman 1993).

Control Variables

Although not a central focus of the present study, we also include a number of control variables measured at the level of both the individual and state. For details on the coding of these variables, we refer readers to the Appendix. Our individual-level control variables include continuous measures of *Residential Mobility*, *Household Size*, and *Highest Grade Attended*, as well as dummy indicators for *Residential Location*, *Dwelling Type*, *Reached Puberty*, *Have Driver's License*, *Worked in an Informal Job*, and *Earned an Allowance*. In order to account for statutory eligibility in other transitional behavior that varies across states, we control for *School Dropout Eligible*, *Sexual Consent Eligible*, and *Unrestricted Driving Eligible*.¹⁰

We also incorporate several state-level control variables in order to adjust for conditions that could be correlated with state child labor laws as well as youth employment prospects. From the Regional Economic Information System of the Bureau of Economic Analysis, we control for state *Gross Domestic Product*, *Income Per Capita*, *Transfer Payments*, and Total Employment, as well as state industrial mix relevant to youth employment (*Percent Retail Industry*, *Percent Service Industry*). Note that all dollar values are in logged 2000 dollars. From annual editions of the *Statistical Abstract of the United States*, published by the

⁷By using a variety scores for delinquent behavior and substance, we settled on a compromise between single binary indicators and frequency scales. We employed variety scores because frequency scales are generally dominated by less serious offenses. Furthermore, prior research has demonstrated that variety scores are as reliable as frequency scales (Hindelang et al. 1981).

⁸Youth who have dropped out of school, not surprisingly, report an unusually high number of suspensions. The pattern of significance is unchanged when we exclude them. In fact, by excluding dropouts our point estimates for work intensity in the school suspension models are somewhat more conservative.

⁹This variable unfortunately suffers from a substantial degree of non-response. During waves two (1998) and seven (2003), high-school transcripts were requested from the last school of record for every individual who was over 18, who was no longer enrolled in high school, and who had provided written consent. Of the 8,984 youth in the NLSY97, survey staff were successful in collecting high-school transcripts for 6,232 (69.4%). Transcript data are missing because student consent was refused ($n = 1,734$), the student was still in high school and the transcript was not requested ($n = 132$), the school was unable to locate the student record ($n = 544$), and the school or district refused to turn over the transcript ($n = 342$).

¹⁰These variables are dummy coded. For example, if a respondent is eligible to drop out of high school under state law by virtue of her age at interview, we code her "1" on school dropout eligibility and "0" otherwise. Similarly, statutorily age-eligible youths are coded "1" on consensual sexual intercourse and unrestricted driving privileges. See Appendix for further detail. These control variables are intended to absorb variation in other state statutes with age-eligibility conditions that might be correlated with the prevailing child labor law. We are careful to ensure as much as possible that youth employment is being driven by change in workeligibility under the child labor law rather than by dropout-eligibility under school attendance laws, for instance (for a study of the impact of school attendance laws on behavior, see Lochner and Moretti 2004).

Bureau of the Census, we also control for *Youth Labor Supply*, *Unemployment Rate*, *Female Labor Force Participation Rate*, *Union Membership*, *School Enrollment Rate*, *Per Pupil Expenditure*, and *Index Crime Rate*.

Method

The goal of this study is to evaluate the impact of the transition to the labor market at age 16 on various dimensions of youth behavior. The structural model of interest is specified in the following manner:

$$Y_{it} = \alpha_0 + \alpha_1 W_{it} + \alpha_2 X_{it} + \alpha_3 S_{it} + \lambda_i + e_{it}$$

where i indexes individuals and t indexes interview years. In this model, Y_{it} is one of our six response variables, W_{it} is a measure of work intensity (number of hours per week), X_{it} is a matrix of time-varying individual control variables, S_{it} is a matrix of time-varying state control variables, λ_i is an unobserved individual effect, and e_{it} is a disturbance with the usual properties. To maintain consistency with the existing literature on work intensity, we proceed with the estimation of three different models, each imposing different assumptions about unobservables and relying on different sources of identification.

The first model represents the usual random-effects (RE) or random-intercept model, in which the individual effect (λ_i) is assumed to be randomly drawn from a normal distribution with mean zero and standard deviation estimated by the model. This distributional assumption renders RE comparatively efficient. However, the individual effect is also assumed to be independent of the regressors, meaning that time-stable unobservables are presumed uncorrelated with the number of hours that youths are employed. If this assumption is violated, the work intensity effect α_1 is biased and inconsistent.

The second model of interest is the fixed-effects (FE) model, in which individual-specific means are subtracted off from the value at each time period. Because the individual effect does not vary over time, it is swept out of the model by this within-transformation. A distinct advantage of the FE model is that it relaxes the assumption of independence between the individual effect and the regressors. However, FE is by no means a panacea. It is quite possible that change in work involvement is correlated with change in other dynamic omitted variables, making the FE estimate of the work intensity effect α_1 biased and inconsistent.

The third model is our model of substantive interest, because it is driven by variation in work intensity caused by the state child labor laws. We use the FE transformation to eliminate the individual effects and then choose instruments for within-individual change in work intensity. The instruments that we choose are a set of state child labor laws governing the amount of time that young people are allowed to devote to working during the school year. At age 15, all respondents are under a uniform child labor regime monitored by the federal government. At age 16, however, the federal regime expires and is replaced by non-uniform state child labor regimes. Thus, all respondents experience a change in the prevailing child labor regime during the 15-to-16 transition, although the nature of the change varies across states. This model is estimated using the method of two-stage least squares, and we refer to it as the first-effects instrumental variables (FEIV) estimator.

In the FEIV model, identification of the “work intensity effect” is predicated on the exogenous within-individual change in work involvement that can be attributed to the easing of child labor restrictions as youth age out of the federal child labor regime governing 15-year-old employment and into different state child labor regimes governing 16-year-old

employment. It is important to point out that the FEIV model does not avail itself of all of the available within-individual variation in employment, but only that portion that is explained by change in child labor laws. We are particularly interested in this variation for policy purposes, but we also believe that this variation is exogenous with respect to the individual outcomes we are studying. In the next section, we elaborate on our instrument selection.

State Child Labor Laws as Instruments for Adolescent Employment

The aggregate effect of the 15-to-16 transition on the youth labor market is demonstrated in Fig. 1. Here, we plot the proportion of adolescents who are employed in any given month between the ages of 14 and 19. Notice first that fewer than 10% of youth are formally employed when they are 14 years of age, but that virtually all youth (85.8%) gain some formal work experience by their 18th birthday. Most important for the analytical strategy that we propose in this paper is the fact that something very important happens at age 16. In the month prior to their 16th birthday only 23.5% of youth are employed. By the 6th month of their 16th year, however, 41.5% of all youth are employed, and by the 11th month of the same year, 46.7% of youth are employed. Thus, within 6 months of turning 16 years of age, 75% more youth are working, and just before they turn 17 twice as many youth are working.¹¹ The importance of the 16th year is reinforced in Fig. 2, in which we graph the ages at which young people acquire their first formal job. Well over one-third (35.7%) of employed young people begin working their first formal job at the age of 16. Thus, there is a clear discontinuity in the aggregate youth work experience at the 15-to-16 transition. It is this age-graded discontinuity, which we argue is attributable to a change in employment eligibility under federal and state child labor laws, which we intend to exploit for causal identification using an instrumental variables model.

There are three child labor laws that directly concern the number of hours per week that adolescents may work—the maximum number of hours per week, the maximum number of hours per weekday, and the latest time of evening work allowed. At age 15, most youths are under the jurisdiction of the federal child labor law (the Fair Labor Standards Act of 1938) that restricts their school-year work involvement to 18 h per week (40 during summer), 3 h per weekday (8 during summer), and a 7:00 pm work curfew on evenings before a school day (9:00 during summer). At age 16, however, the federal child labor law expires and is supplanted by state child labor laws. At this age, the modal state child labor law is one that allows unlimited work involvement during the school year (23 states). The remaining 27 states and the District of Columbia impose a variety of constraints on youth work involvement until age 18. For example, 16 year olds in California are limited to 28 h per school week, 4 h per school day, and a 10:00 pm work curfew on school nights. Similarly, 16 year olds in Pennsylvania are not allowed to work more than 28 h per week during the school year, but may work up to 8 h per school day and as late as 12:00 midnight on school nights. These cross-state differences are an important source of quasi-experimental variation.

To elaborate further for illustrative purposes, we limit our attention to state child labor laws governing the maximum number of hours per week. Figure 3 provides an intuitive way of thinking about how a change in child labor law eligibility influences change in formal employment and work intensity. We graph the mean within-individual change in work intensity among youth who are 15 years old at one interview and 16 at the next, grouping them by the work hours allowed for 16 years olds under the child labor law of the state in

¹¹The same figures for cumulative employment are 42.8% in the month before turning 16, 63.4% 6 months into the 16th year, and 72.8% in the month before turning 17.

which they reside. We select youth were not employed at age 15. Recall that at age 15 virtually all youth are subject to the 18-h restriction under the federal child labor law, while at age 16 they come under state laws that vary with respect to the number of working hours they permit. The question is, then, does the working behavior of youth change in response to changes in state work rules? The answer to this question is, “yes”—different state laws do have a meaningful impact on youth behavior. Notice that there is a positive slope in Fig. 3, indicating that youth residing in states that permit more hours of work during the school year actually do, on average, work longer hours. A 20-h increase in the number of permitted work hours during the school year—from 28 to 48 h per week— corresponds roughly to an increase of about 2 h per week in actual work. This figure thus clearly shows that youth work behavior is responsive to the child labor law that prevails in the state within which they reside. This is critical for our approach because we intend to take advantage of the variation in state laws as an exogenous source of variation in the working behavior of youth.

We thus have graphical evidence that the change in state child labor laws does indeed influence change in both adolescent work status and work intensity. In further examining the usefulness of using state labor laws as instruments for formal employment and work intensity, Table 2 provides fixed-effects estimates of the relationship between state child labor laws and work intensity, incorporating our entire set of time-varying, individual- and state-level control variables. The models are estimated from the 2,224 youth who were 15 years of age at one interview and 16 years of age at the following interview, and who were not employed at age 15. The dependent variable in these models is a continuous measure of the number of hours worked per week at age 16. Key measures of instrument relevance are provided at the bottom of this tables, and include the model *R*-square, the change in *R*-square with the inclusion of the instrumental variables, the partial *R*-square for the instruments (Shea 1997), an *F*-statistic for the joint significance of the instruments (Staiger and Stock 1997), and our estimate of the approximate finite-sample bias of FEIV relative to FE (Bound et al. 1995).¹²

We provide four different specifications of the first-stage models in Table 2. We begin with Model 1, which models change in work intensity as a linear function of change in the number of hours per week allowed under the state child labor law as well as a dummy variable for states with unlimited hours allowed (these states are coded as allowing 0 h per week on the continuous measure), which is also treated as an excluded instrument. The coefficient implies that a 10-h increase in the number of hours that adolescents are allowed to work corresponds with a mean increase of 0.32 h per week. In Model 2, we substitute number of hours per weekday allowed, and in Model 3 we substitute the work curfew. The pattern of findings remains unchanged, in that more liberal child labor laws are associated with larger within-individual increases in work intensity. In Model 4, we include all three child labor laws together. Because these laws are highly correlated, they are no longer individually significant in a consistent way. However, as indicated by the *F*-statistic, they are indeed jointly significant, which is paramount for evaluating their plausibility as excluded instruments. The fact that they withstand the inclusion of an exhaustive array of

¹²Figure 3 demonstrates that, while change in work intensity is correlated with change in child labor laws, the correlation is modest. An important limitation of the instrumental variables estimator is that, while consistent, in finite samples it is known to be biased in the direction of the least squares estimator. This problem is exacerbated in the “weak instrument” case. In such a situation, first-stage diagnostics become paramount in evaluating the validity of the chosen instruments. The first-stage *F* and partial *R*-square for the instruments are common metrics (Shea 1997; Staiger and Stock, 1997), with larger values obviously preferred. Our estimate of the relative bias is derived from Bound et al.’s (1995, pp. 449–450) approximation, given by the formula: $1 - (\tau^2/k) {}_1F_1(1, (k+2)/2; -\tau^2/2)$ where *k* is the number of instruments, τ^2/k is the *F*-statistic for the joint significance of the instruments, and ${}_1F_1(\bullet, \bullet; \bullet)$ is the confluent hypergeometric function evaluated at the argument. By this metric, smaller values are preferable and indicate less finite-sample bias in the FEIV model relative to the FE model. From our first-stage models, we estimate the finite-sample bias to be at most 4.5% (from Model 4), which is quite respectable.

individual- and state-level control variables increases our confidence that they are truly exogenous.

Results

In the top panel of Table 3, we provide the structural estimates for the effect of work intensity on deviant behavior, academic achievement and school attachment. In the first column we summarize the coefficients from random-effects (RE) models. These results demonstrate that longer hours have uniformly negative consequences for behavior. For example, youth who work are significantly more likely to report delinquent behavior, are more likely to be arrested, are more likely to use alcohol and drugs, are more likely to get suspended from school, have lower grades, and are more likely to drop out of school. On balance, these results are consistent with the vast literature on the detrimental effect of youth work involvement, which forms the basis for the National Research Council's (1998) call for stricter federal limits on school-year work for adolescents. This literature unambiguously suggested that working too many hours each week (i.e., working "intensively") encouraged a variety of problem behaviors and pulled youth away from school-related pursuits.

Despite the consistency of results, random-effects models are based on some rather strong assumptions (Paternoster et al. 2003). One way to assess their reasonableness is to estimate the model in fixed-effects (FE) form, the results of which are provided in the second column of Table 3. In these models, the effect of work intensity on delinquency, arrest, and suspensions disappears altogether, and diminishes substantially (by half) for substance use and grades. These results largely replicate the work of Paternoster et al. (2003) and Rothstein (2007). Selection into employment is clearly non-random.

In the third column of Table 3, we provide the coefficients from the fixed-effects instrumental variables (FEIV) models. These models are fundamentally different because they are identified solely from state child labor laws. In these models, the first stage is specified as in Model 4 of Table 2, with all three state child labor laws treated as excluded instruments. Recall that identification in the FEIV model comes from the portion of within-individual change in work intensity that can be attributed to statutory changes in allowed hours when the child labor regime changes from the federal to the state level. In other words, we exploit only the variation in work involvement that is exogenously determined by change in the restrictiveness of the prevailing child labor law.

We find that the adverse effect of work intensity on school dropout remains robust, and in fact increases by a non-trivial order of magnitude when compared to the FE model, from .002 to .011. The impact of work intensity changes signs from positive to negative and becomes significant for delinquent behavior and school suspension. Specifically, the transition to formal work corresponds with a significant and substantial *decrease* in delinquency and suspensions. For the remaining three outcomes—arrest, substance use, grades—an increase in work intensity has no discernible effect, statistically speaking. The finding for grades replicates the work of Rothstein (2007), but our results do not tell the same story as the FE model. Even with very strong controls for selection, more intensive work involvement appear to lead to increased school dropout, decreased delinquency, and decreased suspensions from school.¹³

In the bottom panel of Table 3, we provide reduced-form estimates of the effect of state child labor laws on each of the response variables. Reduced-form models simply demonstrate the effect of the instrumental variables on the response variables, which are presumed to affect outcomes only through their impact on work intensity. Whereas the structural estimates are from linear models, the reduced-form estimates are from non-linear

models (where appropriate), and serve as a test of robustness of the linear functional form as well as sensitivity to the choice of instruments. These are fixed-effects models limited to the subsample of youth that change their value on the response variable between ages 15 and 16. The reduced-form results are consistent with expectations from the structural estimates provided in panel A. If state child labor laws significantly influence work intensity, and work intensity significantly influences the outcome, then the state child labor laws should significantly influence the outcome. This appears to be true with respect to delinquency, school suspensions, and dropout. Moreover, notice that the work curfew laws are the most strongly related to these three outcomes. This is not surprising, because the first-stage model using work curfew as the instrument was the strongest (see the diagnostics for Model 3, Table 2).

Discussion

This study was motivated by the National Research Council's (1998) recommendation that federal child labor laws be amended to impose stricter limits on youth work intensity and by the introduction of the Youth Worker Protection Act (YWPA) in the U.S. Congress to implement that recommendation. To date, literally dozens of studies have found that youth who work longer hours while in high school are at elevated risk with respect to deviant behavior and academic achievement. However, an observed correlation between work intensity and problematic transitional behavior is not the same as showing that the relaxation of rules governing youth work would increase problem behavior, or conversely, that the imposition of stricter rules would decrease problem behavior. In this study, we directly study the impact of child labor laws on changes in behavior as youth transition from the federal to the state child labor regime. The difference in state laws in this case is one that is of direct policy relevance for the question posed by the National Research Council and the resulting Youth Worker Protection Act.

We find that first-time, formal work involvement at the 15-to-16 transition appears to provide some benefits as well as to impose some costs for adolescent behavior. For example, we find that higher work intensity produces a significant and substantial *decrease* in delinquent behavior and school suspensions, but an equally substantial *increase* in the likelihood of school dropout. On the other hand, we find that there is no change in substance use or transcript grades, and no change in the risk of arrest. Given our sample composition (15 year old non-workers) and our estimation strategy (fixed-effects instrumental variables), the work intensity effects on delinquency, suspensions, and dropout may be interpreted as causal. To be precise, each represents a "local average treatment effect" (LATE), or the effect of work intensity on the outcome among those youth who change their work behavior in response to the easing of child labor restrictions at the 15–16 transition (for extended discussion of LATE in the context of instrumental variables models, see Angrist et al. 1996; Imbens and Angrist 1994).

¹³Given space constraints, we only briefly describe some of the more important sensitivity analyses that we conducted. Our results were substantially similar across the robustness tests. First, we experimented with different measures of employment in addition to the continuous measure of work intensity used here—a dichotomous indicator for employment, a continuous measure of work intensity limited to youth who were employed at age 16, and dichotomous indicators for moderate (1–20 h per week) and intensive (over 20 h per week) employment. Second, we evaluated the sensitivity of our estimates to the exclusion of subsets of the instrumental variables set. Third, we substituted a series of dummy variables for the state child labor laws rather than continuous measures by grouping states with similar restrictions (e.g., up to 20 h maximum allowed per week, 21–30 h maximum, 31–40 h maximum, and so on). Fourth, we substituted the mean number of hours worked per week during the nine months of the school year, rather than during the entire calendar year. Fifth, to be sure that our instruments were identifying only variation in work intensity at the first stage, we controlled for several other characteristics of youth jobs, including the number of different jobs, the total number of weeks worked, and hourly wages. Sixth, we systematically removed states one at a time to ensure that outlying states were not exerting inordinate influence on our point estimates. Seventh, to be sure that our results were unaffected by period variation, we removed one interview wave at a time from our models.

Our finding that work involvement has little or no effect on arrest, substance use, and academic performance is consistent with comparatively recent work employing stronger selection controls (Apel et al. 2006, 2007; Mortimer 2003; Mortimer et al. 1996; Paternoster et al. 2003; Rothstein 2007; Schoenhals et al. 1998; Warren et al. 2000). However, the inverse causal effect of work intensity on delinquency was unexpected, and it challenges the conclusions from virtually all prior empirical work and one of the justifications for the YWPA. But these results are less surprising in a larger context. Research on adult employment consistently indicates that adults who are strongly attached to work and who acquire full-time employment are less likely to be criminally involved (Fagan and Freeman 1999; Grogger 1998; Hagan 1993; Horney et al. 1995; Sampson and Laub 1993; Thornberry and Christenson 1984; Uggen 1999, 2000; Witte and Tauchen 1994). These seemingly anomalous results force researchers into the position of suggesting that the “sign” of the work effect changes at some point during the transition to adulthood, that is, that strong attachment to work (as measured by the number of hours per week) is criminogenic for adolescents but prophylactic for adults (e.g., Blumstein et al. 1986, p. 52; Uggen 2000, p. 530). Our present findings suggest that there is no such change in the effect of work involvement on criminality in adolescence and adulthood. On the contrary, our finding about the ameliorative effect of work involvement in adolescence is friendly to the position that general processes probably govern the employment-crime relationship at all life stages, at least from age 16 onward.

In our view, the more theoretically puzzling result is the empirically robust finding that work leads to dropout at the same time that it decreases delinquency. Adverse developmental outcomes are often thought to coalesce into a single package. And, the literature is unambiguous that one mechanism for increased delinquency is detachment from school (Steinberg and Cauffman 1995). The problematic link in this story is between dropout and crime, for which there is a large literature. As in the case of the work literature reviewed above, there is a strong positive relationship between dropout and crime in the cross section, but there has also been a consistent strand of longitudinal work showing that that delinquency actually decreases after dropout. Elliott and Voss (1974) and Elliott (1966) found that rates of official delinquency for those who would eventually drop out of school were highest just before they quit, then declined sharply (regardless of the age at which leaving school occurred). Other research has also shown that the level of delinquency declines among those who drop out of school (LeBlanc and Fre'chette 1989; for review, see Phillips and Kelly 1979).

On the other hand, there is also some work that shows a criminogenic effect of school dropout, particularly over the long run. Thornberry et al. (1985) found, for example, that dropping out of school had a modest, short-term criminogenic effect. Additionally, when they examined the effect of dropping out on post-school arrest histories up to age 25, they found a positive relationship between dropping out and crime, controlling for age, race and social status. Lochner and Moretti (2004) similarly found that high-school non-completion was associated with higher self-report crime in the short term (at least among white males), but resulted in a substantial long-term increase in the risk of incarceration that they interpreted as reflecting a genuine increase in the criminal behavior of dropouts rather than differential processing by criminal justice officials.

Some of the most definitive work comes from Jarjoura (1993, 1996), who used the National Longitudinal Survey of Youth 1979. Jarjoura (1993) examined the relationship between dropping out and subsequent delinquent offending separately for seven different self-reported reasons for leaving school early and three different offense types. He found that net of selection controls, dropping out of school to get married, because of pregnancy, because of a dislike for school, and for “other” reasons increased subsequent involvement in violent

delinquency. However, *dropping out of school for economic reasons (employment) decreased subsequent delinquency*. Following up on this study, Jarjoura (1996) investigated whether the relationship between dropping out and delinquency varied by the youth's social class, again controlling for a set of observed covariates for selection controls. Dropping out of school for economic reasons had a crime inhibiting effect on theft for lower status youth. Sweeten et al. (2008) recently completed the first study of dropout on crime using the NLSY97, the same dataset used in this paper. With strong controls for selection, they found no overall impact of dropout on delinquency. However, when they explored the impact by reason for dropout, they again found that dropout for economic reasons lead to a decrease in delinquency. This result is completely consistent with the results in this paper that increased work leads to increased dropout, but decreased delinquency. Sweeten et al. (2008) suggest that the explanation for this set of findings can be found in identity theory. Increased work provides a positive identity for youth who are already detached from school or find little positive identity in school.

Conclusion

The present findings unambiguously show that policy pronouncements intended to “protect” youth from the negative consequences of intensive employment are based on a thin empirical base with respect to most of the behaviors that justified their adoption. Much emphasis has been placed on the problem behavior consequences of intensive youth work. Consider the following quotes:

The preponderance of evidence ... has found higher rates of problem behaviors, such as alcohol and other drug use and minor delinquency, among young people who work—*particularly among those who work at high intensity*—in comparison with their nonworking peers. (National Research Council 1998, p. 132, emphasis added).

Research clearly indicates that *working more than 20 h a week* in addition to a normal school schedule has a negative effect on student's [sic] academic progress. Additional studies show that children who work long hours also tend to use more alcohol and drugs. (Remarks made before the U.S. House of Representatives during the introduction of the Youth Worker Protection Act, H.R. 3139, by Representative Tom Lantos on September 24, 2003; emphasis added).

Our findings suggest that the emphasis on work intensity as a causal agent of widespread problem behavior is overdrawn. In fact, with respect to delinquency, arrest, substance use, school suspensions, and grades, the emphasis is entirely misplaced.

The results with respect to school dropout, on the other hand, are cause for concern. Working may be viewed as preparation for the future among youth for whom a high-school diploma is of dubious value (Newman 1999; Sullivan 1989), and certain youth, particularly disadvantaged ones, might “channel their energies toward work as an alternative arena for success” (Entwisle et al. 2000, p. 292). Although in the short run, employment that leads to dropping out may pay off by making additional work opportunities available, it may backfire in the long run if the returns to high-school completion are substantially higher than the returns to high-school work experience (on this point, see Hotz et al. 2002). It may also backfire in the area of crime control, given the apparent correlation between high-school dropout and long-term crime outcomes (Lochner and Moretti 2004; Thornberry et al. 1985). However, policy solutions that would impose more restrictive child labor laws, as the Youth Worker Protection Act would enact at the federal level, *may do more harm than good because many state child labor laws apply to youth who are enrolled in school*. Dropping out of school frees youth from the restrictions of the prevailing child labor law, and would

thereby free them to work as many hours as they choose. The possibility that substantial numbers of youth temporarily “stop out” rather than permanently “drop out” is encouraging (Entwisle et al. 2004). It is possible that those who drop out of school to go to work subsequently return to school with increased motivation for achievement. More research on this complex relationship between policies on employment and schooling is clearly warranted.

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Appendix

Variable definitions

| Variable | Definition |
|---|---|
| <i>Adolescent work involvement</i> | |
| Formal employment | =1 if respondent was employed for at least 1 week in a formal job since the last interview |
| Work intensity | Number of hours worked per week, on average, computed as the total number of hours worked since the last interview, divided by the total number of weeks worked since the last interview |
| <i>Deviant behavior, academic achievement, & precocious transitions</i> | |
| Delinquent behavior | Variety score representing the sum of the number of different kinds of illegal behaviors committed since the last interview: (1) vandalism; (2) minor theft (under 50 dollars); (3) serious theft (over 50 dollars); (4) “other” property crime (e.g., fencing, receiving, possessing, or selling stolen property); (5) aggravated assault; (6) sold or helped sell drugs; (7) handgun possession |
| Arrest | =1 if respondent was arrested by police or taken into custody for an illegal or delinquent offense since the last interview, excluding minor traffic violations |
| Substance use | Variety score representing the sum of the number of different kinds of substances consumed since the last interview: (1) cigarettes; (2) alcohol; (3) marijuana |
| School suspension | Number of days suspended since the last interview (censored at 30). Excludes respondents who are not enrolled in school at both interviews |
| Transcript grades | Grade point average (on a 4.0 scale), created from the school transcript file, for all terms that overlap with the reference period, weighted according to the number of term days that occur since the last interview |
| School dropout | =1 if respondent is not currently enrolled in school and does not have a high school diploma. Respondents with a G.E.D. are classified as dropouts. Note that school dropout is not an absorbing state. That is, a respondent may be classified as a dropout in one time period but return to school the next |
| <i>Individual-level control variables</i> | |
| Residential location | |
| Central city | =1 if respondent lives in a metropolitan statistical area (MSA) in the central city |
| Suburbs | =1 if respondent lives in MSA not in the central city |
| Rural | =1 if respondent does not live in MSA |
| Dwelling type | |
| House, condo, or farm | =1 if respondent lives in a house, condo, townhouse, row house, farm, or ranch |

| Variable | Definition |
|--------------------------------------|--|
| Apartment or flat | =1 if respondent lives in an apartment or flat |
| Other dwelling | =1 if respondent lives in some other type of dwelling (e.g., hotel/motel, rooming house, trailer) |
| Residential mobility | Mean number of different residences per year since age 12 |
| Household size | Number of people that currently live in household |
| Highest grade attended | Highest grade attended as of the interview |
| Reached puberty | =1 if pubertal changes seem completed (for boys) or youth has had a menstrual period (for girls) |
| Have driver's license | =1 if respondent has a driver's license |
| Worked in an informal job | =1 if respondent worked in an informal ("freelance") job or was self-employed since the last interview |
| Earned an allowance | =1 if respondent received an allowance from family in the previous year |
| School dropout eligible | =1 if respondent is eligible to drop out of school under state statute |
| Sexual consent eligible | =1 if respondent is eligible to consent to sexual intercourse under state statute |
| Unrestricted driving eligible | =1 if respondent is eligible to drive with no restrictions (e.g., passenger restrictions, driving curfew) under state statute |
| <i>State-level control variables</i> | |
| Gross domestic product | State gross domestic product. This variable is logged and in 2000 dollars |
| Income per capita | Per capita personal income. This variable is logged and in 2000 dollars |
| Transfer payments | Total benefits disbursed for public assistance medical care (Medicaid), supplemental security income (SSI), family assistance (AFDC/TANF), food stamps (WIC), and unemployment insurance (UI). This variable is logged and in 2000 dollars |
| Total employment | Total number of full- and part-time jobs, in tens of millions |
| Percent retail industry | Percentage of total employment in the retail industry |
| Percent service industry | Percentage of total employment in the service industry |
| Youth labor supply | Resident population of 18–24 year olds, in tens of thousands |
| Unemployment rate | Percentage of individuals not employed but participating in the labor force |
| Female L.F.P. rate | Percentage of females participating in the labor force |
| Union membership | Percentage of workers in labor unions |
| School enrollment rate | Percentage of 15–17 year olds enrolled in school |
| Per pupil expenditure | Average per pupil expenditure in average daily attendance. This variable is logged and in 2000 dollars |
| Index crime rate | Number of index crimes, per 100,000 population |

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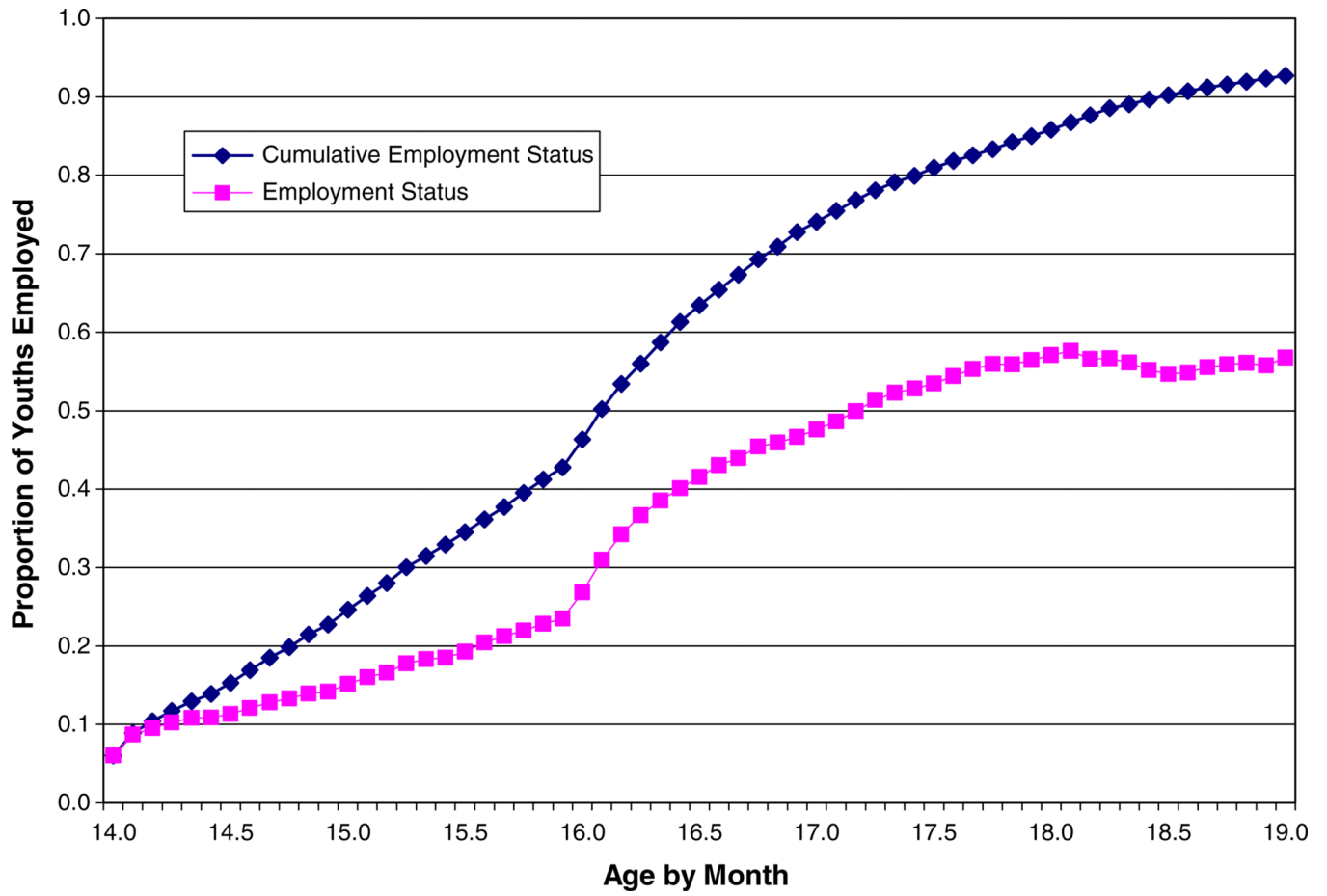


Fig. 1. Monthly employment probabilities from age 14 to age 19. *Note:* $N = 8,984$. Estimates are weighted to provide generalizability to the population of all youths who were 12–16 years of age at yearend 1996. *Source:* National Longitudinal Survey of Youth 1997, rounds 1–7

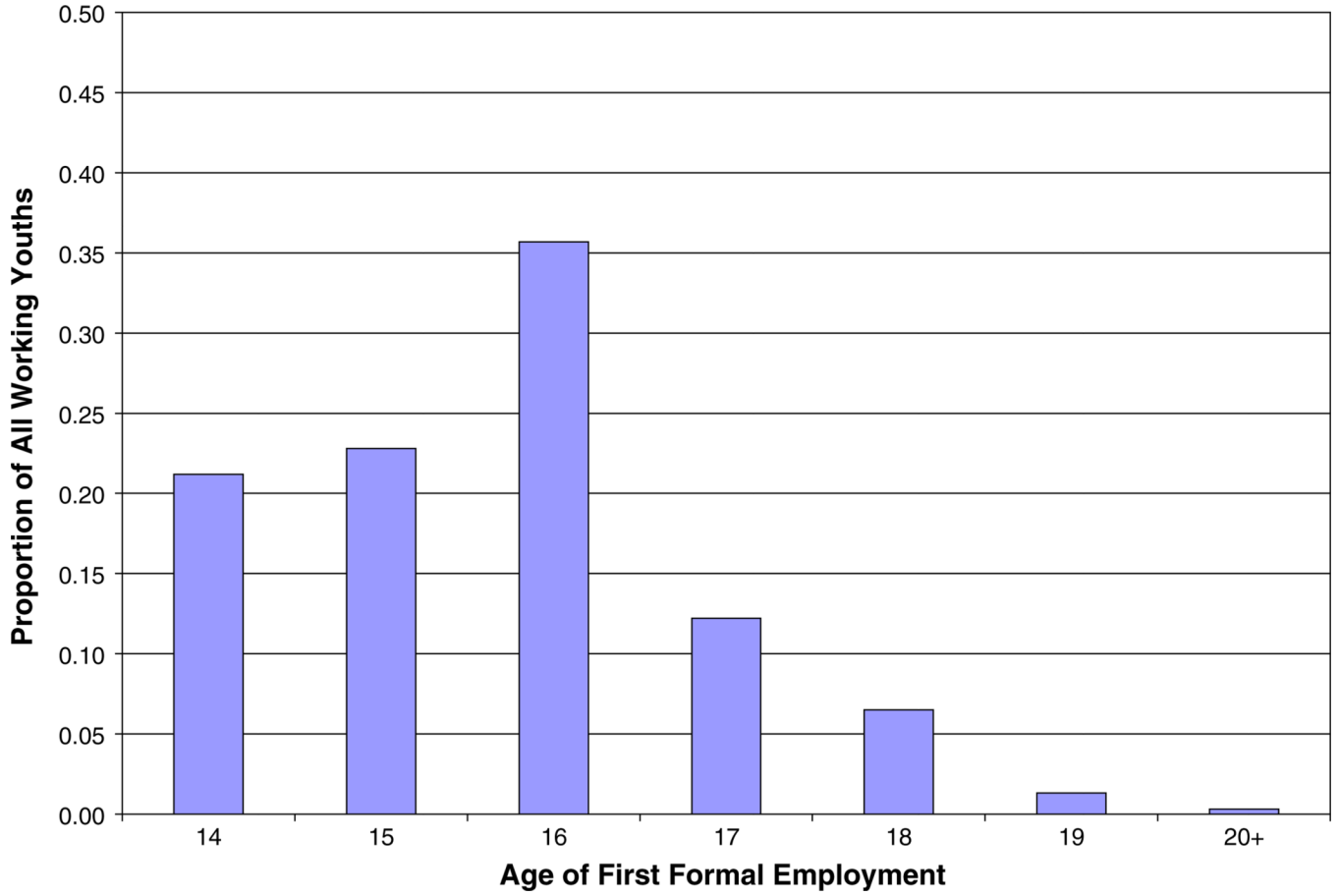


Fig. 2. Age distribution of first formal job. *Note:* $N = 7,552$. Estimates are weighted to provide generalizability to the population of all youths who were 12–16 years of age at yearend 1996. *Source:* National Longitudinal Survey of Youth 1997, rounds 1–7

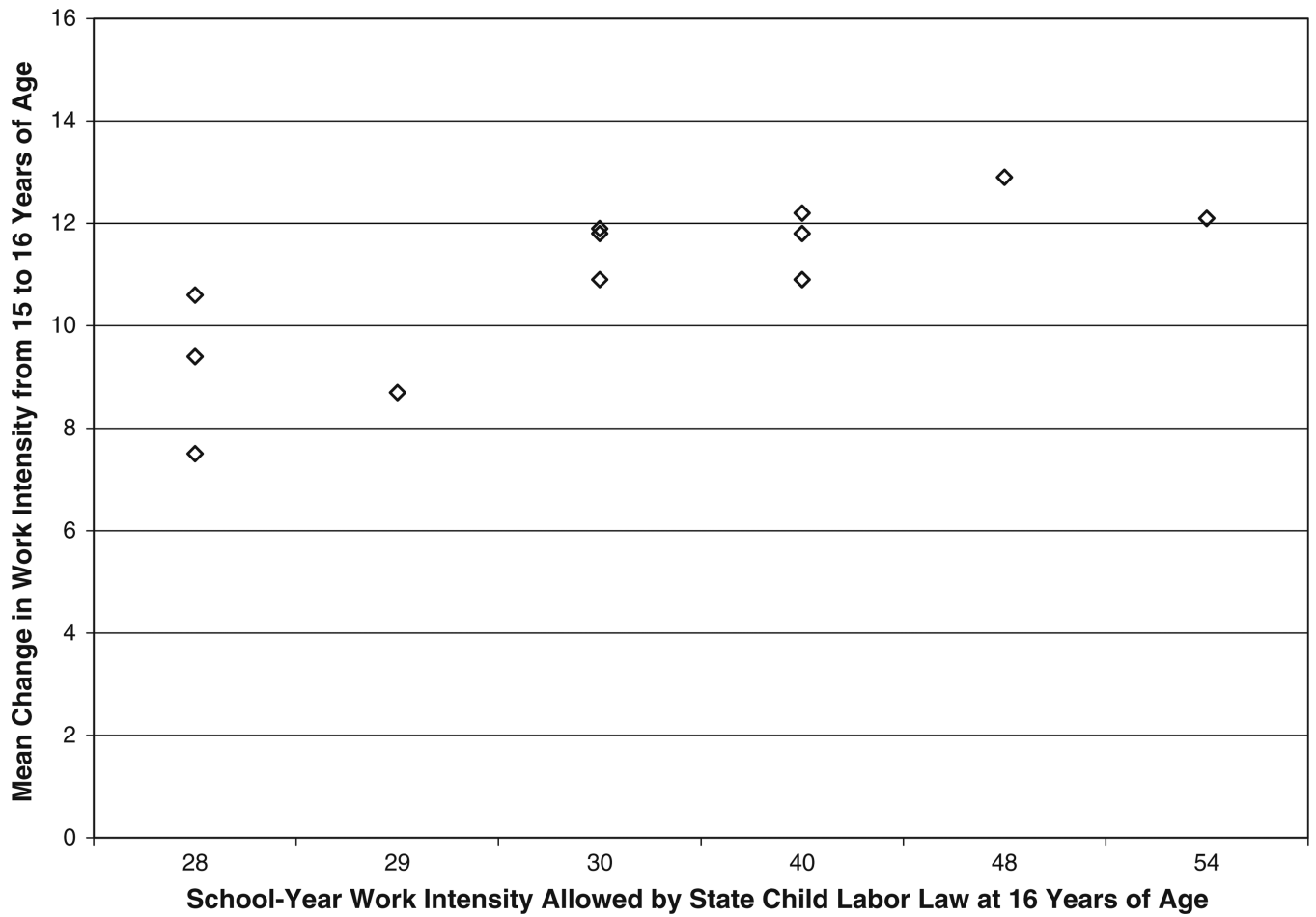


Fig. 3. Mean within-individual change in work intensity at the 15-to-16 transition by change in weekly hours restrictions under state child labor laws. *Note:* Estimates are unweighted. This figure is limited to youth who were not employed while age 15. Open diamonds represent the aggregate of responding youth from individual states. The figure is limited to states with at least 10 responding youth

Table 1

Descriptive statistics

| Variables | Min | Max | Valid N | Age 15 Descriptives | Age 16 Descriptives | % Change |
|-------------------------------|-----|-----|---------|---------------------|---------------------|----------|
| Formal employment | 0 | 1 | 2,224 | 0.0% | 47.8% | 47.8% |
| Work intensity | 0 | 60 | 2,224 | 0.00 (0.00) | 10.42 (13.46) | 47.8% |
| Work intensity if employed | 1 | 60 | 1,063 | - | 21.87 (11.40) | 100.0% |
| Delinquent behavior (variety) | 0 | 7 | 2,207 | 0.56 (1.07) | 0.44 (0.97) | 34.4% |
| Arrest (prevalence) | 0 | 1 | 2,199 | 6.4% | 6.2% | 8.1% |
| Substance use (variety) | 0 | 3 | 2,203 | 0.88 (1.07) | 0.95 (1.08) | 41.7% |
| School suspension (frequency) | 0 | 30 | 1,905 | 0.81 (3.31) | 0.61 (3.13) | 18.3% |
| Transcript grades | 0 | 4.0 | 1,264 | 2.43 (1.00) | 2.41 (0.99) | 95.7% |
| School dropout (prevalence) | 0 | 1 | 2,217 | 3.1% | 6.9% | 8.3% |

Note: School suspensions are treated as invalid if a respondent reports not being enrolled in school at any point during the relevant transition. School dropout is not an absorbing state; in other words, a respondent may be classified as a dropout in one time period but return to school the next. The column headed by “% change” represents the percent of the sample that changes value on the variable of interest

Table 2

First-stage, fixed-effects models of work intensity at the 15-to-16 transition

| | Model 1 | Model 2 | Model 3 | Model 4 |
|----------------------------------|----------------------------|----------------------------|----------------------------|----------------------------|
| <i>State child labor laws</i> | | | | |
| Hours per week | .317 (.049)*** | | | .019 (.073) |
| Hours per weekday | | 1.193 (.186)*** | | .310 (.260) |
| Work curfew | | | 2.188 (.266)*** | 1.813 (.387)*** |
| No hours per week limit | 11.426 (1.58)*** | | | -.592 (3.31) |
| No hours per weekday limit | | 9.371 (1.25)*** | | 3.378 (2.31) |
| No work curfew limit | | | 23.831 (2.66)*** | 19.750 (4.28)*** |
| <i>Individual-level controls</i> | | | | |
| Residential Location: | | | | |
| Central City (ref.) | | | | |
| Suburbs | -.207 (1.80) | -.287 (1.80) | -.181 (1.79) | -.179 (1.79) |
| Rural | -2.947 (3.97) | -3.375 (3.96) | -3.065 (3.94) | -3.018 (3.94) |
| Dwelling type | | | | |
| House, condo, or farm | -1.176 (1.28) | -.834 (1.28) | -.886 (1.27) | -.853 (1.28) |
| Apartment or flat | -1.573 (1.52) | -1.291 (1.52) | -1.315 (1.51) | -1.279 (1.52) |
| Other dwelling (ref.) | | | | |
| Residential mobility | 7.877 (3.39)* | 7.951 (3.39)* | 9.048 (3.38)** | 8.958 (3.38)** |
| Household size | .144 (.287) | .130 (.287) | .106 (.285) | .109 (.285) |
| Highest grade attended | .719 (.538) | .961 (.521) [†] | -.305 (.565) | -.276 (.567) |
| Reached puberty | .824 (.872) | .906 (.871) | .600 (.867) | .619 (.867) |
| Have driver's license | 3.600 (.632)*** | 3.326 (.633)*** | 3.409 (.626)*** | 3.358 (.631)*** |
| Worked in an informal job | -1.833 (.587)** | -1.824 (.586)** | -1.724 (.583)** | -1.706 (.583)** |
| Earned an allowance | -1.229 (.539)* | -1.150 (.539)* | -1.132 (.535)* | -1.123 (.536)* |
| School dropout eligible | 1.508 (.634)* | 1.696 (.631)** | 1.259 (.619)* | 1.189 (.652) [†] |
| Sexual consent eligible | -.238 (.657) | -.951 (.694) | -1.238 (.703) [†] | -1.387 (.755) [†] |
| Unrestricted driving eligible | .469 (1.07) | .319 (1.08) | -.048 (1.08) | -.065 (1.09) |
| <i>State-level controls</i> | | | | |
| Gross domestic product | 29.159 (15.8) [†] | 28.668 (15.8) [†] | 34.965 (15.7)* | 34.892 (15.8)* |
| Income per capita | -2.012 (24.1) | 20.157 (23.7) | 4.541 (23.6) | 5.138 (24.0) |
| Transfer payments | 2.905 (6.27) | 1.318 (6.24) | 3.907 (6.25) | 3.204 (6.27) |
| Total employment | 6.158 (40.1) | -15.527 (40.4) | -15.679 (40.3) | -20.939 (40.6) |
| Percent retail industry | -.036 (.217) | .165 (.215) | .042 (.214) | .054 (.218) |
| Percent service industry | .216 (.137) | .204 (.137) | .155 (.136) | .176 (.142) |
| Youth labor supply | -.007 (.082) | .044 (.083) | -.109 (.084) | -.076 (.087) |
| Unemployment rate | -1.437 (1.03) | -1.723 (1.03) [†] | -.986 (1.01) | -1.121 (1.03) |
| Female L.F.P. rate | .159 (.377) | .028 (.373) | -.086 (.370) | -.044 (.380) |

| | Model 1 | Model 2 | Model 3 | Model 4 |
|--|--------------|--------------------------|--------------|--------------|
| Union membership | .523 (.464) | .801 (.462) [†] | .514 (.461) | .588 (.466) |
| School enrollment rate | -.048 (.201) | -.031 (.200) | -.094 (.200) | -.089 (.201) |
| Per pupil expenditure | -.363 (1.91) | .622 (1.91) | .082 (1.91) | .199 (1.92) |
| Index crime rate | .002 (.002) | .001 (.002) | .001 (.002) | .001 (.002) |
| df | 2,193 | 2,193 | 2,193 | 2,189 |
| R-square | .4002 | .4013 | .4085 | .4093 |
| ΔR -square including instruments | .0143 | .0154 | .0226 | .0234 |
| Partial R-square for instruments | .0233 | .0252 | .0368 | .0382 |
| F-test for instruments | 26.20 | 28.32 | 41.95 | 14.47 |
| Approximate relative bias | .0000 | .0000 | .0000 | .0450 |

Note: $N = 2,224$. $NT = 4,448$. Models also include dummy indicators for control variables with missing data

[†] $p < .10$;

* $p < .05$;

** $p < .01$;

*** $p < .001$ (two-tailed tests)

Table 3

Comparative models of deviant behavior and academic achievement at the 15-to-16 transition

| Dependent variable | N | Panel A: structural coefficients for work intensity | | |
|---------------------|-------|---|-----------------------------|-------------------------|
| | | (1) Random effects | (2) Fixed effects | (3) Fixed effects IV |
| Delinquent behavior | 2,207 | .0034 (.0014)* | .0000 (.0017) | .0233 (.0089)** |
| Arrest | 2,199 | .0007 (.0004)* | -.0006 (.0005) | -.0000 (.0023) |
| Substance use | 2,203 | .0070 (.0013)*** | .0034 (.0014)* | .0067 (.0073) |
| School suspension | 1,905 | .0185 (.0051)*** | .0005 (.0064) | -.0765 (.0372)* |
| Transcript grades | 1,264 | -.0040 (.0012)*** | -.0020 (.0012) [†] | -.0073 (.0069) |
| School dropout | 2,217 | .0021 (.0003)*** | .0017 (.0004)*** | .0109 (.0025)*** |

| Dependent variable (model) | N | Panel B: reduced-form coefficients for state child labor law | | |
|-------------------------------|-------|--|-----------------------------|-----------------------------|
| | | (1) Hours per week | (2) Hours per weekday | (3) Work curfew |
| Delinquent behavior (poisson) | 760 | -.0150 (.0088) [†] | -.0561 (.0329) [†] | -.1217 (.0460)** |
| Arrest (logit) | 178 | -.0092 (.0274) | -.1226 (.1220) | .1110 (.1500) |
| Substance use (poisson) | 918 | -.0027 (.0074) | .0197 (.0293) | .0121 (.0400) |
| School suspension (poisson) | 349 | -.0563 (.0113)*** | -.2514 (.0334)*** | -.2593 (.0454)*** |
| Transcript grades (OLS) | 1,263 | -.0013 (.0031) | .0025 (.0111) | -.0303 (.0176) [†] |
| School dropout (logit) | 184 | .0835 (.0449) [†] | .2227 (.1625) | .3984 (.1941)* |

Note: The first stage for the instrumental variables models is specified as in Model 4 of Table 2. Coefficients for control variables are not shown to conserve space. The models in Panel B are all fixed-effects models and are limited to individuals whose value on the dependent variable changes over time. Separate models are estimated for each of the state child labor laws, and each includes a dummy variable for no restriction although only the linear coefficient is shown

[†] $p < .10$;

* $p < .05$;

** $p < .01$;

*** $p < .001$ (two-tailed tests)