

ABSTRACT

Title of dissertation: DISENTANGLING SELECTION FROM CAUSATION
IN THE EMPIRICAL ASSOCIATION BETWEEN
CRIME AND ADOLESCENT WORK

Robert John Apel, Doctor of Philosophy, 2004

Dissertation directed by: Professor Raymond Paternoster
Department of Criminology and Criminal Justice

Researchers consistently find that youths who work longer hours during high school tend to have higher rates of crime and substance use. On the basis of this and other research showing the negative developmental impact of an “intensive” work commitment during high school, the National Research Council (1998) recommended that federal lawmakers place limits on the maximum number of hours per week that teenagers are allowed to work during the school year. However, recent empirical research demonstrates the possibility of severe bias due to failure to control for unobserved sources of heterogeneity.

I take advantage of two unique characteristics of the National Longitudinal Survey of Youth 1997 to assess the veracity of the claim that longer work hours are causally related to elevated involvement in crime and substance use. First, since the same respondents are followed over a period of five years, I use individual fixed effects to adjust for the omission of relevant time-stable covariates. Second, I exploit state-to-state

variation in the restrictiveness of child labor laws governing the number of hours per week allowed during the school year, and the fact that these restrictions are relaxed (and eventually expire) with increasing age. In this model—based on a fixed-effects instrumental variables (FEIV) estimator—identification of the “work intensity effect” on problem behavior is predicated on exogenous within-individual variation in school-year work hours attributable to the easing of child labor restrictions as youths age out of their legal status as minors. The attractiveness of the FEIV estimator is its ability to eliminate bias in the estimated “work intensity effect” due to omitted stable and dynamic variables. The model thus provides an especially powerful test of the thesis that intensive employment during the school year causally aggravates involvement in problem behavior.

The empirical results demonstrate that longer work hours are associated with a significant *decrease* in adolescent crime, contrary to virtually all prior research. The results for adolescent substance use are mixed, suggesting the possibility that longer work hours either increase or have no effect on substance use, depending on whether a fixed-effects or first-differences procedure is implemented.

DISENTANGLING SELECTION FROM CAUSATION IN THE EMPIRICAL
ASSOCIATION BETWEEN CRIME AND ADOLESCENT WORK

by

Robert John Apel

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2004

Advisory Committee:

Professor Raymond Paternoster, Chair
Professor Shawn Bushway
Professor Laura Dugan
Professor John Laub
Professor Seth Sanders

DEDICATION

This dissertation is dedicated to my good friend and fellow student of “the Way,”

Shane D. Ford (1960-2003).

I have no doubt that the first thing he did upon arriving at the Golden Gates was to challenge St. Peter to a football throwing contest. His courage, selflessness, integrity, and wit were unparalleled. He will be sorely missed.

ACKNOWLEDGMENTS

As always, I have to recognize and thank my family for their endless support of my “lifetime student” pursuits. They have never questioned the career decisions I have made; only offered their help along the way. However, if anyone is more delighted than I am about reaching the end of my graduate career, I am sure it is them. Maybe now they will have the money to take that dream vacation they have always talked about! Aunt Chris and “The Dude” have provided a home away from home for the last six years, and I will be forever grateful to them for it. I accomplished several firsts in their company: my first crab-picking experience, my first West Virginia hot dog, etc. I will miss our shrimp nights in Pigtown; I doubt I will ever be able to find another hole in the wall as cosmopolitan as J. Ray’s Café.

I must also thank each of the individuals on my dissertation committee. People often wince when I list my committee members, but I purposely selected individuals who have shaped my academic career, and who, in my humble opinion, are the best and brightest in the field. Ray Paternoster and Shawn Bushway—co-founding members, along with Bobby Brame, of the so-called “Paternoster Mafia”—gave me my first research opportunity. They invested a lot of time in teaching me the skills to be a methodical researcher. This dissertation is as much a product of their work as it is mine, because many of the ideas were hammered out in idea sessions with them. I am especially grateful to Ray for being my advisor and mentor for the last six years. He has been very influential in directing me onto the career path that I am now on, and his talents as a scholar serve as a model for my own career. I found myself navigating uncharted econometric territory as I was writing this dissertation, and Shawn was often the first

person that I consulted when I was uncertain about the next step. He has an intuitive feel for research that I greatly respect and admire.

For the last three years, I have had the pleasure of working closely with Laura Dugan. She has broadened my criminological (and musical) interests and provided wise counsel on numerous occasions. I am thankful that I have had the chance to learn from her. John H. Laub (“El Presidente”) has had an enormous influence on my intellectual development, perhaps more so than he realizes. Indeed, I often ask myself, “W.W.J.D.?” (“What Would John Do?”). In all seriousness, John is one of the hardest-working scholars I know, and he has a criminological “sixth sense” that I hold in very high regard. Seth Sanders rounds out the “Carnegie Mellon Trifecta” that I sought for my committee (with Shawn and Laura accounting for the other two slots); he bravely agreed to serve on my committee before having met me or knowing anything about my dissertation topic. He has since been instrumental in pushing Shawn and me to develop these ideas further and to pursue external sources of funding in order to do so.

I would be remiss if I did not recognize the undergraduate professors from Drury College (now Drury University) that also played an influential role in my criminological trajectory—A.L. Marsteller, Hooshang Pazaki, and Alex del Carmen. It was Marsteller’s enthusiasm for criminology (with the help of his perpetual “chalkboard dashes”) that got me hooked on the subject during my very first semester.

I have several friends in the department whom I will miss as we all steadily move on—Carlos Rocha, Darin Reedy, Elaine Eggleston, Dave Bierie, Gary Sweeten, Nancy Morris, and Lee Slocum. I look forward to our annual reunions at ASC. I will especially miss the regular jam sessions with Carlos, Dave, and Gary; it just won’t be the same

blowing on the harp in front of my dog. Carole Gibbs has kept me grounded, and I am thankful for her companionship.

TABLE OF CONTENTS

List of Tables	viii
List of Figures	x
Chapter One: Introduction	1
Empirical Research on the Association between Adolescent Employment and Problem Behavior	5
Original Data Collection Efforts	6
Secondary Data Analyses	8
Summary of Empirical Evidence	10
The Identification Problem and Causal Inference	11
Tyler’s (2003) Instrumental Variables Design	16
Paternoster et al.’s (2003) Fixed-Effects Design	17
A Replication and Methodological Extension	19
Summing Up and Looking Ahead	21
Chapter Two: Literature Review	23
The Nature of Contemporary Adolescent Employment	24
Employment Status	25
Work Intensity	27
Age of First Employment	29
Industry and Occupation	30
Adolescent Work Intensity, Developmental Research, and Criminological Theory	33
Causal Mechanisms for the Adverse Work Intensity Effect	35
Social Control Theory	35
General Strain Theory	38
Learning Theory	41
Routine Activity Theory	43
Spurious Mechanisms for the Adverse Work Intensity Effect	45
Precocious Development Theory	47
Propensity Theory	50
Race, the Urban Underclass, and the Potential Benefits of Intensive Youth Employment	52
Integration of Existing Research from a Life-Course Perspective	58
The Principle of Context	58
The Principle of Timing	61
The Principle of Interdependency	65
The Principle of Agency	68
Summary	69
Chapter Three: Data and Methods	70
Key Measures	71
Problem Behavior	71

Formal Work Hours during the School Year	73
Individual Control Variables	75
County Control Variables	76
Econometric Motivation	77
Instrumental Variables (IV) Estimator	79
Fixed-Effects (FE) Estimator	81
The Fixed-Effects Instrumental Variables (FEIV) Estimator	82
Instruments for Formal Work Hours during the School Year	85
Missing Data and Variance Adjustment	87
 Chapter Four: Results	 91
Evaluating the Validity of State Child Labor Laws as Instruments for School-Year Work Hours	 92
First-Stage Models	94
Annual Crime Models	98
Random Effects	101
Fixed Effects	103
Fixed Effects Instrumental Variables	105
Summary	108
Recent Substance Use Models	109
Random Effects	110
Fixed Effects	111
Fixed Effects Instrumental Variables	112
Summary	114
Sensitivity Analysis	115
Behavior-Specific Models	115
Alternative Model Specification	119
Estimation in First Differences	122
 Chapter Five: Discussion and Conclusion	 130
Youth Employment and Problem Behavior	132
School-Year Work Intensity and Crime	132
School-Year Work Intensity and Substance Use	135
Potential Threats to Causal Inference	137
Youth Employment and Criminological Theory	140
Selection into the Youth Labor Market	142
The Causal Impact of Investment in Work	146
Youth Employment and Public Policy	148
Future Research	151
Conclusion	155
 Appendix	 199
 References	 213

LIST OF TABLES

1. Comparison of Employment Estimates from the National Longitudinal Survey of Youth 1997 with Existing Surveys of High-School Youths 157
2. Descriptive Statistics Pooled across Five Waves ($N = 8,724$, $NT = 40,283$) 158
3. Problem Behavior Indicators Used in Scale Construction 159
4. Descriptive Statistics for Problem Behavior and Work Intensity, by Interview Year 160
5. Instrumental Variables for School-Year Work Hours in a Formal Job, as of January 1, 1997 161
6. Proportion of NLSY Respondents Subject to State Child Labor Laws, by Interview Year 162
7. First-Stage, Fixed-Effects Models of Annual School-Year Work Intensity 163
8. First-Stage, Fixed-Effects Models of Recent School-Year Work Intensity 164
9. Random-Effects Models of the Effect of School-Year Work Intensity on Annual Crime 165
10. Fixed-Effects Models of the Effect of School-Year Work Intensity on Annual Crime 166
11. Fixed-Effects Instrumental Variables Models of the Effect of School-Year Work Intensity on Annual Crime 167
12. Random-Effects Models of the Effect of School-Year Work Intensity on Recent Substance Use 168
13. Fixed-Effects Models of the Effect of School-Year Work Intensity on Recent Substance Use 169
14. Fixed-Effects Instrumental Variables Models of the Effect of School-Year Work Intensity on Recent Substance Use 170
15. Summary of Fixed-Effects Instrumental Variables Estimates of the Effect of School-Year Work Intensity on Individual Problem Behaviors 171
16. Fixed-Effects Models of the Effect of School-Year Work Intensity on the Frequency of Smoking Cigarettes and Drinking Alcohol 172

17. Summary of Fixed-Effects Instrumental Variables Estimates of the Effect of School-Year Work Intensity on Problem Behavior with Alternative Model Specifications	173
18. First-Differences Models of the Effect of School-Year Work Intensity on the Frequency of Smoking Cigarettes and Drinking Alcohol	174

LIST OF FIGURES

1. Proportion of Youths Employed in Formal Jobs, by Month during High School 175

2. Continuous Mean Weekly Hours of Formal Employment, by Month during High School 176

3. Categorical Mean Weekly Hours of Formal Employment, by Year of High School 177

4. Proportion of Enrolled Youths Employed in Formal Work, by Age, 1940-2000 178

5. Proportion of Youths Employed in Formal and Informal Jobs, by Age 179

6. Continuous Mean Weekly Hours of Formal Employment, by Age 180

7. Categorical Mean Weekly Hours of Formal Employment, by Age 181

8. Age of First Employment in Formal and Informal Jobs 182

9. Share of All First Formal Jobs in Specific Industries, by Age 183

10. Share of All First Formal Jobs in Specific Industries, by Gender and Race 184

11. Share of All First Formal Jobs in Specific Occupations, by Age 185

12. Share of All First Formal Jobs in Specific Occupations, by Gender and Race 186

13. Share of All First Informal Jobs in Specific Occupations, by Age 187

14. Share of All First Informal Jobs in Specific Occupations, by Gender and Race 188

15. Diagram of Potential Mediating Causal Mechanisms to Explain the Positive Correlation between Intensive Work and Problem Behavior during Adolescence 189

16. Diagram of a Spurious Positive Correlation between Intensive Work and Problem Behavior during Adolescence 190

17. Problem Behavior in the NLSY Estimation Sample, by Age 191

18. School-Year Work Intensity in the NLSY Estimation Sample, by Age 192

19. Diagram of the Logic of Instrumental Variables Estimation	193
20. Annual Crime (Variety) among NLSY Youths, by School-Year Work Hours	194
21. Recent Substance Use (Variety) among NLSY Youths, by School-Year Work Hours	195
22. Problem Behavior among NLSY Youths, by Categorical Work Intensity	196
23. Mean School-Year Work Hours by Change in Weekly Hours Restrictions from 15 to 16 Years of Age	197
24. Mean Change in School-Year Work Hours by Change in Weekly Hours Restrictions from 15 to 16 Years of Age	198

CHAPTER ONE: INTRODUCTION

The labor market is a major life domain among youths enrolled in high school. The ubiquity of high-school employment is illustrated in Figures 1, 2, and 3 using a nationally representative sample of American youths. These data are from the first three waves of the National Longitudinal Survey of Youth 1997, and are generalizable to the population of contemporary youth cohorts. Figure 1 provides the employment rate and cumulative employment rate by month during the four years of high school. Averaging over the nine months of their freshman year of high school, 13 percent of youths are employed at least once during the academic year, but this figure increases to 25 percent during the sophomore year, 44 percent during the junior year, and 56 percent during the senior year. Over the course of these four academic years, 38 percent of youths work during any given school month, on average. There is also a very clear seasonal pattern to youth employment, as summer months are consistently associated with a 10-point increase in the proportion of youths employed (May-to-July comparison).¹ A consideration of the cumulative employment probabilities further illustrates the dynamic nature of the youth labor market. Fully 87 percent of students have ever worked during high school by May of their senior year, while only 58 percent are actually employed during this month. Thus, less than two-thirds (60%) of youths who have ever worked by the end of their senior year are actually employed during this period.

*** Figure 1 Here ***

¹ Notice also that summer work may be “sticky,” in that not all youths who acquire summer jobs leave them once the school year commences. Thus, summer work may constitute one gateway to the acquisition of more long-term employment. Rothstein (2001) and Oettinger (2000) offer similar descriptions of summer work.

The fact is that almost all youths gain some employment experience during high school. This observation is consistent with past youth employment research that finds cumulative employment rates of 80 percent and as high as 90 percent (Marsh, 1991; National Research Council, 1998; Steinberg and Cauffman, 1995). In addition to the fact that most high-school youths work, prior research suggests that a non-trivial proportion of employed students work at “high intensity”—a label applied by prior researchers to denote employment of over 20 hours per week. Figure 2 provides mean weekly hours of work by month among employed youths in the National Longitudinal Survey of Youth 1997, showing that this 20-hour benchmark is surpassed by the spring (March) of the sophomore year of high school. The average work commitment continues to increase to almost 30 hours per week by May of the senior year. Notice also that the “summer effect” on work intensity tends to become less pronounced over the high-school years. Figure 3 groups work intensity into five-hour intervals (1-5, 6-10, 11-15, etc.) for further comparison. Notice that during their freshman year, three of every four employed youths (76%) work 20 or fewer hours per week, or what is commonly referred to as “low-intensity” work. This percentage declines steadily to well under half of employed youths by the senior year (45%). Almost one in four employed seniors (23%) is working full time at over 35 hours per week. By all appearances, then, by the time they graduate from high school, many youths are fully attached to the labor force.

*** Figures 2 and 3 Here ***

Folk wisdom suggests that employment during the school year provides a number of positive benefits for adolescents, and prior to the early 1980s, educators, policy makers, and laypersons were united in the belief that adolescents benefit from exposure

to the world of adult work. As the thinking went, working structures a youth's leisure time, increases exposure to adult authority figures, fosters independence and maturity, teaches responsibility in the use of money, and promotes balancing of multiple responsibilities. In fact, a series of "blue ribbon" commissions at the time strongly encouraged greater work experience among students for precisely these reasons (Carnegie Council on Policy Studies in Higher Education, 1979; National Commission on the Reform of Secondary Education, 1973; National Commission on Youth, 1980; National Panel on High School and Adolescent Education, 1976; Panel on Youth of the President's Science Advisory Committee, 1974).² The statements of one such panel are representative:

The Panel urges the removal of those regulations...that handicap and limit the employment of adolescents.... With half the high school group already in the work force the Panel's suggestions seek to facilitate the growth of employment opportunities for still more young people as a necessary adjunct to formal education. (National Panel on High School and Adolescent Education, 1976:11)

Of particular interest is the fact that one panel—the Carnegie Council on Policy Studies in Higher Education (1979)—encouraged greater work experience on the grounds that *such experience would reduce crime and delinquency*.³

² The overriding concern of virtually all of these panels was finding a more effective way of bridging the gap between adolescence and adulthood at a time when society was undergoing rapid social changes, evidenced by high levels of youth unemployment, crime and drug use, high-school dropout, and suicide. The fear was that some groups of young people were not adequately prepared to compete successfully in adult institutions, mired as they were in a school-based "youth culture" cut off from other traditional socializing institutions of the family, school, workplace, and community. The emphasis was thus on minimizing the amount of time that youths spent in a period of transition: "The bridge of time between youth and adulthood has become a bridge too long" (National Commission on Youth, 1980:9). Although I focus on the labor market recommendations of these commissions, the truth is that the proposed solutions were manifold, requiring collective effort on the part of schools, employers, communities, and public officials.

³ Consider the following: "How do young people who are neither in school nor in the labor market, or who are unemployed over long periods of time, support themselves? Clearly, crime plays a role" (Carnegie Council on Policy Studies in Higher Education, 1979:65).

Ruhm (1995:293), however, explains that the presumed benefits of employment were taken for granted, and that these early panel recommendations were “made in the absence of hard empirical evidence that increased job-holding caused or even was correlated with favorable outcomes.” It thus came as a surprise when an extensive empirical literature consistently found that youths that work during high school also tend to engage in higher rates of minor delinquency (e.g., theft, vandalism, phony ID), serious delinquency (e.g., Index crime, interpersonal aggression, assault), substance use (e.g., cigarettes, alcohol, marijuana, cocaine), and school misconduct (e.g., truancy, cheating, plagiarism, detention, suspension), and to have higher rates of police contact (e.g., trouble with police, arrest).⁴ Particularly influential was a book by Greenberger and Steinberg (1986) that drew lay attention to the manifold adverse correlates of youth employment, not the least of which was elevated involvement in problem behavior. On the basis of this and other research, just 20 years after the first round of panel recommendations that touted the benefits of adolescent employment, the prestigious National Research Council (1998:227) was compelled to propose limits on the extensiveness of the youth work commitment:

The Department of Labor should be authorized by Congress to adopt a standard limiting the weekly maximum number of hours of work for 16- and 17-year-olds during the school year. This standard should be based on the extensive research about the adverse effects of high-intensity work while school is in session.

⁴ In fact, over 30 published studies—at least 20 in peer-reviewed journals—provide empirical support for the proposition that employment contributes to adolescent problem behavior. For exceptional reviews of the research on problem behavior and other outcomes, I refer the reader to Greenberg and Steinberg (1986), Mortimer (2003), the National Research Council (1998), and Steinberg and Cauffman (1995).

Notice that the suggested policy change was focused squarely on the number of hours that youths spend in the workplace.⁵ The implication of the panel's proposal was that high work intensity while enrolled in high school is a direct or indirect cause of elevated levels of adolescent problem behavior. Although the empirical research reviewed by the National Research Council (1998) leaves no doubt about the *presence* of this positive correlation between intensive employment and antisocial behavior, there remains considerable ambiguity about its *causal significance*. In the next section, I review the empirical literature responsible for this scholarly "about-face." I then evaluate the strength of the evidence for interpreting this adverse "work intensity effect" as causal.

EMPIRICAL RESEARCH ON THE ASSOCIATION BETWEEN ADOLESCENT EMPLOYMENT AND PROBLEM BEHAVIOR

Some of the earliest evidence for a relationship between adolescent employment and problem behavior can be found in the classic criminological studies.⁶ Glueck and Glueck (1950), for example, found that official delinquency among Boston-area 10-17 year olds was related to after-school employment, especially daily employment and work in the street trade. Nye (1958) found that the "most delinquent" boys in his school sample earned money outside of the family during the school year (there was no relationship for

⁵ The panel speculated that a 20-hours-per-week cutoff would be appropriate for 16 and 17 year olds, although it recognized that some exceptions should be allowed:

On balance, in the judgment of the committee, the scientific evidence supports increased restrictions on the intensity of work by children and adolescents. In keeping with its guiding principles and the research evidence, the committee believes that limiting the hours of work for most 16- and 17-year-olds during the school year is essential to their healthy development. . . . Following the majority of the evidence to date, and the conventional cutoff used in many studies, the committee strongly supports a limit of 20 hours of work per week during the school year for adolescents under most circumstances. (National Research Council, 1998:226)

⁶ I follow Jessor and Jessor (1977:33) and define problem behavior as "behavior that is socially defined as a problem, a source of concern, or as undesirable by the norms of conventional society and the institutions of adult authority, and its occurrence usually elicits some kind of social control response."

girls). Hirschi (1969) found that in his school sample of white males, those who were currently working for pay engaged in a wider variety of delinquent acts (hours of employment were unrelated to delinquency).

Original Data Collection Efforts

Beginning in the early 1980s, the developmental consequences of adolescent employment gained the sustained attention of researchers. This increased attention led to the creation of several original longitudinal surveys of high-school students from geographically localized samples, from which the most influential research on youth employment to date has been conducted. Greenberger et al. (1981), for example, conducted the first systematic study of the effects of adolescent employment on problem behavior among a sample of 10th and 11th graders in Orange County, California, high schools. They found that work status (a binary measure for working vs. not working) was generally unrelated to substance use, although weekly earnings and time spent in the workplace (the product of hours per week and length of employment) were consistent predictors of higher levels of substance use, particularly excessive alcohol use and marijuana use, among both males and females. Steinberg et al. (1982) followed up the non-workers from this study, and found that youths spending more time in the workplace one year later had a higher risk of cigarette and marijuana use than youths who remained non-workers.

Steinberg and Dornbusch (1991) collected data from high-school students in northern California and Wisconsin, finding that longer work hours (ordinal measure) were associated with higher rates of substance use (cigarettes, alcohol, marijuana, other

drugs), minor delinquency (theft, carrying a weapon, vandalism, using phony ID), and school misconduct (cheating, copying homework). Following up on this sample, Steinberg et al. (1993) reported that, controlling for the level of problem behavior at the first wave, non-workers who entered the labor market one year later at high intensity (over 20 hours) reported higher levels of substance use, minor delinquency, and school misconduct than non-workers who remained out of the labor market. Steinberg and Avenevoli (1998) used structural equation models to examine the relationship between work intensity (ordinal measure) and problem behavior (drug and alcohol use, minor delinquency) over the high-school years. In addition to a positive cross-sectional correlation between the two at all four years, they found that work intensity during one year predicted higher levels of problem behavior the next year.

Mortimer et al. (1996) found that youths from St. Paul, Minnesota, participating in the Youth Development Study who worked intensively (indicator for 21+ hours) were consistently more likely to drink alcohol during each of the last three years of high school, controlling for prior behavior. There was no relationship of intensive work, however, with school problem behavior (got in trouble for breaking rules, sent to principal's office or detention) or smoking, and in fact, moderate work involvement (1-20 hours) tended to be associated with less smoking. Along similar lines, McMorris and Uggen (2000) found that high work intensity (binary and continuous measures) was associated with 9th and 12th grade alcohol use. Even more recently, Mortimer (2003; see also Mortimer and Johnson, 1998) concluded that intensive employment (two binary measures) was positively associated with 12th grade alcohol use, irrespective of whether it was of low duration ("sporadic") or high duration ("most invested"). Staff and Uggen

(2003) again confirmed the adverse effect of work intensity (continuous measure) on 12th grade alcohol use, net of other work characteristics (e.g., hourly pay, work-derived peer status, learning opportunities, autonomy, work stressors, work-school compatibility). They also found that longer work hours predicted elevated involvement in 12th grade school misconduct (got into trouble for breaking rules, sent to principal's office or detention). Research from the Youth Development Study thus conclusively found that "it is with respect to alcohol use that we find the greatest cause for concern about some youth's high involvement in work" (Mortimer et al., 1996:1257).

Secondary Data Analyses

In addition to these original data collection efforts to assess the problem behavior consequences of adolescent employment, numerous other researchers interrogated pre-existing data sources. The results from these more representative samples confirmed the findings from the earlier non-probability samples. For example, Bachman and colleagues (1981, 1986, 2003; Bachman and Schulenberg, 1992, 1993; Safron et al., 2001) pooled data for various years from high-school seniors in the annual Monitoring the Future survey. They consistently found that an ordinal measure of work intensity (measured in 5-hour intervals) was predictive of the use of cigarettes, heavy alcohol, marijuana, and even cocaine. They found similar adverse work intensity effects for theft, interpersonal aggression, and trouble with police. Agnew (1986) used the second wave of the Youth in Transition survey, when the young men were in the 11th grade. He found that the number of hours worked per week (continuous measure) was consistently related to a scale of total delinquency, in addition to its component subscales of interpersonal aggression

(fighting, gang fighting, robbery, aggression against parents) and theft/vandalism (petty and major larceny, shoplifting, trespassing, arson, vandalism), even while controlling for other work characteristics (e.g., hourly pay, skill, satisfaction, length of employment).

Marsh (1991) used data from the sophomore- and senior-year interviews of the High School and Beyond study. He found that an ordinal measure of the number of hours worked per week during high school was related to senior-year school troublesomeness (others see youth as a troublemaker, disciplinary problems, suspended, cut classes, serious trouble with the law), controlling for a wide array of covariates, including sophomore-year school troublesomeness. Wright et al. (1997) examined the relationship between work intensity and problem behavior among a sample of enrolled 12 to 18 year olds in the National Survey of Families and Households. They found that a continuous measure of work intensity significantly increased parent-report problem behavior (school misbehavior, official delinquency, substance use, aggression), controlling for numerous known correlates of delinquency.

Cullen et al. (1997) utilized the subsample of enrolled youths from the fifth wave of the National Youth Survey. They found that the most consistent work characteristic related to delinquency was work intensity (continuous measure), adjusting for wages, job stability, and job changes, in addition to numerous other controls, including prior delinquency. Mihalic and Elliott (1997) also used enrolled youths from the National Youth Survey, finding that, relative to youths who did not work at all during the third or fourth waves or who worked during only one of the two waves, those that worked during both waves had the highest level of alcohol, marijuana, and other drug use. They then selected on third-wave non-workers in order to examine the transition to employment by

the fourth wave, finding that entering the labor market at high intensity (over 20 hours) was associated with the highest level of alcohol and drug use, controlling for prior behavior.

Summary of Empirical Evidence

This lengthy series of findings underscores the idea that work intensity, rather than work experience *per se*, is a crucial distinction in understanding the relationship between youth employment and problem behavior.⁷ The conclusions from major reviews of the literature suggest as such (emphases added):

If there is reason to be concerned about possible deleterious consequences of *extensive employment during the school year*,...it is to be found in studies of working and problem behavior. (Steinberg and Cauffman, 1995:158)

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, 1998:132)

Thus, irrespective of whether work intensity is measured continuously, ordinally, or dichotomously, virtually all prior studies suggest that intensive work during high school is robustly correlated with problem behavior, relative to non-work or work of moderate intensity.⁸ That is, high-school youths who work the longest number of hours per week

⁷ The only other job characteristic as strongly related to problem behavior as work intensity is weekly earnings (Bachman et al., 1981; Bachman and Schulenberg, 1992; Greenberger et al., 1981; Heimer, 1995; Ruggiero, 1984; Wright et al., 2002). Since this itself is a function of work intensity, perhaps a better measure would be wages, but the evidence is mixed, especially when controlling for other work characteristics (for adverse wage effects, see Abe, 1999; Agnew, 1986; Staff and Uggen, 2003; for null wage effects, see Cullen et al., 1997; Tanner and Krahn, 1991; Uggen, 2000a). Research that considers the impact of “resources” on problem behavior, with no control for other work characteristics, finds that having money or desiring large sums of money is a predictor of higher levels of delinquency and drug use (Agnew, 1990, 1994; Cullen et al., 1985). Moreover, the source of this money—whether from a job or allowance—appears to be irrelevant (see Wright et al., 2001).

⁸ Studies that consider gender, race, and gender-race interactions in the “work intensity effect” on problem behavior are comparatively few, but illustrative. There is a fairly clear tendency for work intensity to be more strongly related to substance use and school problems among females (Bachman and

during the school year also have the highest levels of participation in a wide variety of problem behaviors. In fact, the only three studies from this stream of research that found no positive association between employment and adolescent problem behavior ignored work intensity, and instead relied solely on indicators of work status (Good et al., 1986; Gottfredson, 1985; Ploeger, 1997).⁹

THE IDENTIFICATION PROBLEM AND CAUSAL INFERENCE

Robust as the existing findings are across a variety of surveys and a plethora of control variables, there remains considerable uncertainty about whether the adverse work intensity effect on problem behavior is genuinely causal. In the absence of random assignment to employment, any observed correlation between adolescent work and problem behavior could be a consequence of differences across youths in “initial endowments” or “population heterogeneity.” This is the problem of *self-selection*, the idea that working adolescents (or adolescents that work at high intensity) are systematically different with respect to pre-existing (or omitted) characteristics that are

Schulenberg, 1992, 1993; Bachman et al., 1986; Greenberger et al., 1981; Heimer, 1995; Mortimer and Johnson, 1998). There is also a tendency for males to respond more readily to long work hours by acting out in a delinquent manner (Abe, 1999; Bachman and Schulenberg, 1992; Crowley, 1984; Steinberg et al., 1993; Wright et al., 1997). Mihalic and Elliott (1997) are the only researchers to find the opposite tendency; that is, for female work intensity to increase serious delinquency and male work intensity to increase substance use. There are even fewer studies that examine race \times work interactions. There is a tendency for white youths to increase their substance use in response to long work hours (Johnson, forthcoming; Mihalic and Elliott, 1997). The evidence for differential work effects on delinquency suggests that black and Hispanic youths experience larger increases in their delinquency than white youths (Abe, 1999). Other research suggests that non-whites increase minor delinquency, whereas whites increase serious (Index) delinquency (Mihalic and Elliott, 1997).

⁹ There are also other important differences worth highlighting. Gottfredson (1985) relied on an urban, primarily minority sample, whereas virtually all other research relies on suburban, primarily white, samples of youths. Similarly, Good et al. (1986) used a sample of inner-city youths from low-income families who were participating in a crime prevention program, and they in fact found that employment was *negatively* related to police contact. This is the sole study that found that employment might be beneficial for involvement in adolescent problem behavior. Ploeger (1997) found that adolescent work status was robustly related to substance use (alcohol use, marijuana use, public drunkenness)—even while controlling for prior substance use—until he introduced a measure of association with delinquent peers.

correlated with problem behavior. The research on the basis of which the National Research Council (1998) proposed stricter limits on adolescent work involvement has not convincingly ruled out the possibility that youths with a high propensity for problem behavior are precisely those most likely to work at high intensity while in school. Once these pre-existing differences are accounted for, it is entirely plausible that the adverse work intensity effect will disappear (or could even reverse signs; see Brame et al., forthcoming). The implication of this assertion is that prior researchers have mistaken self-selection for the causal impact of working, rendering the correlation between work intensity and problem behavior a spurious one.¹⁰

¹⁰ The problem posed by selection bias is exacerbated by the fact that most prior research is conducted on samples of school-going youths. Following is a list of all known (to this author) empirical studies of youth employment and problem behavior. The first group of studies relies on representative school samples:

- Monitoring the Future (Bachman et al., 1981, 1986, 2003; Bachman and Schulenberg, 1992, 1993; Heimer, 1995; Safron et al., 2001).
- Youth in Transition (Agnew, 1986).
- High School and Beyond (Marsh, 1991).
- National Longitudinal Study of Adolescent Health (Johnson, forthcoming; Resnick et al., 1997).

A second sizable group of studies relies on non-representative school samples:

- Youth Development Study (McMorris and Uggen, 2000; Mortimer, 2003; Mortimer et al., 1996; Mortimer and Johnson, 1998; Staff and Uggen, 2003; Uggen, 2000a).
- The Orange County, California, study (Greenberger et al., 1981; Ruggiero, 1984; Steinberg et al., 1982).
- The California and Wisconsin study (Steinberg and Avenevoli, 1998; Steinberg and Dornbusch, 1991; Steinberg et al., 1993).
- School Action Effectiveness Study (Gottfredson, 1985).
- A sample of Canadian students (Tanner and Krahn, 1991).
- A sample of students in Midwestern towns (Hansen and Jarvis, 2000).
- Maryland Adolescent Development in Context (Bartko and Eccles, 2003).

Other non-school samples of adolescents are studied in specific geographic areas:

- Two birth cohorts (1942, 1949) in Racine, Wisconsin (Shannon, 1982, 1988).
- A sample of inner-city Philadelphia youths (Good et al., 1986).

Only a handful of studies use nationally representative samples of adolescents:

- National Longitudinal Survey of Youth 1979 (Crowley, 1984).
- National Longitudinal Survey of Youth 1997 (Abe, 1999, 2001; Apel et al., 2003; Brame et al., forthcoming; Huang et al., 2001; Paternoster et al., 2003).
- National Youth Survey (Cullen et al., 1997; Mihalic and Elliott, 1997; Ploeger, 1997).
- National Survey of Families and Households (Wright et al., 1997).

Of this latter group of surveys, Crowley (1984), Cullen et al. (1997), Mihalic and Elliott (1997), and Wright et al. (1997) restricted their analyses to those youths that remained enrolled in school. Consequently, only the studies by Abe (1999, 2001), Apel et al. (2003), Brame et al. (forthcoming), Paternoster et al. (2003),

There is indeed reason for exercising caution in causal interpretations of the adverse work effect. For example, youths may enter the labor market in part as a result of weaker emotional attachment to their parents (Gottfredson, 1985; Greenberger et al., 1981). Research has also found that lower scholastic performance, educational expectations, and time spent on homework predict the transition into the world of adolescent work (Bachman and Schulenberg, 1993; Entwisle et al., 1999; Mihalic and Elliott, 1997; Mortimer, 2003; Mortimer and Finch, 1986; Schoenhals et al., 1998; Singh, 1998; Singh and Ozturk, 2000; Steinberg and Avenevoli, 1998; Steinberg et al., 1993). Employed youths are less involved with their family and exercise greater autonomy vis-à-vis their parents before they begin working (Mihalic and Elliott, 1997; Steinberg and Avenevoli, 1998; Steinberg et al., 1993). Youths who work also engage in more delinquency, aggression, substance use, and school misconduct prior to labor market entry, and are more heavily involved with delinquent peers than future non-working youths (Gottfredson, 1985; Mihalic and Elliott, 1997; Mortimer, 2003; Ploeger, 1997; Staff and Uggen, 2003; Steinberg and Avenevoli, 1998; Steinberg et al., 1993). Thus, there is sufficient evidence to warrant serious concern that *school disengagement, emotional and behavioral autonomy, and problem behavior precede the transition into the adolescent labor market.*

The problem of self-selection has not gone unnoticed in existing studies. Indeed, prior research has controlled for an exhaustive list of known correlates of problem behavior, including family background (SES, parental attainment, family income, intact family, household size), residential location (Census region, urban residence, local

and Ploeger (1997) are generalizable to the population of all adolescents (as is Huang et al., 2001, but they examine only bivariate relationships). Notably, three of these latter studies found no evidence for the adverse effect of employment on adolescent problem behavior encountered in previous research.

unemployment), youth-parent interaction (attachment, monitoring, rule making, conflict), school performance (educational goals, homework, grades, test scores, absenteeism), and other selection controls (antisocial peers, low self-control, intelligence, self-concept).¹¹ Numerous researchers have also employed a lagged dependent variable as a proxy for population heterogeneity, or time-invariant differences between individuals responsible for a “propensity” toward problem behavior (Cullen et al., 1997; Gottfredson, 1985; Johnson, forthcoming; Marsh, 1991; McMorris and Uggen, 2000; Mihalic and Elliott, 1997; Mortimer et al., 1996; Mortimer and Johnson, 1998; Ploeger, 1997; Staff and Uggen, 2003; Steinberg et al., 1982, 1993).

These efforts to control for observed covariates do indeed substantially reduce the magnitude of the relationship between work intensity and problem behavior. For example, Bachman and Schulenberg (1993) found that when they control for educational commitment (GPA, college plans, curriculum) and other background variables (gender, race, region, urbanicity, parent education), they “knock out” over 30 percent of the correlation of work intensity with cigarette and heavy alcohol use, and about 25 percent of the correlation with interpersonal aggression. Safron et al. (2001) found further that controlling for participation in unstructured social activities (dating, riding around for fun) and background variables decreases the strength of the relationship of work intensity with alcohol use and binge drinking among 8th and 10th graders by well over one-half,

¹¹ The single most extensive analysis to date was conducted by Marsh (1991), who used the High School and Beyond study to control for over 25 potential confounding variables that were temporally prior to senior-year employment. One of these control variables was a lagged dependent variable measured during the sophomore year. Despite the breadth of control variables, work intensity persisted as a significant predictor of senior-year school troublesomeness (others see youth as a troublemaker, disciplinary problems, suspended, cut classes, serious trouble with the law).

and with marijuana use by well over one-third (and in several cases, the impact on marijuana use was reduced to non-significance).¹²

Despite these controls for observables, work intensity persists, however modestly, as a significant predictor of adolescent problem behavior. The robustness of the adverse work intensity effect in the face of control for such extensive observed covariates bolsters the case for a causal impact of intensive employment on adolescent problem behavior.

This logic is expressed in Staff and Uggen's (2003:264, citations omitted) recent unequivocal assessment of the research on work intensity and problem behavior:

[W]orking more than 20 hours per week appears to increase delinquency and problem behaviors for adolescents. This relationship is not necessarily a selection artifact arising from preexisting propensities of young workers, because some longitudinal evidence shows that the number of hours worked affects delinquency and substance use even when prior delinquency is statistically controlled.

The fact is, however, that *all* prior studies rely on observational data analyzed using conventional cross-sectional methods. Even surveys that were collected longitudinally (e.g., Youth Development Study, Orange County study, California and Wisconsin study, Monitoring the Future, High School and Beyond, National Longitudinal Survey of Youth, National Youth Survey) are analyzed using non-panel methods with controls for observed covariates and, in the most rigorous specifications, a control for prior problem behavior. A more recent line of research, however, recognizes that it may be impossible to control for all persistent heterogeneity through “selection on observables” (Heckman and Hotz, 1989), as traditional research is limited to doing. In other words, the inclusion of observed covariates alone—including lagged problem

¹² Gottfredson (1985) reduced the “work status effect” to non-significance by introducing a measure of prior delinquency. Ploeger (1997) did so by introducing a measure of delinquent peer association. As previously noted, however, neither of these two studies employed measures of work intensity, and relied instead on “work vs. no work” comparisons.

behavior—may be insufficient to sweep out bias attributable to persistent, *unobserved* heterogeneity. Two recent studies explicitly acknowledge the inferential risk posed by ignoring this source of selection bias, and attempt to formally account for it in their models. These two studies form the analytical foundation for the current analysis. The methods and findings from each are briefly summarized in turn.¹³

Tyler’s (2003) Instrumental Variables Design

Although Tyler (2003) examined the impact of work intensity on academic achievement, he used a clever identification strategy that informs the current study of problem behavior. He used a cross section of high-school seniors from the National Educational Longitudinal Survey (the 1992 second follow-up survey), employing a categorical measure of work hours and substituting the interval midpoints to obtain a quasi-continuous measure. He accounted for the endogeneity of school-year work hours through the use of exclusion restrictions involving state-level child labor laws, a strategy capable of producing exogenous variation in work intensity (i.e., variation in work hours that is uncorrelated with the model’s error term). Specifically, Tyler (2003:396) used the following seven instrumental variables for work intensity:

¹³ A third study—Brame et al. (forthcoming)—similarly takes selection bias seriously. These authors attempt to quantify the uncertainty about the basis for valid estimates of the causal impact of adolescent employment on delinquent behavior. Here, they rely on a binary indicator of work status (employed vs. not employed) using the first wave of the National Longitudinal Survey of Youth 1997. After conditioning on a propensity score (incorporating gender, race/ethnicity, and age), they find that the estimated “work effect” is indeed positive, as expected from previous studies. Recognizing that employment is not exogenous, they then evaluate the sensitivity of the “work effect” to plausible assumptions about (1) the effect of an unobserved “crime trait” on the probability of employment, (2) the effect of the unobserved crime trait on the probability of delinquent behavior, and (3) the prevalence of the unobserved crime trait in the population. Their sensitivity analysis showed that, although most of the estimated “work effects” were positive, in several instances they were practically zero or actually negative. Thus, their analysis was incapable of even identifying the *sign* of the work effect on crime, let alone its magnitude. Importantly, they concluded that if the unobserved crime trait increased the probability of employment and also increased the probability of delinquent behavior (both consistent with prior research), the estimated work effect could be shown to be negative.

- (1) Dollar amount collected in civil monetary penalties from child labor law violations.
- (2) Limits placed on student work after 10 p.m. on school nights.
- (3) State department of labor publicizes the names of employers who violate child labor laws.
- (4) Imposition of criminal penalties for child labor law violation.
- (5) 40-hour limit on the number of work hours per week while school is in session.
- (6) Required work permits for minors employed in agriculture-related jobs.
- (7) Required work permits for minors employed in non-agriculture-related jobs.

A traditional least squares model with controls for observed heterogeneity suggested that a 10-hour increase in school-year work hours reduced math and reading test scores by about 0.03 standard deviation. However, instrumenting for work intensity suggested a corresponding decrease of approximately 0.20 standard deviation. Tyler thus found evidence for selection bias in the relationship between work intensity and scholastic achievement, such that *the consequences of intensive work for school performance were actually worse than a traditional cross-sectional model implies*. This effect size led Tyler to conclude that child labor policies that statutorily reduce the amount of time adolescents spend in the workplace during the school year could substantially raise test scores.

Paternoster et al.'s (2003) Fixed-Effects Design

Paternoster et al. (2003) addressed the selection problem by using three waves of data from the National Longitudinal Survey of Youth 1997 and estimating a fixed-effects panel model.¹⁴ This strategy has the advantage of sweeping out time-stable differences between individuals (so-called “unobserved heterogeneity”) that may contaminate the

¹⁴ In fact, Paternoster et al. (2003) used a random-effects estimator with decomposition of all time-varying explanatory variables into between- and within-individual variation (i.e., within-panel means and deviations from those means, respectively). For inferential purposes, however, they focused on the coefficients corresponding to the deviations from within-panel means, analogous to the fixed-effects estimator since both are identified on change. They also replicated all findings with the fixed-effects logit model.

estimated effect of work intensity on problem behavior. Paternoster and colleagues used two binary variables for work intensity: (1) worked over 20 hours per week during the school year, and (2) worked over 20 hours per week during the summer only. Their estimation strategy proceeded in two steps. First, to replicate prior analyses, they estimated a cross-sectional model that included a list of common covariates, including a lagged dependent variable. In this model, the estimated coefficient for the effect of intensive work on problem behavior (delinquency, substance use, problem behavior) was positive and statistically significant. The findings from this traditional model were wholly consistent with the results of prior research. Second, they estimated a set of panel models using three waves of data, focusing their attention on the within-panel variation (i.e., within-individual change) in high-intensity work during the school year. Upon doing so, they found that *changing from non-work to intensive school-year work had no adverse effect on problem behavior*, and in fact the coefficient for intensive work was negative in all three models (and significant in one). They concluded from their analysis that the consistently reported positive association between intensive school-year work and adolescent problem behavior is driven by a process of *selection* rather than *causation*.¹⁵

¹⁵ This finding also suggests that controls for prior problem behavior are insufficient to sweep out all population heterogeneity, and thus all potential sources of selection bias. The justification for a lagged *Y* by Steinberg et al. (1993:174) is representative: “Controls for Time 1 scores rule out the possibility that observed differences at Time 2 are due to selection effects and point instead to consequences of differential patterns of employment.” This is a questionable assertion, however, since lagged *Y* is by necessity correlated with the current disturbance. If lagged *Y* is also correlated with current work involvement—and we know that it is (see Gottfredson, 1985; Mihalic and Elliott, 1997; Mortimer, 2003; Ploeger, 1997; Staff and Uggen, 2003; Steinberg and Avenevoli, 1998; Steinberg et al., 1993)—the estimate of the “work intensity effect” is biased and inconsistent. Thus, using lagged *Y* as a proxy for population heterogeneity may in fact make matters worse.

A REPLICATION AND METHODOLOGICAL EXTENSION

This study constitutes a re-assessment of the relationship between work intensity and problem behavior. The primary innovation introduced in this study is the use of instrumental variables within a panel framework to investigate the causal effect of work intensity on two problem behavior outcomes: crime and substance use. The use of this particular method follows from two important features of the data. First, I use a contemporary, nationally representative sample of youths, whereas much prior research relied on localized school samples. As such, all 50 states and the District of Columbia are represented. Like Tyler (2003), I exploit the fact that there is state-to-state variation at the cross-sectional level in the restrictiveness of the child labor laws that are enforced. Specifically, states vary in the total number of hours per week that youths are allowed to work during the school year, and in how late youths are allowed to work during the evening before school days. Second, since the same respondents are followed over a period of five years, I use a panel estimator to adjust for the potential omission of relevant time-stable covariates. Like Paternoster et al. (2003), I include individual fixed effects to sweep out all sources of unobserved heterogeneity. The resulting estimates are identified from within-individual change in school-year work hours over time.

The proposed model is a fixed-effects instrumental variables (FEIV) estimator. In this model, identification of the “work intensity effect” is predicated on exogenous within-individual variation in school-year work intensity that is attributed to the easing of child labor restrictions as youths age out of their legal status as minors. The strength of this procedure lies in the fact that state child labor laws are applicable to specific age ranges. For example, the most restrictive laws apply to youths under 16 years of age, less

restrictive laws apply to youths 16 and 17 years of age, and child labor laws no longer apply to youths 18 years of age and older. Moreover, states vary in the restrictiveness of these laws. The attractiveness of the FEIV estimator lies in its ability to eliminate bias in the estimated work effect due to omitted stable and dynamic variables. To the extent that the assumptions of the model are valid, the results provide the best estimate to date of the true average causal effect of work hours on problem behavior during the adolescent developmental period. The FEIV model thus provides an especially powerful test of the thesis that longer hours of employment during the school year causally aggravates involvement in problem behavior.

The FEIV estimator has received limited attention in economic studies of crime. For example, Levitt (1996) used a first-differenced instrumental variables (FDIV) strategy to examine the impact of prison population size on crime rates for all 50 states from 1972 to 1993. Because of the endogeneity of prison population, he introduced as instruments the status of prison overcrowding litigation, in which a state's entire prison system came under court order. As anticipated, there was a negative relationship between prison population and violent and property crime. However, the coefficient from the FDIV specification was over three times that from a traditional first-differenced model. Ayres and Levitt (1998) considered the impact of the introduction of Lojack on the auto theft rate in 57 large cities between 1981 and 1994. When they included city fixed effects, they found that the number of years of Lojack availability and the percent share of registered vehicles with Lojack installed corresponded with significant decreases in auto theft. When they instrumented for these endogenous explanatory variables (using the number of years elapsed since Lojack began the regulatory approval process as the

instrumental variable), they found that the effect of Lojack on auto theft increased by factors of four and nine, respectively.

SUMMING UP AND LOOKING AHEAD

A lengthy literature suggests that employment has detrimental consequences for involvement in a wide variety of adolescent problem behaviors, most notably delinquency and substance use. The emphasis in this literature has been almost exclusively on the amount of time that adolescents spend in the workplace, and the accumulated findings suggest that work that is of high intensity (i.e., over 20 hours weekly) is most strongly related to adolescent problem behavior. The tendency has been to interpret this association as causal, as the effect has proved to be remarkably robust to control for an exhaustive list of observed covariates. Because of this, scholars have called for time limits on youth work to minimize its adverse developmental impact. Nevertheless, recent evidence urges greater restraint in inferring causality from this empirical association. Although the intensive work effect is robust to controls for observed heterogeneity, recent analyses show that it may not withstand more rigorous controls for unobserved heterogeneity.

In this dissertation, I evaluate the plausibility of an adverse causal effect of work intensity on problem behavior during adolescence using a nationally representative panel of youths. The research evidence is sufficiently lengthy and the policy stakes sufficiently high to justify the use of a more sophisticated analytic strategy in an attempt to parse out the relative contributions of self-selection and causality. Specifically, I use a fixed-effects instrumental variables design that exploits exogenous within-individual variation in

formal work hours during the school year, using as instrumental variables a vector of state child labor laws governing school-year employment for enrolled minors. This document will proceed as follows. Chapter Two exhaustively reviews the empirical and theoretical literature on youth employment. Chapter Three describes the data and provides a detailed overview of the modeling strategy employed in this study. Chapter Four presents the results from these statistical models. Finally, Chapter Five ties the results from this study into broader issues concerning crime, criminological theory, and youth employment policy.

CHAPTER TWO: LITERATURE REVIEW

Prior to 1970, researchers paid little attention to youth employment. By this time, however, systematic surveys revealed that youth employment was an exceedingly common phenomenon (Entwisle et al., 2000). Consider the historical context of youth employment illustrated in Figure 4. This figure provides employment rates by age (14-15, 16, 17, 18) in the decennial Census from 1940 to 2000 among enrolled youths.¹ There is a clear secular increase in employment among all youths over this seven-decade period. For example, employment increases by 3.1 points per decade, on average, among 16 year olds (or 2.7% per decade), 4.8 points among 17 year olds (3.9%), 5.5 points among 18 year olds (4.2%), and 4.1 points among 19 year olds (3.1%). In the 2000 Census, 24 percent of enrolled 16 year olds were employed, 37 percent of enrolled 17 year olds, and 46 percent of enrolled 18 year olds.

*** Figure 4 Here ***

Apel et al. (2003) provide similar estimates of youth employment using the annual Current Population Survey, designed much like the Census. They find that from 1980 to 2000, 31 percent of 16 and 17 year olds were employed during school months. For a variety of reasons, however, the Census and Current Population Survey conservatively estimate youth employment. For one, parents are relied upon to report on the employment of their teenage children. More importantly, both surveys inquire about formal jobs—what may be termed regular, “paycheck” work—thereby ignoring a much more pervasive, informal labor market. We will see the magnitude of this omission in the

¹ I attained these estimates from five percent samples available from the Integrated Public Use Micro-Data Series (IPUMS). The time series for 14-15 year olds is included for comparative purposes, but is not discussed due to incomplete data. Beginning in 1970, the Census inquired about employment only among youths 16 or older.

next section, in which I describe in some detail the nature of youth employment, both formal and informal.

THE NATURE OF CONTEMPORARY ADOLESCENT EMPLOYMENT

A pernicious descriptive problem is arriving at a consistent definition of youth employment. One source of variability across surveys is the scope of the definition of employment. Many surveys operationalize youth employment vaguely as “paid work” or “work for pay” (Bartko and Eccles, 2003; Johnson, forthcoming; Marsh, 1991; Wright et al., 1997), a “regular job” (Gottfredson, 1985), or a “paying job” (Tanner and Krahn, 1991). Other surveys offer more precise definitions. Following is a sampling of employment definitions from the six major youth surveys from which multiple publications appear in the literature:

Orange County, California, study – regular, paid employment, by non-family members, of at least three hours per week.

California and Wisconsin study – employed in a regular paying part-time job, excluding volunteer and occasional work.

Youth Development Study – employed at least once a week outside of the home for pay.

Monitoring the Future – employed in a paid or unpaid job currently or in the last three months, excluding chores.

National Youth Survey – employed in the community for pay during the last year, excluding allowance.

National Longitudinal Survey of Youth 1997 – employed in a situation that entails an ongoing relationship with a specific employer.

A second source of variability is the temporal dimension of adolescent employment. The first four definitions above, because they are student surveys, refer to

employment during the current school year, whereas the latter two refer to employment during the calendar year. In other studies, youths are variously defined as employed if they worked in a specific calendar year (Gottfredson, 1985), during the previous week (Wright et al., 1997), or during the current school year (Johnson, 2003; Tanner and Krahn, 1991). Only three studies simultaneously consider school-year and summer work (Apel et al., 2003; Marsh, 1991; Paternoster et al., 2003). A third source of variability is whether the sample is stratified by age or by year in school. After all, a 16-year-old youth can be in the 10th or 11th grade, meaning that age-based and grade-based samples are not directly comparable.

Employment Status

Figure 5 illustrates rates of employment in formal and informal jobs by month from ages 14 to 18 among youths in the National Longitudinal Survey of Youth 1997 (hereafter NLSY). During the month in which they turn 14, 43 percent of all youths are employed in some kind of job. However, we see that three percent of 14 year olds are employed only in formal jobs, 37 percent are employed only in informal jobs, and another three percent are employed in both formal and informal jobs. When we look ahead to the month in which youths turn 18, we see that 56 percent of all youths report working in a job. At this age, 49 percent are employed only in formal jobs, four percent are employed only in informal jobs, and four percent are employed in both formal and informal jobs. Thus, youth employment figures are sensitive to the degree of formality of the definition of employment. Definitions restricted to formal jobs will underestimate the true extent of youth work at younger ages. Conversely, broad definitions of employment

will overestimate the extent of youth employment at younger ages. Consider that the share of all youth employment accounted for by formal work increases from 14 to 94 percent from age 14 to age 18. It is thus at later ages—toward the end of high school—that formal employment figures accurately reflect the true extent of youth work.

*** Figure 5 Here ***

In order to assess comparability across youth surveys, I consider employment among 10th through 12th graders in high school. The following figures are summarized in Table 1. *Lower-range estimates* of youth work in existing surveys suggest average employment rates of 40 to 50 percent. Greenberger et al. (1981) report that 40 percent of 10th and 11th graders were employed. Agnew (1986) finds that 46 percent of 11th grade boys report working. Steinberg and Dornbusch (1991) find that 45 percent of their high-school youths are employed (excluding 9th graders). *Middle-range estimates* suggest average employment rates of 50 to 70 percent. Mortimer (2003) finds that 47 percent of 10th graders were employed, 58 percent of 11th graders, and 64 percent of 12th graders. Tanner and Krahn (1991) report that 70 percent of seniors were employed during the school year (while 57 percent were currently employed). *Upper-range estimates* suggest average employment rates over 70 percent. Bachman et al. (1986) report an 80 percent employment rate among high-school seniors. Bachman and Schulenberg (1993) find further that 74 percent of seniors were working for pay, while another 6 percent were working but not for pay. Marsh (1991) reports a 64 percent employment rate among 10th graders, 70 percent among 11th graders, and 77 percent among 12th graders. Wright et al. (2002) find that 86 percent of seniors in their second survey (Tri-Cities Study) were employed (75% currently).

*** Table 1 Here ***

The formal employment figures provided by the NLSY place it in the lower range of 40 to 50 percent. Recall that Figure 1 provides school-year formal employment rates in the NLSY of approximately 25 percent among 10th graders, 44 percent among 11th graders, and 56 percent among 12th graders. The weighted average for all three grades is 38 percent. The primary explanation for the conservative employment rate in the NLSY relative to other surveys is the use of the narrower, more explicitly “formal” work criterion. Most other surveys include “informal” workers in their employment definitions, which we know will inflate estimates of youth work at younger ages (see Figure 5). As an illustration, Mortimer (2003) reports that 23 percent of 10th grade workers, nine percent of 11th grade workers, and six percent of 12th grade workers are employed in informal work. Broadening the definition of employment in the NLSY to include either formal or informal work places it in the middle range, as 50 percent of sophomores, 59 percent of juniors, and 65 percent of seniors are employed in any job (see Table 1).

Work Intensity

Figure 6 provides the average work intensity by age, showing that the number of hours worked increases in a uniform fashion through the teenage years. During the month in which they turn 14, employed youths are working 13 hours per week, on average. By their 18th birthday, however, the typical employed youth is working 28 hours per week. The impact of summer on work intensity is much more pronounced at younger ages. Figure 7 categorizes the intensity of the work commitment into five-hour intervals over the 14-18 age span. Over half of employed 14 year olds (58%) work 10 hours or less per

week, on average, a figure that drops to 27 percent by age 16, and just 12 percent by age 18. If we use the conventional 20-hour cut-off to distinguish “moderate” from “intensive” work, we see that intensive employment (over 20 hours) accounts for only 20 percent of youth work at age 14, but 37 percent at age 16, and 60 percent at age 18. Notice that the proportion of “full-time workers” (over 35 hours) is seven percent, 12 percent, and 28 percent, respectively.

*** Figures 6 and 7 Here ***

Consider how the NLSY compares with other youth surveys with respect to work intensity. To maximize comparability, I will again restrict my attention to surveys that include 10th through 12th graders in high school (see Table 1). I will begin with estimates of mean work intensity. At the low end are the high-school youths in Marsh’s (1991) study, in which 10th graders work 6 hours per week, on average, 11th graders work 12 hours, and 12th graders work 13 hours. Tanner and Krahn (1991) report an 18-hour work week among currently employed 12th graders. At the higher end of the work intensity continuum is Agnew (1986), whose employed 11th graders work 21 hours per week. Mortimer (2003) finds that 10th grader youths work 17 hours per week, 11th grade youths work 20 hours, and 12th grade youths work 21 hours.

Several studies rely on categorical measures of work intensity. The moderate-intensive distinction provides a convenient way to compare across these studies. Steinberg and Dornbusch (1991) provide conservative estimates of intensive work, categorizing 28 percent of their high-school youths as intensive workers (excluding 9th

graders).² Mortimer (2003) reports that 28 percent of employed 10th graders are categorized as intensive workers, as are 40 percent of 11th graders, and 40 percent of 12th graders. Bachman et al. (1986) find that 39 percent of employed seniors are intensively employed. Using later cohorts, Bachman and Schulenberg (1993) find that 42 percent of employed seniors are intensive workers.

Among employed youths in the NLSY, Figure 2 shows that during school months, 10th graders work an average of 19 hours per week, 11th graders 22 hours, and 12th graders 26 hours. The weighted average for these three years is 23 hours per week during school months. The NLSY also suggests that 34 percent of 10th graders, 42 percent of 11th graders, and 55 percent of 12th graders, work at high intensity during the school year (see Figure 3). By both measures, then, the estimates of work intensity in the NLSY tend toward the high end relative to other youth surveys. As with employment rates, this is probably due in large part to the fact that the NLSY emphasizes formal work, whereas other surveys sweep up both formal and informal employment, the latter of which tends to be more irregular.

Age of First Employment

Figure 8 provides age of first employment in formal and informal jobs. Almost two-thirds (63%) of employed youths take their first informal job between 12 and 14 years of age. The mean age of the first informal job is 13.1 (median = 13). The modal age of the first formal job is 16 years (36%), corresponding with the age in which many state

² In their follow-up study, Steinberg et al. (1993) report that 22 percent of the sample of 10th and 11th grade youths at the first wave are employed at high intensity. At the second wave, as 11th and 12th graders, 28 percent of employed youths are working at high intensity.

child labor laws ease work restrictions.³ Notice that by this age, however, 80 percent of employed youths have already taken their first formal job. The mean age of the first formal job is 15.5 (median = 16).

*** Figure 8 Here ***

Industry and Occupation

Figures 9 and 10 describe the industry of first formal youth jobs. The four top industries accounting for 62 percent of all youth work between ages 14 and 18 include eating and drinking establishments, other retail trade, entertainment and recreation service, and grocery stores (see Figure 9). Employment in eating and drinking establishments takes on particular relevance at age 16, when fully 40 percent of first jobs acquired at this age are concentrated in this industry. When we consider gender and race patterns (see Figure 10), white males are most likely to work in agriculture and, along with Hispanic males, construction. Males are over-represented in grocery stores and, along with white females, recreation and entertainment facilities (e.g., movie theaters, video rental stores). Females, meanwhile, are much more likely than males to work in

³ By way of example, the child labor provisions of the federal Fair Labor Standards Act of 1938 (FLSA) govern businesses that (1) have annual gross sales of at least \$500,000; (2) are hospitals or educational institutions; (3) are public agencies; or (4) are engaged in interstate commerce or in the production of goods for interstate commerce (see U.S. Department of Labor, 2000:3-13). The FLSA bans employment of youths under 14 years of age, although exceptions for informal work are allowed (e.g., babysitting, paper routes). Youths 14 and 15 years of age may work outside of school hours, but are prohibited from working in occupations other than retail, food service, and gasoline service. Their employment is also restricted to no more than 18 hours per week or 3 hours per day when school is in session, and to no more than 40 hours per week or 8 hours per day when school is not in session. They are also restricted to working between 7 a.m. and 7 p.m. except during the summer, when they may work evenings until 9 p.m. Youths 16 and 17 years of age may work in any occupation that is not deemed hazardous for this age group (e.g., mining, meatpacking, operating certain types of machinery). These youths are allowed to work any time of the day, any day of the year, and for unlimited hours. Youths 18 years of age and older are no longer subject to the child labor provisions of the FLSA. Note that not all youth employment is covered under the FLSA, and numerous exemptions from these provisions are allowed (particularly in agricultural work). Moreover, every state has its own child labor law that may be more or less restrictive than the provisions of the federal law.

eating and drinking establishments, which also tend to over-represent black males. Other retail trade (e.g., department store, gasoline service station, shoe store, appliance store, gift shop) also tends to be dominated by females, white and Hispanic females in particular, as well as health and social services (e.g., doctor's office, hospital, child care). Notice that, relative to their same-gender peers, black youths are over-represented in health and social service industries, but under-represented in other retail trade.

*** Figures 9 and 10 Here ***

Figures 11 and 12 describe the occupation of first formal youth jobs. At all ages, food preparation and service work accounts for the largest share (28%) of youthful occupations (see Figure 11). By the middle teenage years, work as a cashier is the second most common occupation (17% at age 16). Considering gender and race patterns (see Figure 12), males are more likely than females to work in occupations as construction helpers, stock handlers or baggers, and other laborers. Females, on the other hand, are much more likely to work as cashiers (especially black females) and administrative support (e.g., receptionist, file clerk, messenger). Females also tend to be over-represented in sales occupations (e.g., clothing store, home furnishings store, appliance store, sales counter clerk) and, along with black males, in food service occupations (e.g., waiter/waitress, cook). Black youths tend to predominate in other service occupations (e.g., family child care, dental assistant, janitor, baggage porter).

*** Figures 11 and 12 Here ***

Informal occupations are illustrated in Figures 13 and 14. The two most pervasive types of informal jobs (see Figure 13)—which together comprise 80 percent of first informal jobs between ages 11 and 14—are babysitting and yard work (e.g., lawn

mowing, snow shoveling, leaf raking, tree trimming, wood cutting). Across all ages, babysitting accounts for one-half (49%) and yard work one-quarter (27%) of all first informal jobs. Manual labor (e.g., maintenance and repair, construction, painting, moving or hauling) takes on greater importance at later ages, accounting for 10 percent and more of first informal jobs at age 16 and later. Paper routes account for five percent of youth jobs at age 10, but decline in importance thereafter. The remaining informal occupations entail such tasks as cleaning (house, office, church, school), farm work, office chores (e.g., filing, reception, computer work, flyers), and odd jobs (e.g., pet sitting, house sitting, car washing, hair cutting, tutoring, errands). When we consider gender and race patterns (see Figure 14), we see that females are far more likely to babysit in their first informal job than males, although a non-trivial proportion of males also babysit in their first informal job. Males, on the other hand, are much more likely than females to do yard work and manual labor. Relative to their same-gender peers, black youths tend to work somewhat more often in odd jobs.

*** Figures 13 and 14 Here ***

The foregoing description of the contemporary youth workplace is consistent with reports that youth employment as a whole is overwhelmingly concentrated in the retail and service sectors, constituting what Greenberger and Steinberg (1986) refer to as the “new adolescent workplace.” It is also consistent with other reports about the gender and racial stratification that exists in adolescent jobs. Males are much more likely than females to be employed in formal and informal jobs that entail some form of manual labor (e.g., yard work, construction, stock handler) or farm work, whereas females are more likely to be employed in jobs that entail counter work (e.g., cashier, retail sales),

desk work (e.g., administrative support), or caring for people (e.g., babysitting, health and social services) (see also Apel et al., 2003; Entwisle et al., 1999; Greenberger and Steinberg, 1983; Mortimer, 2003; Mortimer et al., 1990; National Research Council, 1998; White and Brinkerhoff, 1981). In other words, young males tend to “work with their hands,” while young females tend to “work with people” (see Mortimer, 2003). Minority youths, on the other hand, are more likely than their white counterparts to work in service and unskilled occupations (e.g., cook, janitor, hospital worker) and in unspecified “odd” jobs (see also Apel et al., 2003; Entwisle et al., 1999, 2000; National Research Council, 1998; U.S. Department of Labor, 2000).

ADOLESCENT WORK INTENSITY, DEVELOPMENTAL RESEARCH, AND CRIMINOLOGICAL THEORY

A new phase of youth employment research commenced in the early 1980s. This research began to raise serious questions about the potential interference of employment with more important developmental tasks of adolescence (Entwisle et al., 2000). The conclusions from developmental research were in stark contrast to those from economic research, from which there was nearly universal agreement that work involvement during high school is a form of investment that provides net benefits in the early adult labor market (Ruhm, 1995:302). For example, for up to two years following graduation, youths who worked during high school tend to have higher rates of labor force participation, earn higher wages and income, work for longer duration, achieve a higher-status occupation, and experience fewer bouts of unemployment than their peers who remained unemployed or out of the labor force during high school (Light, 1994; Marsh, 1991; Meyer and Wise, 1982; Michael and Tuma, 1984; Mortimer and Johnson, 1998;

Stephenson, 1981a, 1981b; Stern and Nakata, 1989; Stevenson, 1978; but see Hotz et al., 2002). These results have been corroborated in long-term follow-ups of six years (Ruhm, 1995, 1997) and ten years (Carr et al., 1996).

The seminal work among developmental researchers, on the other hand, was a volume by Greenberger and Steinberg (1986), the culmination of an extensive and well-publicized collaboration that challenged widely held beliefs about the benefits of work experience during adolescence (e.g., Greenberger and Steinberg, 1981; Greenberger et al., 1981, 1982; Steinberg et al., 1981, 1982). They stated their conclusions bluntly: “[E]xtensive commitment to a job may interfere with the work of growing up...work that requires exploration, experimentation, and introspection” (1986:7, 9). This research agenda did not arise in a vacuum, and in fact, they were not the first to express concerns about the precocity of early work involvement; they were simply the most vocal about it (see Greenberger, 1983; Steinberg, 1982).⁴

Results from developmental research generally favored a less sanguine view of the benefits of early work experience. To be sure, the overarching theme was that “the correlates of school-year employment are generally negative” (Steinberg and Dornbusch, 1991:309). Developmental research showed that adolescent employment—particularly “high-intensity” employment of over 20 hours weekly during the school year—has

⁴ Greenberger and Steinberg (1986) echoed a theme made forcefully in the early 20th century by Hall (1904:xvi-xvii):

In this environment our 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.

The heightened concern with adolescents who adopted adult-like behaviors and attitudes may have been symptomatic of deeper concerns about the social changes taking place at the turn of the century. Kett (1978) points out that precocious adolescents were probably viewed as symbols of an increasingly “precocious” society.

detrimental consequences for several arenas of adolescent development: mental and physical health, family relationships, school performance, and peer associations, to name a few. The accumulated empirical findings from the last 20 years suggest several theoretical avenues to explain the consistently positive relationship between work intensity and delinquency and substance use reviewed in Chapter One. I review two types of theories: those that specify causal mechanisms and those that specify spurious mechanisms.

Causal Mechanisms for the Adverse Work Intensity Effect

Figure 15 illustrates several potential causal mechanisms to explain the relationship between intensive work during adolescence and elevated involvement in problem behavior. In the language of structural equations, this positive correlation reflects a “reduced-form” or indirect relationship, in that one or more intermediate mechanisms are directly responsible for the observed effect. Thus, intensive work is simply the first step in a causal sequence, the end result being increased levels of problem behavior. Prominent criminological theories that specify mediating causal mechanisms include social control, general strain, learning, and routine activity theories. Each of the potential causal mechanisms highlighted by these theories, as well as their empirical plausibility, is summarized below.

*** Figure 15 Here ***

Social Control Theory. Youth employment has particular salience for adolescent functioning in family and school domains. With respect to family relationships, developmental research suggests that intensive employment disrupts healthy parent-child

relationships. Youths who work intensively during high school tend to spend less time with (Greenberger et al., 1980; Marsh, 1991; Mihalic and Elliott, 1997; Mortimer and Shanahan, 1994; Pickering and Vazsonyi, 2002; Steinberg and Dornbusch, 1991),⁵ are less emotionally close to (Greenberger et al., 1980; Pickering and Vazsonyi, 2002; Shanahan et al., 1991; see Gottfredson, 1985, for an exception), engage in more disagreements with (Bachman et al., 1986; Bachman and Schulenberg, 1992, 1993; Manning, 1990; Mortimer and Shanahan, 1994), are less closely monitored by (Manning, 1990; Pickering and Vazsonyi, 2002; Steinberg and Avenevoli, 1998; Steinberg and Dornbusch, 1991; Tanner and Krahn, 1991), and exercise greater decision-making autonomy vis-à-vis (Mortimer and Shanahan, 1994; Steinberg and Avenevoli, 1998; Steinberg and Dornbusch, 1991) their parents than non-workers or moderate workers.⁶

In schooling domains, intensive youth work appears to be responsible for disinvestment in and disengagement from school. High-intensity work during high school tends to be associated with less time spent on homework and studying (Agnew, 1986; Bachman et al., 1986; Bartko and Eccles, 2003; D'Amico, 1984a; Lillydahl, 1990; Marsh, 1991; Mihalic and Elliott, 1997; Mortimer et al., 1996; Steinberg et al., 1982, 1993; for an exception see Gottfredson, 1985; Osgood, 1999; Schoenhals et al., 1998),

⁵ It is possible that this is moderated by residential location and job type. Shanahan et al. (1996) found that intensive work is associated with more time spent with the family. Their sample consisted of youths residing in central Iowa, many of whom worked with other family members in family-owned businesses or on farms.

⁶ Youths that work at moderate intensity, however, may actually experience more positive relationships with their parents than even non-workers (Pickering and Vazsonyi, 2002). This idea fits well with research indicating that parents by and large encourage work involvement that fosters self-reliance and self-worth, financial independence, time management skills, and an appreciation for adult responsibility (Phillips and Sandstrom, 1990). Moreover, many of the youth-parent disagreements that emerge about employment tend to concern such relatively mundane issues as how late youths stay out, helping more around the house, and how earnings are spent (Manning, 1990; see also Campbell, 1969; Csikszentmihalyi and Larson, 1984). In other words, the issue underlying these conflicts is the newfound independence of working adolescents; not so much its existence perhaps as the speed with which it is acquired and the circumstances under which it is exercised (Campbell, 1969:830).

cutting class and absenteeism (Bachman et al., 1986; Bachman and Schulenberg, 1992; Barling et al., 1995; Greenberger et al., 1981; Lillydahl, 1990; Marsh, 1991; Schoenhals et al., 1998; Steinberg and Dornbusch, 1991; Steinberg et al., 1993; for an exception see Gottfredson, 1985; Hotchkiss, 1986), lower educational expectations and aspirations (Agnew, 1986; Bachman et al., 2003; Bachman and Schulenberg, 1992; Marsh, 1991; Mihalic and Elliott, 1997; Mortimer and Finch, 1986; Steinberg et al., 1993; for an exception see Green and Jaquess, 1987), a non-college prep or non-academic track curriculum (Bachman et al., 1986, 2003; Bachman and Schulenberg, 1992; Marsh, 1991), fewer courses in mathematics and science (Marsh, 1991; Singh and Ozturk, 2000), negative school attitudes (Agnew, 1986; Bachman et al., 1986; Steinberg and Avenevoli, 1998; Steinberg and Dornbusch, 1991; Steinberg et al., 1982, 1993; but see Gottfredson, 1985; Mortimer et al., 1996), and dropout and forgoing college attendance and completion (Carr et al., 1996; D'Amico, 1984a; Lillydahl, 1990; McNeal, 1997; Mortimer and Finch, 1986; Mortimer and Johnson, 1998; Steel, 1991; Warren et al., 2001; Warren and Lee, 2003). The research with respect to scholastic performance (as measured by grades and test scores) is mixed, with numerous studies finding evidence that longer work hours correspond with lower educational performance (Agnew, 1986; Bachman et al., 1986, 2003; Bachman and Schulenberg, 1992; Green and Jaquess, 1987, using ACT scores; Hansen and Jarvis, 2000; Marsh, 1991; Mortimer and Finch, 1986; Mortimer and Johnson, 1998; Schill et al., 1985; Singh, 1998; Steinberg and Dornbusch, 1991; Tyler, 2003), while others find no relationship (Green and Jaquess, 1987, using GPA; Hotchkiss, 1986; Mihalic and Elliott, 1997; Mortimer et al., 1996; Schoenhals et al., 1998; Steinberg et al., 1982, 1993; Warren et al., 2000).

Social control theory, for one, predicts that an intensive work commitment during adolescence disrupts emotional attachment to parents (“relational control”), diminishes parental monitoring of adolescent behavior (“instrumental control”), and erodes commitment to conventional educational pursuits (Hirschi, 1969). For Hirschi, youths who share their thoughts, feelings, and goals with their parents, and who personally identify with their parents—who are, in a word, attached—are more strongly bound to their expectations and thus to the legal norms of society. Affectional identification, love, and respect are crucial elements of the emotional bond to parents. At the same time, youths who spend more time in the company of their parents (direct monitoring), but more importantly who perceive their parents as aware of their whereabouts, companions, and activities when away from home (indirect or “virtual” monitoring), are more likely to give thought to parental reaction before engaging in deviant behavior. In the domain of school, youths who demonstrate higher academic competence, express greater liking of school, have more concern for the opinions of teachers, exert more scholastic effort, perceive good grades as important, and devote more time to homework, consequently have more time and energy invested in this conventional socializing institution, and thus more to lose by engaging in deviant conduct. Taken as a whole, then, social control theory proposes that the unattached, uncommitted, and uninvolved youth is subject to lower levels of informal social control over her behavior. To the extent that intensive work weakens the youth’s bond to conventional society, higher levels of delinquency and substance use are a consequence.

General Strain Theory. Most adolescents work in minimum-wage jobs concentrated in the secondary labor market (Greenberger and Steinberg, 1986), and little

of what they do on the job provides opportunities for learning, such as the use of reading, writing, or arithmetic skills, formal training, or spending time with a supervisor (Greenberger et al., 1982). Mortimer (2003) finds that intensive work patterns are characterized by a significantly greater number of work stressors, such as uncertainty about work tasks, role overload, and noxious work conditions, and this job stress is much more likely to be perceived by young males than young females. Greenberger et al. (1981) report that work stressors such as task meaninglessness, poor environmental conditions, autocratic supervision, low wage structure, and conflict between work and school roles increase the likelihood of excessive alcohol use. Ruggiero et al. (1982) also find that a stressful work environment (high levels of noise, time pressure, temperature extremes, heavy lifting, dangerous equipment) increases the likelihood of theft and non-theft deviance in the workplace. Part of this effect may operate through the development of cynical attitudes toward work (Ruggiero et al., 1982; Steinberg et al. 1982).⁷

In addition to these on-the-job stressors is the concern that extensive involvement in employment compromises a youth's central role as student by conflicting with school demands. In other words, premature "embeddedness" in the worker role diminishes the time and effort invested in other developmentally appropriate roles, primarily the student role (see Wright et al., 2002). Hansen and Jarvis (2000) find that longer work hours interfere with school attentiveness (e.g., come to school tired because of work). Warren

⁷ Employed adolescents are not immune from occupational injuries, illnesses, and even fatalities. The U.S. Department of Labor (2000:58-67) estimates that 67 work-related deaths occurred per year during the 1992 to 1998 period, with little variability over time. While 43 percent of these fatalities were concentrated in the agricultural industry, 19 percent were in retail trade, and another 14 percent in construction. Of the fatalities among youths in retail trade, about two thirds were homicides, suggesting robbery. From 1992 to 1997, there were almost 18,800 injuries and illnesses per year, on average, among youths 17 and younger requiring time away from work. Over this period, injured youths lost a median four days of work, with the most frequent type of injury being sprains, strains, or tears. Over two thirds of workplace injuries were in the retail trade sector.

(2002) finds that longer work hours diminish the centrality of the student role for youths' identities. Mortimer (2003) reports that many young workers struggle in combining the roles of employee and student. The most intensively employed youths have difficulty completing their homework, coming to class prepared, coming to school on time, and coming to school with adequate rest. Staff and Uggen (2003) find that work-school incompatibility (work reduces grades) is a consistent predictor of senior-year problem behavior, controlling for other work characteristics.

Several studies also find that intensive employment has consequences for the mental and physical health of adolescents. Intensively employed adolescents tend to suffer from lower self-esteem (Steinberg and Dornbusch, 1991; Steinberg et al., 1993; for exceptions see Bachman et al., 1986; Bachman and Schulenberg, 1992, 1993; Mortimer et al., 1996), depressed mood (Resnick et al., 1997; Shanahan et al., 1991), and diminished time allotted for sleep, exercise, and breakfast (Bachman et al., 1986; Bachman and Schulenberg, 1992, 1993; D'Amico, 1984b; Mortimer, 2003; Safron et al., 2001), in addition to a higher likelihood of crime victimization (Bachman et al., 1986, 2003; Bachman and Schulenberg, 1992, 1993; Dugan and Apel, 2003).

According to general strain theory, heavy work involvement creates job dissatisfaction and stress, a form of noxious stimuli that, in the absence of conventional coping mechanisms, is eased through delinquency and alcohol and drug use (Agnew, 1992). Agnew's theory proposes that negative relationships with others increase the likelihood that individuals will experience negative affect (depression, anger, fear, cynicism), with a corresponding desire to take corrective action to alleviate strain. One potent source of strain relevant to adolescent work is the "presentation of negatively

valued stimuli” in the workplace. The highly impersonal and demanding workplace of teenage “McJobs” may generate a desire to manage the resulting negative affect inwardly through substance use (smoking, drinking, using marijuana) and poor physical health (lack of sleep, exercise, eating). Brezina (1996) highlights three additional coping responses, or delinquent adaptations, to the strain that may be induced by intensive employment. Youth may respond to the strain produced by conflict with school by skipping classes or dropping out of school altogether, a form of “escape/avoidance” behavior. Low remuneration of youth work in combination with long hours on the job may generate a desire to “level the playing field” by stealing money or goods from one’s employer, taking extra time for breaks, purposely working in an inefficient manner, or other forms of “compensation” for their negative emotional states. Strained youths may also adapt through vandalism, physical violence, and other forms of “retaliation” as a way to achieve a sense of justice.

Learning Theory. The adolescent workplace is dominated by young people. Greenberg et al. (1982) find that food service jobs—among the most common adolescent jobs—involve a high degree of time spent with and interaction with peers in the workplace. Thus, an intensive work commitment puts adolescents in contact with a wider circle of young people, some of whom may have dropped out of school. McMorris and Uggen (2000) find that spending more time with work friends leads to higher levels of 9th grade alcohol use. Moreover, several researchers find that employment increases exposure to delinquent peers (Mihalic and Elliott, 1997; Ploeger, 1997; Tanner and Krahn, 1991). Wright and colleagues (Wright and Cullen, 2000; Wright et al., 2002) find further that a high level of co-worker delinquency increases a youth’s own non-work

delinquency. Ruggiero et al. (1982) report that working in a positive social environment (closeness with co-workers, opportunity to get to know people in the workplace, more socializing with co-workers outside the workplace) actually increases the likelihood of engaging in occupational deviance. Wright and Cullen (2000) find that contact with delinquent co-workers is a strong predictor of occupational deviance, net of job characteristics (positive job skills, negative work environment) and delinquent propensity (low self-control, non-work delinquency). In fact, delinquent youths who interact with delinquent co-workers increase their occupational deviance over and above the direct effects of either engaging in non-work delinquency or associating with delinquent co-workers.

Learning theories such as differential association and social learning propose that intensive employment puts youths in regular contact with older, less-than-conventional peers in the workplace, who provide a source of antisocial attitude transference, behavioral modeling, and reinforcement (Akers et al., 1979; Sutherland, 1947). Intensive employment puts youths in close proximity to a wider social circle for a non-trivial number of hours each week. Since the workplace is less age segregated than the high school, working youths are likely to come into regular contact with older adolescents and young adults. Considering that youth job opportunities often originate in low-wage, low-skill, service occupations, adult co-workers in this environment are expected to be more deviant and unconventional. Consequently, intensive employment potentially alters the balance of definitions favorable and unfavorable to violation of the legal code, as Sutherland's differential association theory predicts. Moreover, it is quite likely that the working youth's parents are less familiar with their son's or daughter's new workplace

associates than with his or her school peers. Akers' social learning theory proposes that this differential association forms an immediate context for the acquisition of antisocial definitions (verbal) and delinquent imitation (behavioral). Over time, the resulting problem behavior is maintained through processes of differential reinforcement. Warr (2002) proposes that three potent sources of social reinforcement for antisocial conduct are fear of ridicule for non-compliance, loyalty to the group, and the social status that the association with older youths brings. There is also the possibility of non-social reinforcement attributable to the intrinsically rewarding experience of committing crime, including, in the case of employee theft, the feeling of "being on a high" and the sense of accomplishment associated with "edgework" (Wood et al., 1997), or the "sneaky thrills" associated with conquering one's emotions to maintain a normal, calm appearance at the workplace until the crime has transpired (Katz, 1988).

Routine Activity Theory. In contrast to a straightforward "zero-sum" model of the allocation of time between employment and leisure activities, work intensity does not lead to a reduction in time spent with friends (Greenberger et al., 1980; Steinberg et al., 1982). On the contrary, adolescents who work at high intensity tend to spend more time each week going out on dates, (Agnew, 1986; Bachman et al., 1986; Bachman and Schulenberg, 1992, 1993; Mihalic and Elliott, 1997; Safron et al., 2001), going out for evening fun and recreation (Bachman et al., 1986; Bachman and Schulenberg, 1992; Safron et al., 2001), and spending time with friends (Ploeger, 1997). Working adolescents also participate in such activities as cruising around, going to parties and movies, shopping, and having informal get-togethers with friends (Bachman et al., 1986; Osgood, 1999; Safron et al., 2001). Thus, employed youths appear to be able to find time for an

active social life. Time for these consumption-driven and peer-oriented leisure activities may be drawn from other forms of adolescent time use, as extensive work involvement also tends to be associated with less time spent watching TV, being alone, playing instruments, reading for pleasure, participating in sports, volunteering, joining clubs, and observing religious ceremonies (Bachman et al., 1986; Bartko and Eccles, 2003; Green and Jaquess, 1987; Osgood, 1999; Safron et al., 2001), as well as completing fewer household chores (D'Amico, 1984b).

An extensive work commitment also presents newfound opportunities for workplace deviance unavailable to non-employed youths. Working youths who report that it would be easy to steal at work are more likely to engage in theft and non-theft workplace deviance (Ruggiero et al., 1982). Wright and Cullen (2000) find that low self-control and non-work delinquency—proxies for delinquent “propensity”—are independent predictors of occupational deviance. Thus, the workplace can be a “gold mine” of deviant opportunities for those youths who are so inclined.

Routine activity and opportunity theories predict that intensively employed youths spend more time engaging in unstructured leisure activities with peers in the absence of adult authority figures (Osgood et al., 1996). According to Osgood and colleagues, the motivation for problem behavior inheres in the situation rather than in the participants or in the activity itself (see also Briar and Piliavin, 1965). In particular, situations most conducive to deviance involve (1) time spent in the company of peers, (2) an absence of adult authority figures (e.g., “handlers” or “place managers”), and (3) no organization or agenda for how time is to be spent. Accordingly, youths who are intensively employed are more likely to spend their time being “out and about” (e.g., driving, cruising,

shopping) and “making the scene” (e.g., parties, dates, bars), suggesting a pattern that is “anything but ‘all work and no play’” (Osgood, 1999:180). They thereby more often place themselves in these social situations conducive to deviance. The automobile plays a particularly important role in the routine activities of youths. By providing autonomy, having a car presents ample opportunities to cruise around with friends farther away from home and the watchful eyes of parents and other “handlers” (Felson, 1998). From a simple opportunity perspective, moreover, adolescent employment facilitates access to the “tools” of deviance. For example, the income earned by working can be used to purchase cigarettes, alcohol, marijuana, or stolen property. Additionally, older workplace associates who are of legal age are able to purchase alcohol for their underage co-workers.

Spurious Mechanisms for the Adverse Work Intensity Effect

Some theories propose that the positive relationship between intensive work and problem behavior is a spurious one. These theories view the manifold negative correlates of intensive youth employment reviewed in the foregoing section as more than a mere coincidence. Rather, they are viewed as multiple outcomes of a common underlying process. Bachman and Schulenberg (1993:232) conclude that “heavy time commitment to employment can be seen as an important symptom of a potentially wide range of psychosocial difficulties.” The emphasis on intensive work as a *symptom* clearly suggests that it is but one of several adverse outcomes associated with some latent, unifying process. Studies of the correlational structure of problem behavior during early and middle adolescence lend credence to this perspective. For example, Donovan and Jessor

(1985) find that such diverse behaviors as alcohol use, smoking marijuana, sexual intercourse, general deviant behavior (shoplifting, vandalism, lying, truancy, fighting, parental defiance), and even church attendance and school performance (reverse coded), all load on a single first-order construct that they refer to as a “syndrome” of problem behavior. This finding of a univariate factor structure of adolescent problem behavior is remarkably robust (see also Donovan et al., 1988; Farrell et al., 1992; McGee and Newcomb, 1992).⁸

Empirical studies that explicitly address the possibility of spuriousness support this notion. The results of Paternoster et al. (2003), clearly fall under a “spurious work effect” interpretation. Recall from Chapter One their finding that, once they adjust for unobserved heterogeneity using individual fixed effects, there is no relationship between intensive work during the school year and delinquency, substance use, or problem behavior. Schoenhals et al. (1998) find that the relationship between tenth-grade work intensity and academic outcomes (grades, absenteeism, time spent on homework) disappears once background variables (e.g., parental education, family income, school type) and controls for pre-existing differences between workers and non-workers (e.g., 8th grade academic performance and school detachment) are introduced.⁹ Warren et al. (2000) use instrumental variables to identify the simultaneous relationship between work intensity and academic performance in the 12th grade (grades in academic courses). They instrument for work intensity using the lagged (10th grade) zip-code-level employment

⁸ Notably, this appears to be true only during early and middle adolescence. Studies of late adolescents and young adults suggest greater differentiation (specialization?) of problem behavior (Osgood et al., 1988).

⁹ It is noteworthy that introducing the predictors of employment does not reduce the effect of work intensity on absenteeism to non-significance. In fact, there is a roughly linear increase in absenteeism as a function of work intensity. The effect, however, is quite modest, suggesting that youths that work the most intensively (31+ hours per week) are absent just over one additional day (1.3) per semester, on average, relative to non-working youths.

rate of 16 to 19 year old high-school students and graduates, and they instrument for academic performance using lagged reading and math achievement scores. Subsequently, they find no relationship between work intensity and academic performance.¹⁰ Findings by Hotz et al. (2002) suggest that even favorable work effects on wages in early adulthood may be spurious. In their most rigorous specification (their “dynamic selection control” model), the returns to work experience during high school were small and non-significant.

Theories that specify spurious mechanisms include precocious development and propensity theories. Although there is resemblance with respect to the underlying mechanism that accounts for the correlations among diverse problem behaviors (discussed below), the theories differ in one very important respect: whether the underlying structure of problem behavior is unique to the period of adolescence, or manifests itself in a stable way across all life stages. These two theories are illustrated in Figure 16, and are reviewed below.

*** Figure 16 Here ***

Precocious Development Theory. Precocious development theory draws attention to the life stage of adolescence *per se*, emphasizing the changes in affiliations, identities, and responsibilities that correspond with a stage-appropriate “drive toward autonomy.” This perspective attributes the positive association between intensive work and problem behavior to the overly hasty transition to adult-like roles and behaviors that coincides

¹⁰ This is an interesting conclusion in light of the fact that Warren et al. (2000) used the same data set as Tyler (2003), but arrived at a different conclusion. Moreover, both used an instrumental variables estimation strategy (simultaneous equations in Warren et al.’s study, single equation in Tyler’s study). Warren and colleagues concluded that work intensity has no impact on senior-year grades in academic courses, whereas Tyler concluded that higher work intensity negatively influences math and reading test scores. One possible explanation is that Warren et al.’s conclusion is a methodological artifact, a consequence of questionable exclusion restrictions.

with emancipation from emotional and financial independence upon parents. According to this perspective, intensive employment is one symptom of a broader, latent, stage-specific propensity to expedite the transition to adulthood: “The underlying motivation is to engage in adult behaviors that are rewarding at an age that is generally considered to be premature for such behavior” (Newcomb and Bentler, 1988:39). The issue is thus one of early timing of transitions, rather than the simple occurrence of transitions.¹¹ This notion has been variously referred to as “pseudomaturity” (Greenberger and Steinberg, 1986), “premature affluence” (Bachman, 1983), “transition proneness” (Jessor and Jessor, 1977), and “precocious maturity” (Newcomb and Bentler, 1988). Each refers to “the appearance but not the substance of maturity” (Greenberg and Steinberg, 1986:5), or the idea that adolescents have not yet acquired the capabilities and maturity necessary to assume adult roles and responsibilities.¹²

The idea of early and intensive work as one symptom of a precocious maturity is consistent with a growing body of literature that links youth employment with such activities as dating, sexual intercourse, pregnancy, alcohol use, and smoking (Bachman and Schulenberg, 1993; Ku et al., 1993; Mihalic and Elliott, 1997; Resnick et al., 1997). Much of this problem behavior is normatively age graded, such that they are only considered “problem behavior” when engaged in by teenagers. Accordingly, early and intensive involvement in the youth labor market simply constitutes one of the

¹¹ True to most developmental accounts, then, there is the implicit assumption of a single, “normal” or “ideal” course of development that is accelerated or foreshortened in precocious youth.

¹² Hirschi (1969) also touched on the notion of “premature claims to adult status.” According to his social control theory:

[C]laims to adult status may be seen as an *orientation* toward adult activities which may or may not be expressed in actual indulgence in these activities.... To claim the right to act contrary to the wishes of adults is to express contempt for “their” expectations, which...is to free oneself for the commission of delinquent acts. (1969:166, emphasis in original)

Hirschi thus theorized that “premature adulthood” affects delinquency indirectly through its impact on attitudes toward adult expectations.

accoutrements of a desired “pseudo-adult” lifestyle and the precocious youth’s perceived ticket to independence. In reality, however, this independence manifests itself more as a form of “hyper-independence,” or what Campbell (1969) refers to as “normative excess.” Newcomb and Bentler (1988:216) characterize this normative excess well by relating precocious development to either “extreme versions of those developmental tasks confronted in adolescence or else...ones that should normatively be faced as adults.” However, Jessor et al. (1991) propose that the overall course of development during the transition to adulthood is toward greater conventionality (e.g., higher value on achievement, lower value on independence, lower tolerance of deviance), particularly among those who were the least conventional as adolescents. Thus, by late adolescence or early adulthood, precocious youths “mature out” of problem behavior as they assume conventional work and family roles.

Newcomb and Bentler (1988) point out that a dominant theme of precocious development theories is the inability to delay gratification, in that the more rewarding aspects of adulthood as perceived by youths are sought (and encouraged and respected by peers) at the expense of the more difficult tasks responsibly gained through experience and maturity. This suggests that youth employment provides a way to circumvent the “typical” maturational sequences without acquiring the growth that enhances success in these adult-like roles, thus increasing the likelihood of failure in these roles (1988:35-36). Bachman (1983) also draws attention to the theme of immediate gratification in adolescent spending patterns, observing that the typical high-school student is likely to use most or all of her earnings for discretionary spending. He finds that high-school seniors report spending a substantial portion of their earnings not on saving for college,

car payments, household expenses, or other long-term goals, but rather on clothing, stereos, music, eating out, recreation, and other personal expenses, or on what he refers to as “dressing well and having a good time” (1983:66). Bachman interprets this pattern of spending earnings on immediate sources of pleasure and a lifestyle of leisure as an indicator of a diminished ability to delay gratification.

Propensity Theory. Propensity theories propose that intensive youth employment and such correlates as problem behavior, school disengagement, poor grades, diminished aspirations, emotional distance from parents, and exposure to delinquent peers are all diverse manifestations of a single underlying tendency that is established early in life and remains relatively stable over time. Gottfredson and Hirschi (1990:177) refer to this proclivity as low self-control, or the tendency to pursue short-term gratification without consideration of the long-term consequences of one’s actions. According to their theory, youth employment and its adverse outcomes are simply manifestations of the “versatility” of individuals with low self-control. Wilson and Herrnstein (1985) would also be classified as a propensity or “ontogenetic” theory. Abstracting from their biosocial theory, intensively employed youths have certain traits, such as low intelligence and impulsive personality, which make them less attuned to the long-term consequences of their actions. Their sense of immediacy, in combination with the failure of conventional socializing institutions (primarily the family) to reinforce internal inhibitions (i.e., a “conscience”) against deviance, leads these youths to take advantage of the short-term benefits of intensive employment and antisocial behavior (e.g., autonomy, earnings, social status) at the expense of the long-term costs (e.g., lower grades, dropout, arrest).

A growing body of empirical research provides some support for the notion that stable differences between individuals—whether low self-control or some other construct—account for at least part of the observed correlation between intensive work and problem behavior. Of particular relevance is the finding by Agnew (1986) that youths with low self-control work longer hours, net of sources of informal social control. Studies that include a measure of low self-control as a covariate find that this construct predicts problem behavior net of work involvement (Wright et al., 1997, 2002). Studies that include a lagged measure of problem behavior—often used as a proxy for population heterogeneity or “propensity”—consistently find that prior problem behavior is a strong predictor of present problem behavior (Mihalic and Elliott, 1997; Mortimer et al., 1996; Paternoster et al., 2003; Ploeger, 1997; Wright et al., 2002). Paternoster et al. (2003) are able to “knock out” the intensive work effect on problem behavior by introducing individual fixed effects, suggesting that between-individual differences that are stable over time explain the correlation between the two.

Propensity theory differs from precocious development theory in one important respect—the underlying “propensity” is generally presumed to be stable rather than transitory. This distinction is especially important given that both theories draw upon a concept of the inability to delay gratification as the basis for the relationship between intensive youth work and problem behavior. According to precocious development theory, the positive association is motivated by the maturity gap and the “storm and stress” of adolescence. According to propensity theory, the positive association is rooted

in biological predispositions and poor socialization that are set early in life (by age 8) and remain relatively stable thereafter.¹³

RACE, THE URBAN UNDERCLASS, AND THE POTENTIAL BENEFITS OF INTENSIVE YOUTH EMPLOYMENT

The literature reviewed to this point implies that there is theoretical agreement that a heavy work commitment is detrimental for involvement in adolescent problem behavior.¹⁴ The truth is, however, that criminological theory is not uniformly arrayed against intensive youth work. Indeed, Paternoster et al. (2003) show that the same theories often predict both beneficial and adverse effects of intensive work on problem behavior. For example, if working facilitates strong attachment to conventional employers and other workplace adults and socialization into the conventional order, social control theory predicts a negative association with problem behavior. To the extent that employment provides a source of money and thus alleviates the negative emotional state accompanying the inability to acquire desired goods (e.g., nice clothes, CDs, movies, etc.), general strain theory likewise proposes that intensive work reduces problem behavior. Employers and other well-meaning adults in the workplace who provide a source of conventional socialization and mentoring might also contribute to reduced problem behavior, as predicted by learning theory. To the extent that a heavy

¹³ Newcomb and Bentler (1988:38) speculate that the source of the inability to delay gratification that is the source of precocious maturity “may stretch back into childhood,” in which case their theory is entirely consistent with the propensity theory of Gottfredson and Hirschi (1990). However, they also entertain the possibility that it derives from social sensitivity to pressure to appear grown-up. In the final analysis, their theory remains agnostic about the actual source of precocious maturity. Therefore, because of its similarity with other developmental perspectives, I opt to interpret it as a “maturity gap” theory rather than as a “low self-control” theory.

¹⁴ This is not because delinquency theorists have explicitly incorporated youth employment into their theories. In fact, only two theories have done so: precocious development (Bachman and Schulenberg, 1993; Greenberger and Steinberg, 1986) and social control (Hirschi 1969, 1983).

work commitment after school and on the weekends precludes “hanging out” in peer-dominated social settings, routine activity theory suggests that intensive work reduces problem behavior. In many ways, then, it is only through *post hoc* theorizing that criminologists can explain the positive correlation between intensive youth work and problem behavior.

Empirical accounts provide some basis for the assertion that such social-structural factors as race and socioeconomic risk level moderate the relationship between youth employment and related outcomes. By virtually all accounts, adolescent employment is a suburban white, middle-class phenomenon (Carr et al., 1996; Greenberger and Steinberg, 1986; Marsh, 1991; Mortimer, 2003; National Research Council, 1998; Rothstein, 2001; Schoenhals et al., 1998; U.S. Department of Labor, 2000). There is no shortage of evidence that the availability of so-called “naturally occurring” jobs is structured to a large extent by family background (two parents, higher income, suburban residence, parental employment and high-status occupation) and local employment opportunities (retail jobs, employment growth, unemployment, residential segregation, racial discrimination), which provide a “resource advantage” to suburban white over urban minority youths (see Deseran and Keithly, 1994; Larson and Mohanty, 1999; Lewin-Epstein, 1986; Michael and Tuma, 1984; O’Regan and Quigley, 1996; Stoll, 1998).¹⁵ Moreover, since white youths begin working at a comparatively younger age, whites have acquired a larger stock of work-related skills as well as an enlarged employment network

¹⁵ Gardecki (2001) finds that, in a multivariate model, “family variables” (e.g., family structure, parental employment, sibling employment) jointly predict the probability that a white youth will be employed in a formal job, whereas “neighborhood variables” (e.g., urbanicity, unemployment rate) and “access variables” (e.g., travel time to work) do not contribute net of family factors. For black youths, on the other hand, family variables do not predict formal employment net of neighborhood and access variables.

at any given age than minorities. These experiences give whites a “leg up” in the youth labor market by teaching them the “soft skills” which make them more desirable as employees and by providing them better access to employment networks and opportunities. Apel et al. (2003) entertain the possibility that, because of the fewer employment opportunities available to minority youths (due to disadvantaged family background, racial discrimination, residential segregation, less conventional self-presentation style), employers are better able to “skim” the best prospects from minority applicants. That is, the average minority worker may be generally low risk because of this differential selection process on the part of employers. On the other hand, the average white worker, because employers reach further back into the pool of applicants, may be at a comparatively higher risk for antisocial behavior.

Limited evidence also suggests that the negative “premature affluence” created by adolescent employment does not generalize to all socioeconomic contexts. Entwisle et al. (2000), for example, find that a larger proportion of disadvantaged and minority youths contribute their earnings to the family economy, suggesting that middle-class youths work for very different reasons than lower-class youths (see also Johnson and Lino, 2000). Newman (1999) adds that poor inner-city youths must often cover the marginal cost of their presence in the household (see also Sullivan, 1989).¹⁶ Thus, the meaning of adolescent employment as it relates to the generation of discretionary income, consumption, and a “lifestyle of active leisure” may be true only for relatively advantaged youths. Because of their resource advantages, these youths occupy more

¹⁶ On this point, Marsh (1991) found that using employment earnings to help support the family was inversely related to school troublesomeness.

favorable positions in youth “labor queues” (Deseran and Keithly, 1994), but are also the most likely to take for granted the potential benefits of early work experience.

Leventhal et al. (2001) suggest that for poor urban adolescents exposed to low-quality schools and residing in neighborhoods offering expansive illegal opportunities, *not having a job* may be associated with higher levels of problem behavior (see also Anderson, 1999). The findings by Good et al. (1986) support this notion. They rely on a sample of low-income 13 to 18 year olds enrolled in a crime prevention program in inner-city Philadelphia. Using monthly data on employment and arrest for 47 months, they find that being employed significantly reduces the probability of arrest. This is the only study of a contemporary sample to find that youth employment *reduces* the likelihood of problem behavior, and it is notable that it is the highest-risk sample encountered in this literature (followed by Gottfredson, 1985, who found no relationship).

Farrington et al. (1986) find that rates of officially-recorded offending were higher during periods of unemployment for 16- to 18-year-old working-class London men in the Cambridge Study.¹⁷ Conversely, they find that convicted youths experience higher rates of unemployment (or non-employment), on average, than non-convicted youths. When they condition on a prediction scale of delinquency at age 10 (e.g., low income, poor parental child rearing, low intelligence, parental conviction), they find that unemployment is significantly related to crime only among those with the most risk factors. This latter finding suggests that unemployment is criminogenic only among those with high propensity for crime, and therefore may not cause crime among generally low-

¹⁷ Although labor economists define unemployment as being out of work but seeking employment (i.e., not employed but in the labor force), Farrington et al. (1986) do not appear to make this distinction. Thus, unemployment in their study is more accurately characterized as non-employment, although the authors are unclear on this point.

risk youths. Viewed another way, employment may be associated with the largest crime-preventive benefits among high-risk youths, but may have little or no impact on crime among low-risk youths.

In accord with the foregoing quantitative findings, qualitative accounts further illustrate the strong link between level of risk and the valence of the “work effect.” Hagan and McCarthy (1997) suggest that employment is a key turning point in the youth homelessness “career.” Not only does the income from steady work provide the economic means to successfully exit the street, but embeddedness in employment institutions creates a “dissonance” that is in direct opposition to the maintenance of an active street life. The jobs that homeless youths acquire are, on the whole, low-skill, service types of work, such as fast food, janitorial, and retail jobs. Despite the uniformly low quality of these work opportunities, a larger share of time spent in the workplace corresponds with less time spent hanging out, panhandling, using drugs, stealing, and engaging in other criminal activities.

Sullivan’s (1989) comparative ethnography of groups of young males from three Brooklyn neighborhoods highlights the intersection of ecology, economic opportunity, and family capital in shaping employment and crime decisions during the teenage years. Although youths from all three neighborhoods engaged in similar amounts of expressive violence and exploratory theft during their early teens, only the youths from the two poor, minority neighborhoods (La Barriada, Projectville) sustained their involvement in burglary, larceny, and robbery as a primary source of income through their middle and late teens. The more pervasive legitimate economic opportunities and neighborhood-based employment networks in the most advantaged neighborhood (Hamilton Park)

contributed to these youths' earlier desistance from economic crime as they acquired better paying, skilled manual jobs. Sullivan also draws attention to a secondary benefit of these more expansive youth work opportunities:

Labor market segmentation affects the career patterns described in this study not only by providing competitive labor market advantages to one group but also by weakening social controls in the other two. The unionized workers of Hamilton Park enjoy relatively greater job security and pay, which allow them to maintain more two-parent households and a more stable neighborhood environment. Youth joblessness in the minority communities makes economic crime more attractive, while adult un- and underemployment contribute to a weakened social control environment. (1989:226)

Newman (1999) suggests that intensive employment provides an important sense of order and structure in otherwise disorganized inner-city environments, even employment in low-wage and unskilled jobs such as fast food work. For high-risk youths, the structure and discipline of the workplace tend to permeate other institutional settings, particularly the school. The Harlem "Burger Barn" owners in Newman's study further facilitated the connection between employment and schooling by monitoring report cards, paying for books, sponsoring tutoring programs, offering monetary rewards for good grades, and threatening to cut back on the work hours of young people who performed poorly in school.

In summary, there is some evidence that the relationship between work intensity and antisocial behavior may actually be negative for some groups of youths, particularly racial minorities and lower-class youths residing in inner cities. These youths face different employment opportunity structures and bring different family resources to the table than the suburban, middle-class, white youths that have been the focus of much of the youth employment research conducted during the last 20 years.

INTEGRATION OF EXISTING RESEARCH FROM A LIFE-COURSE PERSPECTIVE

The conceptual tools of life-course analysis provide a convenient way to integrate much of what we know from existing research about adolescent employment and its potential consequences for problem behavior. Elder (1998) highlights four principles of a life-course perspective: context, timing, interdependency, and agency. Each is defined and discussed with respect to adolescent employment in greater detail below.

The Principle of Context

Context refers to Elder's principle of "historical time and place," or the idea that "the life course of individuals is embedded in and shaped by the historical times and places they experience over their lifetime" (1998:3). Historical, spatial, and socioeconomic contexts are important facets of youth employment. From the standpoint of historical context, virtually all of the existing adolescent employment research was conducted on samples of youths born in the 1960s and enrolled in high school during the late 1970s and early 1980s, when youth unemployment reached "crisis" proportions (see Freeman and Holzer, 1986; Freeman and Wise, 1982).¹⁸ However, the booming economy after the early 1990s offered more expansive work opportunities to a larger cross-section of youths. A historical perspective might thus suggest that the positive association between employment and adolescent problem behavior in these studies is due to the

¹⁸ For example, the Monitoring the Future cohorts have included high-school seniors from 1979 (Bachman et al., 1981) and 1980-84 (Bachman et al., 1986). The Orange County study was conducted using youths in the 10th and 11th grades in 1979 (Greenberger et al., 1981). Youths in the High School and Beyond survey were enrolled in the 12th grade in 1982 (Marsh, 1991). Youths in the National Youth Survey were enrolled in high school between 1976 and 1983 (Mihalic and Elliott, 1997). Even relatively more recent research relies on samples graduating from high school during the late 1980s to early 1990s, still prior to the time that the U.S. economy experienced its largest growth (e.g., Bachman and Schulenberg, 1993; Mortimer et al., 1996; Steinberg and Dornbusch, 1991).

employment difficulties encountered by youths as they attempted unsuccessfully to make a smooth transition into the labor market. The implication is that these early studies confound the adverse work effect with a larger secular trend of high youth unemployment. It also suggests that in more recent cohorts of adolescents employed in the middle to late 1990s, there would be no relationship or a negative relationship between employment and problem behavior.

An even longer historical view might suggest that the adverse work effect for adolescents is a relatively recent phenomenon (i.e., a period effect); a consequence of the changing nature of adolescent work (see Greenberger and Steinberg, 1986). Adolescents have become employed in larger numbers in the food service and retail service sectors, contributing to the formation of a “new adolescent workplace.” Over the same period of time, the proportion of adolescents employed in craft, factory, and farm work (the “old adolescent workplace”) has precipitously declined. Greenberger and Steinberg (1986) point to three dimensions of this historical shift. First, the educational value of youth employment has eroded to the point that it is no longer likely to provide preparation for adult work roles, thereby creating a discontinuity between adolescent and adult employment experiences. Simply put, contemporary adolescent work, concentrated as it is in the secondary labor market, is much less likely to provide an opportunity to “learn a trade.” Second, contemporary youth employment is less an economic necessity for the family than it is a convenient way for young people to acquire the symbols and accoutrements of popular youth culture out of their own pockets. For many employed youths, their earnings provide money that does not have to be accounted for (Hirschi, 1983). Third, today’s employed youths are much less likely to work alongside adults who

provide supervision, instruction, and socialization. In other words, there is an intergenerational disconnection in today's youth workplace. According to Greenberger and Steinberg, then, in recent decades there has been a transformation in the adolescent workplace in ways that are likely to interfere with, rather than enhance, healthy adolescent development. Intensive employment in this new adolescent workplace may thus produce the most problematic deviant outcomes.

From the standpoint of spatial and socioeconomic context, the retail and service industries—dual strongholds of youth work—are concentrated in suburban areas. In addition, employment while in high school is a luxury most often available to the white middle class. This is not to say that minority youths are less interested in working, as Greenberger and Steinberg (1986:20, emphasis in original) report that the “racial gap in *having* a job is far greater than the gap in *seeking* employment” (see also Entwisle et al., 2000; Newman, 1999). Ironically, it is precisely those youths that have the least need for independent sources of income—by virtue of their more advantaged family background (e.g., two parents, well educated, high-status occupation, dual-earner household)—that are the most likely to hold down a regular job during high school. For these youths, employment may be the means to fund a middle-class “party subculture” that values partying, concert going, drinking, dating, cruising, and otherwise living for the moment (see Hagan, 1991). As Ruggiero (1984:480, parenthetical material omitted) notes: “The same jobs which promote deviance among financially secure youths might actually decrease deviance among economically disadvantaged youths who truly need the income and may not find the work particularly stressful.” Accordingly, race, space, and class are key determinants of what groups of youths are employed, and in what jobs they tend to

work. Much of this no doubt stems from the opportunistic nature of the adolescent labor market, and to the simple availability (or lack thereof) of “naturally occurring” jobs.

The Principle of Timing

Timing refers to Elder’s principle of “timing in lives,” or the notion that “the developmental impact of a succession of life transitions or events is contingent on when they occur in a person’s life” (1998:3). Timing is a key element of much of the developmental research on youth employment conducted over the last 20 years, and the basis of concerns about the precocity of intensive work during high school. Early and intensive employment—before youths are fully prepared developmentally—is believed to correspond with the most detrimental outcomes, contemporaneously and prospectively.¹⁹

The concern about “off-time” entry into employment stems from a view of the family and school as the primary socializing institutions in adolescents’ lives, with the workplace taking on secondary importance until the late teenage years. This sentiment finds empirical support in Wright et al. (2002:10):

For younger adolescents, . . . embeddedness in work predicted higher levels of delinquent involvement. In contrast, just the reverse is found in the older group. For those at the end of their compulsory school experience and for those who have just completed high school, . . . work embeddedness . . . reduced contemporaneously delinquent involvement.

Recall that intensive employment is correlated with strained family relationships (less family time, less closeness, more conflict, less supervision) and school disengagement

¹⁹ The issue of timing has added significance in light of the fact that full-time (“intensive”) employment during adulthood is inversely associated with deviance and crime. This poses an interesting dilemma—that the “sign” of the work intensity effect changes during the transition to adulthood (Blumstein et al., 1986:52; Uggen, 2000b:530). This suggests that the relationship between employment and problem behavior is conditional on life stage, and thus that age is an essential component in understanding the dynamics of employment and crime. The jury is still out, however, about whether the relationship between adolescent work intensity and problem behavior is causal or spurious; that is, whether there truly is a change in the sign of the work intensity effect.

(less studying, lower class attendance, lower aspirations, lower grades, dropout), among other outcomes. Accordingly, “embeddedness” in developmentally unproductive work roles at an early age competes with family and school roles as the dominant influence in the lives of early adolescents. What some critics of early work experience recommend instead is an Ericksonian “psychosocial moratorium” that provides ample opportunity for learning, exploration, and experimentation; opportunity that is simply not available in the highly routinized and unstimulating “new” adolescent workplace (see Greenberger and Steinberg, 1986).

Related to the detrimental influence of the early timing of the work transition is the issue of the timing of the acquisition of adult-like privileges and responsibilities on the job. Staff and Uggen (2003) suggest that a “good job” in adolescence is very different from a “good job” in adulthood. They find that jobs with higher wages, higher work-derived peer status (having a job provides higher status among friends), and a higher degree of autonomy (control over time spent at work, freedom to make decisions about what to do and how to do it) are positively related to school deviance, alcohol use, and arrest, controlling for work intensity and prior problem behavior.²⁰ They conclude that work characteristics that are valued in adulthood may lead to greater problem behavior among adolescents:

Adolescents in jobs with great autonomy, social status, and relatively high wages—conditions that are positively valued in adult work—are more deviant in their senior year of high school than adolescents in jobs that are more closely supervised, provide less status with friends, and pay relatively low wages. (2003:282)

²⁰ Note also that work intensity retains a positive and significant effect on school deviance and alcohol use (but not arrest) in these multivariate models.

Instead, Staff and Uggen suggest that the “ideal” adolescent job is one that enhances rather than detracts from school performance, and that offers skills, challenges, and learning opportunities that complement academic roles and have a more direct link to future adult economic roles.²¹

A developmental over-emphasis on timing (“on time” vs. “off time”) may mask other important features of the transition into youth employment. Bushway et al. (2003) present evidence suggesting that *how* youths make the transition into work is of perhaps greater importance than *when* they do so. They construct work histories over the entire 14-17 age span with the first three waves of the National Longitudinal Survey of Youth 1997. Using a semi-parametric, group-based model of monthly work status, they encounter five distinct “transition groups” that differ in the timing and patterning of the transition to work: non-workers that remain out of the labor market until at least age 18 (21% of the sample); stable workers that transition by age 14 (6%); early-transition workers that make an orderly transition by age 16 (20%); late-transition workers that make an orderly transition by age 18 (28%); and unstable workers who fail as a group to become fully attached to the labor market by age 18 (24%). By the fourth wave, when the sample is in their late teens and early 20s, the stable workers are the least likely to engage

²¹ Ruggiero (1984:35-36, emphasis removed) reflected on the “ideal” adolescent job implied by the dominant delinquency theories of the time:

[T]he factors which can be expected to mitigate against deviance...are likely to become apparent only through work experiences which exhibit a number of particular features. Specifically, with respect to work activities, it is essential that they allow for significant skill acquisition, utilization, and refinement; advancement both within and across jobs; complementarity rather than competition with the education of in-school adolescents; cooperation, collaboration, and the development of prosocial ties among workers; responsible, autonomous action and decision-making; adequate monetary compensation, and participation in “meaningful,” non-trivial tasks—while keeping psychological and physiological distress to a minimum. With respect to work associates, it is essential that they be non-deviant, conventional role models; that they be adult and supervisory for substantial percentages of the time; that they reinforce non-deviant, conventional worker attitudes and behaviors; and that they be available for congenial non-work- as well as work-related interaction.

in criminal behavior (24%), followed by the non-workers (27%), and all other workers (29%). At the same time, the stable workers are the least likely to have dropped out of high school (3%), followed by the early- and later-transition workers (11%), the unstable workers (15%), and the non-workers (17%). Bushway and colleagues conclude that successful and orderly (i.e., stable) work transitions are the least problematic with respect to subsequent outcomes. On the other hand, failure to transition to work successfully (or to make the transition at all) may be associated with the most problematic prospective outcomes.

A life-course emphasis on timing should be attentive to the fact that the adolescent labor market is much more dynamic than its adult counterpart, as these youths have yet to become fully attached to the labor force. For example, Lorence and Mortimer (1985) show that the youngest workers (16-29) exhibit the least stable work orientations (job involvement, income, occupational status, work autonomy) during the four-year period they consider. They conclude that the initial phase of the work career manifests considerable volatility, which they attribute to a “career stage” phenomenon in which young workers have yet to find their occupational niche. Topel and Ward (1992) similarly find that work attachment following entry is extremely fragile, but that job mobility declines with experience.

Thus, employment during adolescence can be characterized as inherently unstable, while increasing age brings with it greater job stability. Numerous studies link employment instability with higher levels of deviance among adolescents. Agnew (1986) finds that the duration of the current job predicts lower levels of delinquency. Cullen et al. (1997) report that a higher rate of job switching produces higher levels of delinquency.

Mortimer and Johnson (1998; see also Mortimer, 2003) find that high-intensity work produces the most detrimental outcomes when that work is of relatively short duration (i.e., “sporadic” work). Paternoster et al. (2003) and Apel et al. (2003) similarly find that employment restricted to the summer months only—a rough indicator of short-duration or unstable work—tends to be associated with elevated levels of problem behavior.²²

The Principle of Interdependency

Interdependency refers to Elder’s principle of “linked lives,” or the idea that “lives are lived interdependently, and social and historical influences are expressed through this network of shared relationships” (1998:4). One aspect of interdependency that may have particular salience for youth employment is the density of job referral networks through parents, siblings, other family members, friends, and acquaintances. These may be especially important in inner cities beset by a dearth of employment opportunities. The youths in Newman’s (1999) study lament that “what you know” is often of secondary importance to “who you know” when looking for a job. For example, she finds that jobs are in such short supply, and the pool of eligible workers so large, that business owners often exploit the social networks of existing employees to cut down on

²² Other studies find similar effects for adults, suggesting a general process in which job instability aggravates involvement in a variety of deviant and antisocial behaviors for both adolescents and adults. In other words, the positive relationship between adolescent employment and problem behavior may be due less to the timing of work entry than to the fact that adolescent work involvement is inherently less stable. Sampson and Laub (1993), for example, found that job stability during the 17-25 and 25-32 age spans (composite of employment status, stability of most recent employment, work habits) was strongly correlated with excessive alcohol use, general deviance (gambling, illicit sexual behavior), and arrest among official delinquents and non-delinquents in the sample of Glueck men. Nagin and colleagues (Nagin et al., 1995; Nagin and Land, 1993) found that the “high-rate chronic offenders” in the Cambridge Study had the least stable work records and the highest rate of unemployment during the late teens. Witte and Tauchen (1994) found that the proportion of each year employed during the 19-25 age span reduced the probability of arrest among the men in the 1945 Philadelphia birth cohort. Fagan and Freeman (1999) found that the number of weeks worked in 1979 had a stable, negative relationship with the probability of incarceration at four time periods (1980, 83, 86, 89) among men in the National Longitudinal Survey of Youth 1979.

the cost and trouble of looking for new workers. One deviance-related possibility is that youth jobs acquired through personal connections may be associated with reduced antisocial behavior than jobs acquired through a “walk-in” application process. This is because there is greater interest in “keeping one’s nose clean” associated with the former type of jobs, because one’s personal reputation, as well as the reputation of the individual who makes the referral, is jeopardized by misconduct inside and outside of the workplace.

Granovetter (1973) draws attention to the strength of neighborhood-based social ties, suggesting for our purposes that weak ties may improve access to job networks relative to strong ties. He finds weak ties to be associated with broader diffusion of information, because they “bridge” people who move in different social circles. In other words, weak social ties promote the diffusion of more information than strong social ties which link similar individuals to one another. The implication is that youths with a broader web of “friends of friends” may develop the kind of job contacts that lead to more desirable work opportunities. Elliott and Sims (2001) find that, in addition to neighborhood effects on access to job referral networks, there are equally important cultural differences in the quality of these personal contacts and in how they are exploited to actually obtain jobs:

[W]hen barrio residents look for and acquire jobs, they tend to use contacts that are close to themselves socially (strong ties), spatially (neighbors), and organizationally (insider referrers); whereas, when ghetto residents look for and acquire jobs, they tend to rely on contacts that are further from themselves socially (acquaintances) and spatially (non-neighbors). (2001:355, emphasis removed)

A second component of interdependency concerns the adolescent workplace itself. Greenberger and Steinberg (1986) note that youth jobs have become ever more age

segregated. The lack of intergenerational contact in today's adolescent workplace is no doubt attributable in part to the fact that many of these jobs are simply unappealing to adults, being as they are unskilled, minimum-wage, part-time jobs with irregular work schedules. These prototypically "teenage" jobs (e.g., fast food worker, grocery bagger, cashier), because they bring together a large number of young people both during and (perhaps more importantly) after work, might be expected to correspond with higher levels of antisocial behavior. Other jobs characteristic of the "old adolescent workplace," on the other hand, may reduce problem behavior because they facilitate meaningful relationships with adult co-workers and supervisors.

A third aspect of interdependency touched on by Newman (1999) is the possibility that, in disadvantaged communities, working long hours may narrow youths' friendship circles to contain a larger share of fellow workers as youths increasingly withdraw from non-working (and presumably, less desirable and less conventional) friends and associates. In effect, young inner-city workers may begin to substitute the disorganization and unpredictability of the street with the stability and routinization of the workplace. A similar theme is expressed by Laub and Sampson (2003), who propose that the experience of and investment in full-time employment contributes to meaningful changes in routine activities and peer relations. Over time, these changes restrict criminal opportunities, and eventually foster desistance from crime and deviance. Importantly, these changes need not be the result of conscious effort by working youths. Rather, it is quite possible that they accumulate as byproducts of "a commitment to go straight without even realizing it" (2003:278-279).

The Principle of Agency

Agency refers to Elder's principle of "human agency," or the idea that "individuals construct their own life course through the choices and actions they take within the opportunities and constraints of history and social circumstances" (1998:4). For many youths—especially those from the suburban middle class—employment serves the more immediate function of acquiring spending money. For lower-class youths attending schools of poor quality, however, employment may serve as a means of acquiring and strengthening work-related skills. Working is preparation among those youths for whom a high-school diploma is of dubious value (Newman, 1999; Sullivan, 1989). In this sense, then, intensive employment may be a planful adaptation to disillusionment with and subsequent detachment from school. This notion of agency suggests reverse causality with respect to developmental accounts which propose that intensive employment is a precursor to school disengagement and dropout.

Recent empirical work by Entwisle et al. (2000) confirms this proposition. They collect information on family background, work history, and school performance for a sample of Baltimore youths from age six (kindergarten) to age 20. They find that, among males, low math and reading scores at age 8 predict semiskilled job holding at age 13 (sales, clerical, craft). Moreover, these early semiskilled jobs increase the likelihood of holding a more desirable job at age 17, but also of dropping out of high school by age 20. The authors conclude that youths (particularly males) performing poorly in school "channel their energies toward work as an alternative arena for success" (2000:292). Although this strategy is successful in the short term, Entwisle and colleagues suggest that in the long term, these early choices may backfire. Indeed, Hotz et al. (2002) find

that the returns to high-school completion for young males are substantially larger than the returns to high-school work experience (which are not significant) in their “dynamic selection control” model.

Summary

In short, upon further inspection, there is some ambiguity about the true nature of the association between intensive employment during high school and involvement in problem behavior. Some accounts suggest a positive association relationship due to causal mechanisms, others suggest a negative relationship due to causal mechanisms, and still others suggest an entirely spurious mechanism. Resolution of this problem has been limited by the use of relatively homogenous populations of school-going youths. A life-course perspective suggests that if we look past simple “intensive vs. moderate vs. no work” distinctions, we find that other characteristics of youth work besides work intensity may have more salience for participation in antisocial behavior, including workplace context, neighborhood context, work quality, and job stability.

CHAPTER THREE: DATA AND METHODS

The data used in this study are from the National Longitudinal Survey of Youth 1997 (hereafter NLSY), sponsored by the Bureau of Labor Statistics, U.S. Department of Labor. The NLSY is a nationally representative sample of 8,984 youths born during the years 1980 through 1984 and living in the United States during the initial survey year in 1997. The NLSY consists of a cross-sectional sample of respondents representative of all youths ($N = 6,748$) and an oversample of black and Hispanic youths ($N = 2,236$). The initial interview took place in 1997, and follow-up interviews were conducted in 1998 ($N = 8,386$), 1999 ($N = 8,209$), 2000 ($N = 8,081$), and 2001 ($N = 7,883$). Of the 8,984 youths interviewed in 1997, 80.2 percent ($N = 7,203$) were interviewed in all four follow-ups.

The NLSY has four distinct advantages to recommend its use for the current study. First, it is a nationally representative sample of youths, providing generalizability to the population of all youths in the United States who were 12 to 16 years of age in 1996.¹ Relatedly, all 50 states and the District of Columbia are represented, providing an opportunity to exploit cross-state variation in child labor laws. Second, the NLSY gathers information relevant to the school-to-work transition, collecting an impressive amount of detail on adolescent work histories. Third, the NLSY has a special self-report crime and substance use module administered on an annual basis, unlike its predecessor, the National Longitudinal Survey of Youth 1979. Fourth, the respondents have been assessed

¹ Due to the oversampling of black and Hispanic youths, all youths ages 12 to 16 in the United States did not have an equal probability of being selected to participate in the NLSY. In order to take into account this differential selection probability, and to generalize to the population of all youths, each respondent is assigned a normed sampling weight that is applied in all statistical analyses. Since observations are pooled across five interviews, I use the sampling weight from the first interview for each youth. Constructed in this way, the sampling weights are constant within panels (individuals).

annually for five years, providing a unique opportunity to examine changes in employment and problem behavior as youths mature.

KEY MEASURES

Descriptive statistics for all measures are provided in Table 2. A full list of variable definitions is also available in Appendix A.

*** Table 2 Here ***

Problem Behavior

There are two problem behavior outcomes of interest in this study: crime and substance use. Each is a summary scale of multiple items, listed along with definitions in Table 3.

*** Table 3 Here ***

Annual crime is a composite of 22 self-report offenses. This measure spans relatively minor property crimes such as vandalism and shoplifting, major property crimes such as auto theft and burglary, violent crimes such as robbery and aggravated assault, drug crimes such as selling marijuana and hard drugs, economic crimes such as receiving illegal income from stolen property, and arrest for a delinquent or criminal offense. For each behavior, an indicator of participation is constructed such that youths who report engaging in the problem behavior on at least one occasion since the last interview (or ever prior to the initial interview) are coded “1,” and all other non-participating youths are coded “0.” The measure of annual crime is a variety scale

constructed as the sum of all 22 binary indicators of participation.² The range of possible values for this measure is from zero to 22.

Recent substance use is a composite of four self-report substance use items referenced during the 30 days prior to the interview: smoking cigarettes, drinking alcohol, using marijuana, and drinking five or more drinks in a single setting (binge drinking). As with annual crime, an indicator of participation for each behavior is constructed such that youths who participated on at least one occasion are coded “1,” and all other non-participating youths are coded “0.” The measure of recent substance use is a variety scale constructed as the sum of these four binary indicators of participation.³ The range of possible values for this measure is from zero to four.

Table 4 provides detailed descriptive statistics for the problem behavior measures. Pooled across all five interviews, over one third of person-year observations (33.4%) report engaging in at least one delinquent or criminal behavior since the previous interview (or ever prior to the 1997 interview). There is a roughly linear decrease in offending over the five waves, such that from the first to the fifth wave, crime prevalence decreases from one-half (52.6%) to one-quarter (23.1%). Recent substance use occurs in half (50.4%) of person-year observations. In contrast to crime, there is a roughly linear increase in substance use over the five waves, with prevalence rates increasing from approximately one-third (29.7%) to two-thirds (66.1%) from the first to the fifth waves, respectively.

*** Table 4 Here ***

² Cronbach’s alpha by interview year: $\alpha_{1997} = .85$, $\alpha_{1998} = .86$, $\alpha_{1999} = .84$, $\alpha_{2000} = .85$, $\alpha_{2001} = .83$.

³ Cronbach’s alpha by interview year: $\alpha_{1997} = .77$, $\alpha_{1998} = .75$, $\alpha_{1999} = .75$, $\alpha_{2000} = .75$, $\alpha_{2001} = .72$.

A more useful statistic is provided in the “within person” column of Table 4. This suggests that over two-thirds (68.6%) of youths in the NLSY reported committing a crime during at least one of the five interviews, committing 2.76 crimes, on average, when they are active. Over three-quarters (78.9%) of youths have recently engaged in substance use during at least one of the five interviews, using 2.03 different substances, on average, when active.

Figure 17 graphs the “marginal” distributions of annual crime and recent substance use by age, plotting actual and fitted values (using a third-order polynomial in age). These figures are adjusted for exposure time. The expected “age-crime curve” is observed in this sample, with self-report criminal behavior reaching a peak at 16.5 years of age (or 17.25 if crime is modeled as a quadratic in age). Substance use, on the other hand, increases in a practically linear fashion from age 12 to age 22, but evidences a leveling off beginning at age 21.

*** Figure 17 Here ***

Formal Work Hours during the School Year

The key independent variable in this analysis is the number of hours worked per week in a formal job during the school year. The NLSY distinguishes between two types of employment: informal work and formal work. Formal (“employee”) jobs are defined as “a situation in which the respondent has an ongoing relationship with a specific employer,” while informal (“freelance”) 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” (Center for Human Resource

Research, 2002:96). Formal jobs are thus traditional “paycheck” jobs, while informal jobs are traditional teenage “odd jobs” such as babysitting, yard work, and newspaper delivery. Because child labor laws regulate formal employment but not informal employment, this analysis focuses on the former type of work.

Beginning with a youth’s 14th birthday, the NLSY 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 hours during each calendar week, accounting for within-job gaps in employment due to layoff, pregnancy, leave of absence, etc.⁴

Annual school-year work hours are operationalized as the mean number of hours worked per week in a formal job during the school year (from the week containing September 1 to the week containing May 31) since the previous interview (or since age 14 at the initial interview). Because recent substance use has a reference period during the 30 days prior to the interview, I create a corresponding measure of *recent school-year work hours*. The reference period for this measure is the four weeks prior to the interview week.

Constructed in this way, the reference period for school-year work hours matches the reference period for the corresponding response variables used in the analyses.⁵

Table 4 provides detailed descriptive statistics for the work intensity measures.

When considered within persons (final column), over four in five NLSY youths are

⁴ No information is gathered by the NLSY on formal employment of 12- and 13-year-old youths. Thus they are categorized as formal non-workers by definition. Fortunately, this is not as large a source of measurement error as one might first suspect. Statutorily, employers subject to the child labor provisions of the Fair Labor Standards Act (and many state child labor laws) are forbidden from hiring youths under 14 years of age, with but a few exceptions (e.g., youths employed in family businesses and certain types of farm work, youths employed as actors or performers) (refer to U.S. Department of Labor, 2000:3-13, for a detailed overview). Empirically, note that in Figure 5, only 6.8 percent of youths are employed in a formal job at the age of 14 (either a formal job, or in both formal and informal jobs). Fitting a linear trend from age 14 to the month before turning 16 and extrapolating to earlier ages suggests that the formal employment rate is -0.8 percent at age 13 and 3.2 percent at age 13.5.

⁵ As a way to minimize the impact of outliers, I censored work hours at the 99th percentile.

employed during the school year, with 90.0 percent having ever worked since age 14 and 80.6 percent having worked recently (in the four weeks prior to the interview week). The typical employed youths tends to work at high intensity during school as well, with a mean 24.63 hours per week on an annual basis, and 24.96 hours during the previous month.

Figure 18 plots the “marginal” distribution school-year work hours by age. Work intensity increases steadily from 14 to 19 years of age, at which point it begins to level off.

*** Figure 18 Here ***

Individual Control Variables

I incorporate several individual-level control variables. *Age* is the respondent’s age in years (continuous) as of the interview date. *Household size* is the number of individuals that currently live in the household. *Educational attainment* is operationalized as a set of mutually exclusive dummy variables: (1) currently enrolled in high school (the reference category); (2) high-school dropout or GED; or (3) high-school graduate with or without college experience. *Family structure* is operationalized as: (1) two parents (the reference category); (2) single mother; (3) no mother figure; or (4) live independently. *Residential location* is operationalized as: (1) suburban area (the reference category); (2) central city; or (3) rural area.

Because the 1997 annual problem behavior indicators measure lifetime prevalence (i.e., having *ever* engaged in the behavior prior to the initial interview) rather than annual prevalence (i.e., having engaged in the behavior since the last interview), and

the amount of time between interviews varies from as little as two months to as long as two years, I control for differential “exposure.” *Exposure time* is thus the number of years (continuous) since the last interview (or the youth’s age at the first interview).

County Control Variables

I incorporate several county-level control variables in order to adjust for local labor market conditions that might differentially affect youth employment. *Local unemployment rate* is the unemployment rate for the labor market in which the youth resides, available in the geocoded NLSY data.⁶ The remaining covariates were compiled from county-level data available from the Bureau of Economic Analysis, U.S. Department of Commerce (online at <http://www.bea.doc.gov>). *Per capita income* is personal income per capita in constant (1997) thousands of dollars. *Total number of jobs* refers to the total number of full-time and part-time jobs (thousands of jobs). I also incorporate the industrial mix of available jobs, such that *manufacturing jobs*, *retail jobs*, *service jobs*, and *government jobs* refers to the total number of these types of jobs (thousands of jobs). *Unemployment insurance* refers to the total value of the payments issued under state-administered unemployment insurance programs in constant (1997) thousands of dollars. *Medicaid, AFDC, & food stamps* refers to the total value of transfer payments issued for public assistance, AFDC, and food stamps in constant (1997) thousands of dollars.

⁶ Local unemployment refers to the unemployment rate for the metropolitan area in which the youth resides, or the balance of the unemployment rate for the state in which the youth resides (for those cases in which a youth does not live in a metropolitan area). NLS researchers compile this information from annual editions of *Employment and Earnings* for the month of March, published by the Bureau of Labor Statistics, U.S. Department of Labor. The figures are derived from the Current Population Survey.

ECONOMETRIC MOTIVATION

The goal of this study is to evaluate the plausibility of a causal relationship between work hours during the school year and adolescent problem behavior. The analytical problem encountered in this literature is that of estimating a “treatment effect” (of work hours on problem behavior) using observational data where the treatment is not allocated randomly. Since employment and problem behavior are joint products of individual decision making, the characteristics of those in the treatment group (employed or intensively employed youths) are potentially different from the characteristics of those in the control group (non-employed youths). These differences exist in characteristics which, from the point of view of the analyst, are both observable (e.g., age, gender, race, income, family structure) and unobservable (e.g., ability, motivation, delinquent propensity). This is the well known selection problem encountered in observational (i.e., non-experimental) studies.

The debate revolves around the issue of identification (see Manski, 1995:21-50). The cross-sectional “work effect” on problem behavior observed in extant research may arise because (1) exposure to the world of adult work at high intensity induces participation in problem behavior through a variety of direct, indirect, or interactive mechanisms (the “treatment effect” or causal mechanism), (2) youths with a higher-than-average propensity to work intensively have correspondingly higher-than-average propensity to engage in problem behavior (the “unobserved heterogeneity” or spurious mechanism), or (3) some combination of both causal and spurious processes.

The nature of the identification problem can be illustrated using the following two-variable model:

$$y_i = \alpha + \beta x_i + \varepsilon_i$$

where y_i represents the problem behavior outcome of interest and x_i represents a measure of work hours assumed to vary independently of ε_i , the disturbance. The least squares (LS) estimator of β yields

$$b_{LS} = \frac{\text{Cov}(x_i, y_i)}{\text{Var}(x_i)} = \beta + \frac{\text{Cov}(x_i, \varepsilon_i)}{\text{Var}(x_i)}$$

As the second equality shows, in order for b to yield an unbiased estimate of β , the numerator of the second term must be equal to zero; that is, $\text{Cov}(x_i, \varepsilon_i) = 0$. In other words, there can be no relevant variables omitted from the model that are correlated with both x_i and y_i . Failure to meet this assumption introduces bias in the estimated work effect, implying $E(b_{LS}) \neq \beta$. Moreover, in the presence of this endogeneity the least squares estimator is also inconsistent, implying $\text{plim} b_{LS} \neq \beta$.⁷

Paternoster et al. (2003) frame the selection problem as an issue of omitted variable bias, or specification error (see also Heckman, 1979). In previous studies, a typical solution to the selection problem is to estimate a multivariate model that includes a cluster of covariates thought to capture known pre-employment differences among youths at varying work intensities. If these observed covariates adequately account for differences across youths in the propensity to work at differing levels of intensity, the estimated work effect is unbiased. Unfortunately, controlling for *selection on observables* may not entirely resolve the identification problem. If *selection on unobservables* is also part of the data-generating process, the estimated work effect will remain biased (see Heckman and Hotz, 1989).

⁷ It can also be shown that, if ε_i is correlated positively with x_i , the estimated work effect β is positively biased, such that $E(b_{LS}) > \beta$ and $\text{plim} b_{LS} > \beta$.

The issue of identification is both a theoretical and empirical problem. To date, criminological theory is not sufficiently refined to resolve the identification problem, as the same theory can often be applied to explain the presence as well as absence of a work effect, not to mention different signs of the work effect. This implies that selection on observables is not a satisfactory solution to the identification problem, since it may not be possible to control for all potential sources of heterogeneity through the use of selection on observables. Fortunately, empirical strategies can be used as one solution to the identification problem. Two empirical strategies are of particular relevance for the current study: the instrumental variables and fixed-effects estimators.

Instrumental Variables (IV) Estimator

One possible empirical solution to the identification problem is to invoke the methodology of “natural experimentation” using instrumental variables (IV) estimation. IV estimation exploits exogenous cross-sectional variation in the treatment variable (in this context, school-year work hours) that is induced by the instrument in order to identify the desired estimate of the treatment effect. The goal is to “exploit situations in which the forces of nature or government policy have conspired to produce an environment somewhat akin to a randomized experiment” (Angrist and Krueger, 2001:7). The crucial identifying assumption is that the proposed instrument(s) not be directly related to the outcome (problem behavior), except through the endogenous regressor (work hours). In other words, the instrumental variable must be uncorrelated with the residual in the structural equation (work hours \rightarrow problem behavior). This idea is

illustrated in Figure 19. When this assumption is met, the IV estimator provides a consistent estimate of the treatment effect.⁸

*** Figure 19 Here ***

We can therefore define a random variable z_i as an instrument for x_i , with which we can derive the IV estimator of β . A useful way of understanding IV estimation is in terms of an exclusion restriction, in which the instrument affects the outcome only indirectly through the treatment variable (Winship and Morgan, 1999). The IV estimator yields

$$b_{IV} = \frac{\text{Cov}(z_i, y_i)}{\text{Cov}(z_i, x_i)} = \beta + \frac{\text{Cov}(z_i, \varepsilon_i)}{\text{Cov}(z_i, x_i)}$$

For convenience, an alternative procedure when the number of instruments exceeds the number of endogenous regressors is to invoke the logic and method of two-stage least squares (2SLS). This intuitive procedure involves estimating the first-stage regression,

$$x_i = \gamma + \delta z_i + v_i$$

from which a fitted x_i is obtained by

$$\hat{x}_i = \hat{\gamma} + \hat{\delta} z_i$$

These fitted values are then substituted into the second-stage regression,

$$y_i = \alpha + \beta \hat{x}_i + \varepsilon_i$$

⁸ To be exact, the IV estimator provides an estimate of the local average treatment effect (LATE), or an estimate of the treatment effect only for those respondents whose behavior can be manipulated by the instruments (Imbens and Angrist, 1994). This is because instrumental variables solve the identification problem by using only part of the variation in the endogenous regressor—the part that is uncorrelated with the omitted variables—to identify the relationship between “treatment” and the outcome (Angrist and Krueger, 2001). As long as the behavior of the “compliers” is representative of those in the entire treated group, the distinction between LATE and other treatment effects is irrelevant. However, in the presence of heterogeneous treatment effects, the parameter identified by the instrumental variables may differ from the average treatment effect (ATE) of interest (see Angrist et al., 1996, and the subsequent exchange; Heckman, 1997).

to obtain an estimate of β .⁹ The 2SLS estimator thus yields

$$b_{2SLS} = \frac{\text{Cov}(\hat{x}_i, y_i)}{\text{Var}(\hat{x}_i)} = \beta + \frac{\text{Cov}(\hat{x}_i, \varepsilon_i)}{\text{Var}(\hat{x}_i)}$$

Assuming that the instruments are not correlated with the structural disturbance, the IV/2SLS estimator is consistent; that is, $\text{plim}b_{IV} = \text{plim}b_{2SLS} = \beta$.

Fixed-Effects (FE) Estimator

A second empirical solution to the identification problem is available when the same cross-sectional units are observed over time. We can write a two-variable, linear panel model in the following way:

$$y_{it} = \alpha + \beta x_{it} + \mu_i + \varepsilon_{it}$$

where y_{it} , x_{it} , and ε_{it} are defined as before, but where μ_i now represents a time-stable, unobserved component of the disturbance that is individual specific.¹⁰ This individual effect captures such between-individual differences as the inability to defer gratification, propensity for sensation seeking, disadvantaged family background, and other such time-constant risk factors of interest to criminologists. We anticipate that this persistent, unobserved heterogeneity is correlated with x_{it} and y_{it} , and thus entails one important source of omitted variable bias.

⁹ Although the 2SLS estimation procedure is not typically done in two stages as described here, it is possible to do so. Researchers must be careful to obtain the correct second-stage residual. For example, if the analyst substitutes the fitted x_i into a second-stage equation, statistical software will compute the residual as

$$\hat{\varepsilon}_i = y_i - \hat{\alpha} - \hat{\beta}\hat{x}_i$$

Instead, the correct residual should be obtained by reinserting the observed x_i as follows:

$$\hat{\varepsilon}_i = y_i - \hat{\alpha} - \hat{\beta}x_i$$

¹⁰ At the cross-sectional level and in the absence of a measurable proxy, the individual effect, μ_i , is indistinguishable from the disturbance, ε_i . With fixed-effects panel data, the individual effect is treated as a nuisance parameter and is simply swept out of the model. Substantively, then, it is inherently uninformative and is treated as a “black box.”

A distinct advantage of the fixed-effects (FE) estimator is that it relaxes the assumption of orthogonality between the explanatory variables and the individual effect, and thereby allows μ_i to be arbitrarily correlated with x_{it} (Wooldridge, 2002). This is accomplished by taking deviations from within-panel means, thereby “sweeping out” this source of omitted variables. The FE transformation proceeds first by averaging over T to obtain

$$\bar{y}_i = \beta \bar{x}_i + \mu_i + \bar{\varepsilon}_i$$

where $\bar{y}_i = T^{-1} \sum_{t=1}^T y_{it}$, $\bar{x}_i = T^{-1} \sum_{t=1}^T x_{it}$, and $\bar{\varepsilon}_i = T^{-1} \sum_{t=1}^T \varepsilon_{it}$. These within-panel means are then subtracted from the original equation:

$$\begin{aligned} (y_{it} - \bar{y}_i) &= \alpha + \beta(x_{it} - \bar{x}_i) + (\mu_i - \mu_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \\ \tilde{y}_{it} &= \alpha + \beta \tilde{x}_{it} + \tilde{\varepsilon}_{it} \end{aligned}$$

This time-demeaning procedure thereby removes the individual effect, μ_i . The FE estimator of β is thus

$$b_{FE} = \frac{\text{Cov}(\tilde{x}_{it}, \tilde{y}_{it})}{\text{Var}(\tilde{x}_{it})} = \beta + \frac{\text{Cov}(\tilde{x}_{it}, \tilde{\varepsilon}_{it})}{\text{Var}(\tilde{x}_{it})}$$

Assuming that the within-transformed regressor is not correlated with the within-transformed disturbance (a time-varying, person-specific “shock”), the FE estimator is consistent; that is, $\text{plim} b_{FE} = \beta$.

THE FIXED-EFFECTS INSTRUMENTAL VARIABLES (FEIV) ESTIMATOR

The IV and FE estimators are not without their potential shortcomings. An important weakness of cross-sectional IV techniques is that the estimated “treatment effect” is derived from variation across individuals. A related problem is that the standard

errors of IV estimates tend to be large when the sample size is small.¹¹ The FE estimator also suffers from an important weakness. Although it allows x_{it} and μ_i to be correlated, the standard assumption of orthogonality between the explanatory variables and the idiosyncratic error, ε_{it} , maintains. Non-zero correlation can be induced by the omission of relevant time-varying (i.e., dynamic) explanatory variables, including delinquent peer association, school performance, and parental supervision, for example. The resulting estimate of β remains biased and inconsistent in the presence of this dynamic endogeneity.

The fixed-effects instrumental variables (FEIV) estimator represents one potential avenue for overcoming these weaknesses of the IV and FE estimators. First, by taking deviations from within-panel means, the estimated treatment effect is derived from within-individual changes. Moreover, by pooling a cross section of observations across multiple time periods, the working sample size becomes NT , alleviating efficiency concerns. Second, the instruments are used to induce exogenous within-panel variation in school-year work intensity, eliminating concerns about its correlation with the idiosyncratic disturbance.

In order to motivate the use of the FEIV estimator, consider the following structural model:

¹¹ The variance of the IV estimator in a bivariate regression is

$$\text{Var}(b_{IV}) = \frac{\text{Var}(\varepsilon_i) \text{Var}(z_i)}{N \text{Cov}(x_i, z_i)^2}$$

Thus, in order to obtain precise estimates, either the sample size must be unusually large or x_i and z_i must be strongly correlated (Winship and Morgan, 1999). A particularly pernicious problem with IV is weak correlation between the instrument set and the endogenous regressor, which Bound et al. (1995) link to two potential consequences. First, inconsistency of IV can result from even a small correlation between the instruments and the disturbance in the structural model. Second, in finite samples, IV estimates are biased in the same direction as LS estimates. Several researchers have proposed techniques for judging instrument relevance (see Angrist and Imbens, 1995; Bound et al., 1995; Hall et al., 1997; Shea, 1997).

$$y_{it} = \alpha + \beta x_{it} + \mu_i + \varepsilon_{it}$$

where the causal parameter of interest is β , the impact of school-year work hours on problem behavior. The approach adopted here is to use the FE transformation to eliminate the individual effect, μ_i , from the model and then to choose instruments, z_{it} , for the endogenous explanatory variable, x_{it} . The transformed model is estimated using 2SLS. The first stage is

$$\begin{aligned} (x_{it} - \bar{x}_i) &= \gamma + \delta(z_{it} - \bar{z}_i) + (\mu_i - \mu_i) + (v_{it} - \bar{v}_i) \\ \tilde{x}_{it} &= \gamma + \delta \tilde{z}_{it} + \tilde{v}_{it} \end{aligned}$$

The fitted values are then substituted into the second-stage equation

$$\begin{aligned} (y_{it} - \bar{y}_i) &= \alpha + \beta(\widehat{x_{it} - \bar{x}_i}) + (\mu_i - \mu_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \\ \tilde{y}_{it} &= \alpha + \beta \hat{\tilde{x}}_{it} + \tilde{\varepsilon}_{it} \end{aligned}$$

In the context of the current problem, I generalize the model to accommodate the inclusion of a variety of control variables. The structural model of interest is a two-way error components model of the form

$$Y_{ijt} = \beta_0 + \beta_1 H_{ijt} + \beta_2 X_{ijt} + \beta_3 C_{ijt} + \mu_i + \phi_j + \varepsilon_{ijt}$$

where i indexes individuals, j indexes states (or jurisdictions with the inclusion of D.C.), and t indexes interview years. In this model, Y_{ijt} is a problem behavior outcome, H_{ijt} is the number of hours worked during the school year in a formal job, X_{ijt} is a matrix of individual control variables, C_{ijt} is a matrix of county control variables, μ_i is an individual fixed effect, ϕ_j is a state fixed effect, and ε_{ijt} is a stochastic residual.¹² For estimation purposes, I first implement a FE transformation by taking deviations from individual-

¹² State fixed effects adjust for systematic differences across states that may be correlated with child labor laws and school-year work intensity. For example, states differed in the timing of the adoption of child labor laws (see Moehling, 1999).

specific means in order to remove μ_i from the model, and I then include dummy variables representing states. The final model is of the form:

$$\tilde{Y}_{ijt} = \beta_0 + \beta_1 \hat{H}_{ijt} + \beta_2 \tilde{X}_{ijt} + \beta_3 \tilde{C}_{ijt} + \phi_j + \tilde{\varepsilon}_{ijt}$$

where the fitted within-transformed work hours (from the first stage) are substituted for their observed analog.¹³ In this model, within-individual change in problem behavior is identified on within-individual change in school-year work hours as youths are subjected to less restrictive state child labor laws with increasing age.

Instruments for Formal Work Hours during the School Year

A plausible instrument set for adolescent work hours is a series of dummy indicators for state-level child labor laws.¹⁴ The laws and the states where each are applicable are summarized in detail in Appendix B. The seven binary indicators for restrictions to formal school-year work hours can be conveniently grouped into the following categories for weekly hours restrictions during the school year:¹⁵

¹³ Since I construct the “within” transformation of all variables prior to model estimation, it is important to note that the standard errors are estimated using the wrong degrees of freedom: $df = NT - K - 1$ (where K is the number of explanatory variables). This is because they do not take into account that the within transformation requires the computation of N individual-specific means, which are themselves sample estimates. An equivalent strategy would be to include $N - 1$ dummy variable contrasts in a model containing a constant (this is the least squares dummy variable model). Therefore, the correct residual degrees of freedom are: $df = NT - (N - 1) - K - 1$. The standard errors must thus be adjusted from the obtained results by hand in the following way:

$$s'_{\beta_k} = s_{\beta_k} \times \sqrt{\frac{NT - K - 1}{NT - (N - 1) - K - 1}}$$

¹⁴ The motivation for the set of instrumental variables used in the current analysis derives from the 1992 child labor law indicators compiled by Tyler (2003:396). My choice of instruments is specifically based on his indicator for a 40-hour limit on the number of work hours per week while school is in session. Since I have a wider age range (Tyler’s sample was composed of high-school seniors), I can take advantage of the fact that child labor laws tend to become less restrictive at age 16, and then to expire at age 18. I incorporate these age-specific restrictions (under 16, 16-17, 18 or older) into the construction of the instrument set.

¹⁵ In order to compile this information, I first consulted the National Research Council (1998:194-211), which provides state child labor laws as of January 1, 1996. I then consulted the U.S. Department of

- (1) Under 20 hours maximum for youths 14-15
- (2) 20-39 hours maximum for youths 14-15
- (3) 40+ hours maximum for youths 14-15
- (4) No restriction for youths 14-15
- (5) 20-39 hours maximum for youths 16-17
- (6) 40+ hours maximum for youths 16-17
- (7) No restriction for youths 16-17

For each instrument at each interview, a youth that resides in the state where a specific law is in effect and is also in the age range encompassed by that law is coded “1,” whereas youths residing outside of the state or youths residing in the state but that are not within the eligible age range are coded “0.”¹⁶ Table 5 enumerates the seven child labor law indicators and the states where each is applicable (as of January 1, 1997). Among 14 and 15 year olds, the modal child labor law is one that limits work involvement during the school year to 18 hours per week (18 states), consistent with the federal Fair Labor Standards Act (FLSA). This is followed by laws that limit work involvement to 40 hours

Labor (online at <http://youthrules.dol.gov/states.htm>), which provides state child labor laws as of January 1, 2003. If there were discrepancies between these two sources (indicative of legislative changes between 1996 and 2003), I consulted the U.S. Department of Labor’s *Monthly Labor Review*, which summarizes state labor legislation enacted on an annual basis for the years 1996 to 2002 (Nelson, 1997-2003). This was necessary for five states (CT, KY, NM, VT, WV). The child labor law changes for these states are summarized in Table A4 in the Appendix. Note that the trend in all four states was to impose tougher restrictions on school-year employment, with the exception that, while 14 and 15 year olds in Vermont were allowed to work fewer hours, restrictions were removed for 16 and 17 year olds.

¹⁶ For example, suppose a respondent who is 14 years old at the initial interview resides in the state of California, in which minors under the age of 16 are subject to an 18-hour maximum work week during the school year, and minors between 16 and 17 years of age are subject to a 28-hour maximum work week during the school year. The following table demonstrates how the applicable child labor law indicators are coded:

Child Labor Law Indicator	Interview Year (Respondent’s Age)				
	1997 (14)	1998 (15)	1999 (16)	2000 (17)	2001 (18)
(1) Under 20 hours maximum (14-15)	1	1	0	0	0
(2) 20-39 hours maximum (16-17)	0	0	1	1	0

At the first two waves, this hypothetical youth will be assigned a value of “1” on the first child labor law indicator. By the third wave and later when she is at least 16 years old, however, she will be assigned a value of “0,” since this law is no longer applicable to her at these ages. Notice that at the third wave, when she passes out of eligibility for the first child labor law, she passes into eligibility for the second child labor law indicator. She will thus be assigned a “1” on this indicator during the third and fourth waves, but a “0” beginning at the fifth wave.

per week (10 states) and 23 hours per week (8 states). In only one state (New Mexico) is there no weekly hours restriction, but this changed in 1998. By age 16, all but four jurisdictions relax their weekly hours restrictions (CO, DC, MA, MI). Among 16 and 17 year olds, the modal child labor law is one that allows unlimited weekly hours during the school year (30 states). Among the remaining jurisdictions, common restrictions are 48 hours per week (5 states), 28 hours per week (4 states), and 40 hours per week (4 states). Table 6 provides a year-by-year summary of the proportion of the sample that is subject to each child labor law (adjusting for amendments introduced after 1997). Of particular interest is the “within person” column. The child labor law that affects work involvement for the largest proportion of youths during the five waves is one which imposes a less-than-20-hours work week (39.1%).

*** Tables 5 and 6 Here ***

Missing Data and Variance Adjustment

Appendix C summarizes the restrictions imposed to acquire the estimation sample. The first set of restrictions involves the number of respondents retained from the initial pool of 8,984 NLSY youths. Youths who are 18 years of age at the initial interview ($n = 21$) are excluded,¹⁷ followed by youths who dropped out from the NLSY after the initial interview ($n = 215$). These “*i*-wise” (or case-wise) deletions result in a cross-sectional sample of 8,765 youths retained from the initial interview. The second set of restrictions involves the handling of missing data at specific interviews. Person-year

¹⁷ For a small number of respondents ($n = 400$), the first interview was conducted in 1998. This resulted in the interview of 21 youths who had already celebrated their 18th birthday at the first wave. Since these youths are no longer subject to child labor restrictions (and thus cannot experience a change in school-year work intensity due to a change in child labor laws), they are dropped from the sample.

observations are excluded that have missing data on any of the problem behavior measures ($nt = 655$), either of the measures of formal school-year work hours ($nt = 102$), and state of residence ($nt = 227$).¹⁸ These “*it-wise*” (or cell-wise) deletions result in a pooled cross section and time series sample of 40,323 person-year observations. A final case-wise adjustment was made by excluding those respondents who have only a single valid interview after deletion of missing data at specific interviews ($n = 40$). These adjustments result in the final estimation sample of 8,724 respondents retained from the first interview, with 40,283 person-year observations. This constitutes a 97.1 percent rate of retention from the full sample of 8,984 respondents.

Among the 8,724 youths in the estimation sample, 75.1 percent ($n = 6,554$) have five complete interviews, and another 15.1 percent ($n = 1,310$) have four complete interviews. The remaining 9.8 percent ($n = 860$) have only two or three complete interviews. Appendix D describes the interview sequence of youths in the estimation sample. By construction, all respondents contribute at least two complete interviews. The mean number of interviews among these youths is 4.62 (unweighted).

There are two additional issues that must be taken into account during model estimation. First, each outcome is a discrete random variable assuming non-negative integer values, but is modeled under the assumption of a linear functional form. In reality, the appropriate functional form is non-linear, introducing the possibility of bias due to model misspecification by imposing a linearity condition. For example, annual crime

¹⁸ I excluded observations containing missing data on the key measures used in the analyses (problem behaviors, work intensity, state child labor laws). Since the proportion of missing data for the remaining individual-level control variables was well under one percent, I retained these observations and substituted the median value from the pooled cross section and time series. For several county-level control variables, the rate of missing data exceeded one percent. For these missing values, I substituted the value from the previous year. I then constructed dummy variables for those cases in which this imputation was made, and included these dummy variables in all analyses to adjust for any biases that may result from this scheme.

would be best modeled as a binomial random variable with 22 fixed “trials,” and recent substance use would be best modeled as a binomial random variable with four fixed “trials.” However, this functional form limitation is mitigated by the fact that the dependent variable is a deviation from an individual-specific mean.¹⁹ In other words, although “annual crime” in its original metric is a 22-item variety scale that is decidedly non-normal, this is not the case with the within-transformed variable.

A second analytical complexity is that respondents are clustered in primary sampling units (PSUs). This represents a problem of spatial autocorrelation.²⁰ The within-PSU clustering is a consequence of the sampling design of the NLSY. Briefly, the NLSY sample was selected in two phases (for details, refer to the Center for Human Resource Research, 2002:19-21). In the first phase, 147 non-overlapping PSUs were selected. From these, a subset of 96,512 households was chosen. In the second phase, screening interviews identified all NLSY-eligible youths in these households.²¹ Since youths residing in the same cluster tend to be more similar to one another than youths residing in different clusters, standard procedures that assume independence across units are likely to

¹⁹ In order to ensure that inference is not negatively affected by specification error, I model the reduced-form equation for each of the problem behavior outcomes using the fixed-effects Poisson estimator. In these models, the instrumental variables are included in place of the endogenous explanatory variable. The sign and significance of the IVs are informative about the direction of the “work intensity effect” at the second stage.

²⁰ The temporal or within-individual autocorrelation is addressed by sweeping out the individual effect using the fixed-effects transformation. This procedure adjusts for arbitrary within-panel error dependence.

²¹ Respondents are further clustered in households, since all age-eligible youths in each household are included in the NLSY sampling frame. As a way to ensure that the model errors are not severely biased by within-household dependence, I randomly select a single respondent from all multiple-youth households. I then re-estimate all models using this subsample. Of the 8,724 respondents in the estimation sample, 75.6 percent share the household with at least one other respondent at the first wave (not necessarily a sibling). The supplementary models are thus estimated using 6,599 youths from non-overlapping households ($NT = 30,469$).

result in underestimated model errors. I adjust for within-PSU error dependence by estimating survey-adjusted standard errors.²²

²² There is also clustering with states as a result of the use of instrumental variables measured at this level of aggregation. As of yet, however, it is not a simple matter to adjust for multiple levels of clustering. The practical consequence is to bias standard errors downward, resulting in larger *t*-statistics. However, as shown in the next chapter, the *t*-statistics on the child labor law instruments are quite large; thus the within-state clustering is of minimal substantive importance.

CHAPTER FOUR: RESULTS

It is useful to begin this chapter with a simple illustration of the joint distribution of school-year work intensity and problem behavior. Figures 20 and 21 plot fitted values derived from a regression of annual crime and recent substance use on a categorical measure of work intensity (five-hour intervals: 0 hours, 1-5 hours, 6-10 hours, etc.), controlling for age (third-order polynomial) and exposure time. Each work intensity category is entered into the model as a dummy variable. In each figure, moreover, four regression models are estimated for various subsamples to evaluate the sensitivity of the “work intensity effect” for different groups of youths: the total pooled sample ($NT = 40,238$); 12 to 18 year olds ($NT = 31,262$); 14 to 18 year olds enrolled in high school ($NT = 22,969$); and 16 to 17 years olds enrolled in high school ($NT = 12,270$).

*** Figures 20 and 21 Here ***

There is a very clear positive relationship between work hours and both problem behaviors. Consider annual crime, for example (Figure 20). Youths who are 12-18 years old who do not work in a formal job during the school year engage in 1.00 different types of crime (out of 22), on average. Youths who work the longest number of hours per week during the school year (41+ hours), on the other hand, engage in the largest variety of criminal acts, 1.77 on average. The work intensity effect is similar for recent substance use (Figure 21). Non-working 12-18 year olds engage in a mean 0.88 different types of substance use (out of four) during the last month, while the mean for the highest-intensity workers is 1.34.

Figure 22 uses a three-category measure of school-year work hours with the 12-18 year old subsample: no work (0 hours), low work intensity (1-20 hours), and high work

intensity (21+ hours). Again, there is the expected positive relationship between work intensity and problem behavior, with high-intensity workers most likely to report engaging in each of the problem behavior outcomes. Moreover, low-intensity workers are only slightly more likely to be involved in crime and substance use than their non-working counterparts. This illustration confirms the worry of scholars and policymakers that intensive employment of over 20 hours weekly—not necessarily employment *per se*—has the most serious consequences for adolescent development in the area of problem behavior.

*** Figure 22 Here ***

In the section that follows, I examine the validity of the child labor laws used as instrumental variables in the empirical analyses. Then, I present the empirical results from a series of multivariate models intended to provide estimates of the effect of school-year work intensity on crime and substance use. Following this, I conduct a series of sensitivity analyses.

EVALUATING THE VALIDITY OF STATE CHILD LABOR LAWS AS INSTRUMENTAL VARIABLES FOR SCHOOL-YEAR WORK HOURS

The strength of the FEIV model lies in the validity of the child labor laws used as instrumental variables for school-year work hours. One important criterion for a “good” instrument is that it be correlated with the endogenous regressor. In the current context, state child labor laws should influence the number of hours per week that youths are employed during the school year. In this section, I provide a simple graphical illustration of the usefulness of these laws as instrumental variables for school-year work intensity.

Following this, I incorporate the instruments into multivariate “first-stage” models of school-year work intensity.

Recall from the previous chapter that I use the maximum number of hours per week allowed during school weeks as instrumental variables. Figure 23 provides mean work intensity at two time periods, classifying youths by the child labor law in their state at each time period. In the first time period, these youths are 15 years old, while in the next time period, they are 16 years old. At each period, the (weighted) mean number of hours worked during the school year is computed since the last interview (or in the previous year if the first time period is the first interview). At age 15, there is consistency in work intensity, with most youths averaging fewer than four hours per week. It is noteworthy that 15 year olds subject to an under-20-hours restriction work slightly fewer hours, on average, than 15 year olds subject to a 20-to-39-hours restriction (2.92 vs. 3.67). At age 16, there is more variability in work hours that appears to be a function of the child labor law in the state in which youths reside. The key labor law change is that which imposes no restrictions at age 16, evidenced by the apparently larger increase in work intensity among those who, at age 15, must work fewer than 20 hours or between 20 and 39 hours per week, but by age 16 are allowed to work as many hours as they see fit.

*** Figure 23 Here ***

Figure 24 provides a more intuitive way of thinking about how change in child labor laws influence change in work intensity by graphing the change in work intensity between ages 15 and 16, classifying youths by the change in work hours allowed under state child labor laws (using those categories with a sufficient number of observations to

provide stable estimates). As expected, there is a roughly linear increase in the number of school-year work hours corresponding to larger increases the number of allowable hours from age 15 to age 16. Thus, we have graphical evidence that state child labor laws do indeed influence adolescent work intensity.¹

*** Figure 24 Here ***

First-Stage Models

Tables 7 and 8 provide fixed-effects estimates of the relationship between the child labor law instruments and school-year work intensity. In Table 7, *annual* school-year work intensity is the response variable (for use in the annual crime models), while in Table 8, *recent* school-year work intensity is the response variable (for use in the recent substance use models). I will focus only on the estimates for annual work intensity, but the substantive results are similar using the estimates for recent work intensity.

*** Tables 7 and 8 Here ***

Model 1 in Table 7 controls for exposure time, age dummies, and state dummies. The coefficients suggest that all five state child labor laws are significantly related to school-year work intensity. The coefficients for the child labor laws governing 14 and 15 year olds are quite large, while those governing 16 and 17 year olds are of a much smaller magnitude. The reader should bear in mind that these are within-individual change (from

¹ In order to gain some sense of the magnitude of the “work intensity effect” on crime and substance use implied by the use of state child labor laws as instrumental variables, I compute the ratio of the change in problem behavior to the change in work intensity for each category of change in work hours allowed at age 16. These figures are equivalent to Wald estimates of the “work intensity effect” on problem behavior (i.e., the ratio of the reduced-form estimate to the first-stage estimate). The mean (weighted by the number of observations) impact of annual school-year work intensity on annual crime using this method is -0.022 , and the mean impact of recent school-year work intensity on recent substance use is 0.051 , suggesting that longer work hours reduce crime but increase substance use. We can thus anticipate the “work intensity effect” from the FEIV models later in this chapter, as this is indeed what we find (the estimates are comparable to those in Model 1).

individual-specific means) coefficients. It would be inaccurate to claim that being subject to a child labor law mandating fewer than 20 hours of work per week is associated with an 18.63-hour reduction from youths' individual-specific means, on average, than they would otherwise work when not subject to this law. Specifically, no youth experiences a full unit increase (or decrease) in their within-individual change score for this instrumental variable at any wave. This point warrants further explanation.

In the aggregate, the most influential child labor law for 14 and 15 year olds is the restriction under 20 hours per week. For our hypothetical youth, her average change score on this instrument at the second wave (when she is 15) is 0.38, and she experiences a change in work intensity on the order of 7.08 ($.38 \times (-18.6331) = -7.08$) fewer hours than she averages over all five waves than if her work involvement were not constrained by this law. At the third wave (when she is 16), her average change score is -0.15 , which means that she experiences a corresponding change in work intensity on the order of 2.79 ($-.15 \times (-18.6331) = 2.79$) more hours than she averages over all five waves. Thus, the effect of this particular child labor law is such that when it “turns off” at age 16, our hypothetical youth works almost ten hours longer per week during the school year than she did when she was 15 ($2.79 - (-7.08) = 9.87$). The 14- and 15-year-old restrictions to 20-39 hours and 40+ hours are associated with between two and three fewer hours than average, respectively, at age 15. Among 16 and 17 year olds, the practical impact of child labor laws is very small, but is nevertheless negative, as expected.

Model 2 in Tables 7 and 8 introduces the individual-level control variables. Doing so reduces the magnitude of the child labor law coefficients for 14 and 15 year olds by about two hours, yet they remain substantively and statistically significant. Importantly,

the change in work intensity corresponding to change in child labor law eligibility is not attributable to normal aging, which is controlled in this model. Several other control variables significantly influence change in work intensity. Youths who are older experience larger within-individual increases in work intensity, although this begins to level off, as indicated by the significantly negative quadratic term. Youths who change to high-school dropout or high-school graduate status significantly increase their work involvement, as would be expected upon leaving school. Moreover, youths whose experience a change in family structure to living without a mother figure or living independently correspondingly increase their work intensity. The former result is particularly interesting, as it suggests that youths who lose a father figure from the household (through death, divorce, or separation) increase their work involvement, perhaps in order to contribute to the family economy. Oddly, residential location is unrelated to work intensity.

Model 3 adds county-level control variables intended to provide some sense of the conditions of the local labor market. Importantly, the coefficients for the instruments are minimally affected by their inclusion. Youths residing in counties that experience an increase in unemployment increase their work intensity, perhaps indicative of greater work effort or strengthened attachment in response to a slackened labor market. An increase in per capita income is also related to an increase in school-year work intensity, suggesting that an increase in the socioeconomic profile of a community leads to more expansive work opportunities for young people. On the other hand, youths residing in counties that experience an increase in the number of manufacturing jobs tend to decrease their work intensity. Since manufacturing jobs are largely off limits to adolescents, job

growth in this industry may crowd out other youth employment opportunities. For recent work intensity (Table 8), change in the number of service jobs and government jobs are additionally associated with change in the number of hours of employment, with an increase in service jobs resulting in marginally longer hours and an increase in government jobs resulting in a significant reduction in hours.

To summarize, the results in Tables 7 and 8 suggest that state child labor laws restricting the work involvement of 14 and 15 year olds during the school year are particularly effective at doing so, while laws restricting the work involvement of 16 and 17 year olds are less strongly related to school-year work hours. This suggests that the 16-year-old transition is the key one for adolescent work involvement, and that the loosening of work restrictions at this age is a primary agent responsible for the increase in the number of hours of employment. One point worth highlighting is that the impact of child labor restrictions on the work involvement of 14-15 year olds is almost identical irrespective of the number of allowable hours. That is, residing in states that impose an under-20-hours restriction is associated with the same reduction in school-year work intensity as residing in state that impose a 20-to-39-hours or a 40-or-more-hours restriction. This is not the case with 16-17 year olds, however, as the more restrictive state child labor laws (20 to 39 hours) are associated with somewhat fewer hours. This strongly suggests that the operative child labor law at ages 14 and 15 is at the federal level rather than the state level. The Fair Labor Standards Act of 1938 imposes a maximum 18-hour work week while school is in session for 14-15 year olds, and no restriction for 16-17 year olds.

Although the instrumental variables estimator is consistent, in finite samples it is known to be biased in the same direction as the least squares estimator. In Tables 7 and 8 are two test statistics that provide a useful metric for the explanatory power of the instrumental variables in each model, and thus the degree of finite sample bias. First is an F -test for the joint significance of the instrumental variables (see Bound et al., 1995). A “good” instrument set will have a comparatively large F -statistic given the number of instrumental variables, whereas structural models with a small statistic should be interpreted with caution.² Second, a partial R -square for the instruments is provided as an alternative metric, with larger values preferred to smaller ones (see Shea, 1997). By these criteria, all three models produce extremely large F -tests and respectable partial R -squares, minimizing any concern about finite sample bias.³

ANNUAL CRIME MODELS

In the first set of empirical models, I evaluate the “work intensity effect” on annual crime. There are three sections that follow, each of which implements a different estimation procedure: random effects, fixed effects, and fixed effects with instrumental variables. Specification tests are used to determine the most appropriate model. The

² Since the F -statistic for the joint significance of the instrumental variables at the first stage is wrong, I make the following adjustments:

$$F' = F \left[\frac{NT - (N - 1) - K - 1}{NT - K - 1} \right] \left[\frac{1}{\overline{deft}_{IV}} \right]$$

where the term in the first bracket is a degrees-of-freedom correction for the within transformation, and the term in the second bracket is a survey adjustment, with \overline{deft}_{IV} representing the mean design effect for the five instrumental variables.

³ A useful comparison is with Tyler’s (2003) instrumental variables model of the impact of work intensity on academic achievement. For Tyler’s seven excluded instruments, the first-stage F -statistic was 5.69 in the math score model ($N = 9,252$) and 5.90 in the reading score model ($N = 9,242$). Using Bound et al.’s (1995:450, Table A.1) approximation, the relative bias of the IV estimator to the LS estimator lies between .09 and .17 (for $\tau^2/K = 4.0$, and $K = 3, 10$).

Hausman statistic is a convenient way to compare two estimators which under the null hypothesis are consistent, with one more efficient than the other. By way of illustration, take the random-effects (RE) and fixed-effects (FE) estimators. Under the null hypothesis, RE and FE are both consistent, and the estimators differ only by sampling error. Since RE requires fewer degrees of freedom and is thus more efficient, failure to reject the null hypothesis implies that RE is the preferred estimator. Under the alternative hypothesis, only FE is consistent, and thus a rejection of the null hypothesis implies that the efficiency loss of FE is outweighed by its consistency gain relative to RE. Hausman's (1978) result led to the following Wald test for a single regressor (or in matrix-speak, a scalar):

$$H = \frac{(b_{FE} - b_{RE})^2}{S_{FE}^2 - S_{RE}^2} \sim \chi_1^2$$

This statistic generalizes to test the consistency of a parameter vector, but in the present context, the concern is with a single regressor (school-year work hours). A simple transformation produces a z -statistic (or its small-sample counterpart, the t -statistic, when a degrees-of-freedom adjustment is implemented):

$$\sqrt{H} = \frac{b_{FE} - b_{RE}}{\sqrt{S_{FE}^2 - S_{RE}^2}} \sim z$$

I also use the Hausman statistic as a test of the consistency of FE relative to FEIV. In this context, both FE and FEIV are consistent under the null hypothesis, with FE the efficient estimator. Rejection of the null hypothesis implies that FEIV is preferable to FE.

First, I rely on a random-effects (RE) estimator. This model is of the form

$$Y_{ijt} = \beta_0 + \beta_1 H_{ijt} + \beta_2 X_{ijt} + \beta_3 C_{ijt} + \mu_i + \phi_j + \varepsilon_{ijt}$$

where μ_i represents an individual effect assumed to be randomly drawn from a normal distribution with mean zero, rendering it necessary only to estimate the standard deviation of the distribution.⁴ This distributional assumption makes the RE model comparatively efficient. However, μ_i is also assumed to be independent of the regressors, meaning in the present case that unobserved, between-individual differences are assumed unrelated to the number of hours that youths work during the school year. If this exogeneity assumption is violated, RE is biased and inconsistent.

Second, I rely on a fixed-effects (FE) estimator of the form

$$(Y_{ijt} - \bar{Y}_i) = \beta_0 + \beta_1(H_{ijt} - \bar{H}_i) + \beta_2(X_{ijt} - \bar{X}_i) + \beta_3(C_{ijt} - \bar{C}_i) + \phi_j + (\varepsilon_{ijt} - \bar{\varepsilon}_i)$$

$$\tilde{Y}_{ijt} = \beta_0 + \beta_1\tilde{H}_{ijt} + \beta_2\tilde{X}_{ijt} + \beta_3\tilde{C}_{ijt} + \phi_j + \tilde{\varepsilon}_{ijt}$$

where the within transformation eliminates μ_i from the model. An important advantage of the FE model is that it relaxes the assumption of independence between the individual effect, μ_i , and the regressors. Because it makes no parametric assumptions about the distribution of unobserved heterogeneity and involves the loss of $N - 1$ additional degrees of freedom (in order to compute panel means), it is less efficient than the RE model. Nevertheless, if the unobserved heterogeneity is indeed correlated with school-year work hours, we can expect that the FE model will be an improvement over the RE model. The FE transformation is not a panacea, however, as it also requires the assumption that within-individual change in school-year work hours is exogenous. If change in school-year work hours is correlated with change in other dynamic omitted variables, FE is biased and inconsistent.

⁴ Since individuals in the sample contribute up to five interviews, ignoring the panel structure of the data will result in underestimated standard errors and thus type I inferential errors. A random-effects model estimates an autocorrelation coefficient, ρ , which accommodates equicorrelated within-panel residuals. This parameter is a quantification of the degree of unobserved heterogeneity remaining in the model after adjusting for observables.

Third, I rely on a fixed-effects instrumental variables (FEIV) estimator of the form

$$\tilde{Y}_{ijt} = \beta_0 + \beta_1 \hat{H}_{ijt} + \beta_2 \tilde{X}_{ijt} + \beta_3 \tilde{C}_{ijt} + \phi_j + \tilde{\varepsilon}_{ijt}$$

where the fitted within-individual change in work hours is substituted into the structural model. Like the FE model, FEIV relaxes the assumption of independence between the individual effect, μ_i , and the regressors, and makes no distributional assumptions about the unobserved heterogeneity. Because it is also an instrumental variables estimator, though, it is even less efficient than FE. The advantage of this model, however, is that change in child labor law eligibility produces exogenous change in school-year work hours, satisfying the assumption of no correlation between work hours and the structural disturbance.

Random Effects

Table 9 provides the results for annual crime from a series of models estimated using a random-effects estimator. This is an error components model that accommodates the panel nature of the data by allowing equicorrelated within-panel residuals. Although this model quantifies the unobservables, an important assumption is that these unobservables are not correlated with the regressors. This is thus a type of “selection on observables” model (to use the terminology of Heckman and Hotz, 1989) intended to serve as a baseline for comparison with alternative specifications.

*** Table 9 Here ***

Model 1 includes work intensity along with controls for exposure time, age dummies, and state dummies (these coefficients are not shown). This model suggests that

ten additional hours of formal employment during the school year increase the number of different crimes committed by 0.020 ($b = .0020, p < .05$). This “work intensity effect” is quite modest, but is not unlike that found in other studies. Specifically, the increase in crime with ten additional hours constitutes only 0.009-standard deviation ($.020 / 2.228$). To provide some sense of magnitude, when annual crime is evaluated at the sample mean (1.034), youths who work ten hours longer commit 1.054 different crimes, on average, representing a 1.9 percent higher rate of criminal behavior ($[1.054 - 1.034] / 1.034 = .019$).

Model 2 adds the individual-level control variables. The work intensity coefficient increases by 25.0 percent compared to Model 1, and it remains statistically significant ($b = .0025, p < .05$). Among the control variables, we observe the expected age-crime curve with a positive linear coefficient and a negative quadratic coefficient. The coefficients imply that criminal offending reaches a peak at 16.8 years of age. Youths who drop out of or graduate from high school (relative to enrolled youths) engage in significantly more criminal behavior. Youths who live with a single mother or in a household with no mother figure (relative to living with two parents) also engage in significantly more criminal conduct. Residential location is not an important predictor of criminal behavior.

Model 3 adds the county-level control variables. Work intensity retains a positive and significant coefficient in this model ($b = .0024, p < .05$). In an unexpected finding, youths residing in counties with higher unemployment have lower rates of crime. On the other hand, youths residing in counties with a larger number of manufacturing jobs or a smaller number of service jobs engage in significantly more criminal behavior. It is

unclear, however, exactly how manufacturing and service job availability translate into higher crime propensity.

The main summary point worth highlighting from Table 9 is that the adverse “work intensity effect” on criminal behavior is replicated among youths in the NLSY. In these models, however, the effect is extremely modest, constituting at most a 2.4 percent increase in criminality from the sample mean with ten additional hours (Model 2).

Fixed Effects

Table 10 provides the results for annual crime from a series of models estimated using the fixed-effects transformation. This is one type of “selection on unobservables” model, where individual fixed effects are included to sweep out unobserved heterogeneity, or time-invariant, between-individual differences in criminal behavior. In this model, identification is based on change in criminal activity corresponding with contemporaneous change in work intensity.

*** Table 10 Here ***

Model 1 suggests that a change in work intensity on the order of ten additional hours during the school year corresponds with a significant 0.048 *fewer* different crimes ($b = -.0048, p < .001$). Thus, by including individual fixed effects to sweep out unobserved heterogeneity from the model, the “work intensity effect” changes signs relative to the “selection on observables” model in Table 9. Working ten additional hours reduces annual crime by a quite modest 4.6 percent from the sample mean (.022-standard deviation).

The work intensity coefficient is similar and statistically significant in Models 2 and 3 (Model 2: $b = -.0036$, $p < .01$; Model 3: $b = -.0033$, $p < .01$). Notice that, contrary to the random-effects model, changing to dropout status corresponds with a significant decrease in criminal involvement (relative to high-school enrollment). Changing to high-school graduate status, on the other hand, is associated with a significant increase in crime. Additionally, a change in family structure to living with no mother figure is associated with an increase in crime, while a change to living independently is associated with a reduction in crime (relative to living with two parents). County control variables that reduce crime are increases in local unemployment, the number of retail jobs, and unemployment insurance payments, while an increase in the total number of jobs is associated with an increase in crime.⁵

The important lesson from Table 10 is that conclusions about the nature of the association between school-year work intensity and criminal behavior are sensitive to model specification. Specifically, a “selection on observables” model, as in the random-effects (RE) model in Table 9, suggests that high-school work intensity is related to elevated annual crime. On the other hand, a “selection on unobservables” model, as in the fixed-effects (FE) model in Table 10, suggests that youths who *increase* their work involvement tend to *reduce* their criminal involvement. Hausman tests indicate that the RE estimates from Table 9 are inconsistent relative to their FE counterparts in Table 10 (e.g., Model 3: $H = 10.73$, $p < .001$). Therefore, the RE estimates appear to be biased

⁵ It is worth comparing the fixed-effects results in Table 10 with those of Paternoster et al. (2003) and Apel et al. (2003). In these studies (also using the NLSY, but with the first three waves), the intensive work effect on delinquency was consistently negative but did not attain statistical significance. However, each used a binary indicator for working over 20 hours per week, whereas here I use the actual number of hours. Moreover, their measures of crime were operationalized differently, with Paternoster et al. using a five-item binary indicator for participation and Apel et al. using a seven-item variety scale. Here I use a 22-item variety score.

compromise estimates of between- and within-individual differences in work intensity. With respect to crime, at least, work hours are indeed of considerable predictive importance, but not in the criminogenic way that one would anticipate from a reading of existing research.

Fixed Effects Instrumental Variables

Table 11 provides the results for annual crime from a series of models estimated using a fixed-effects instrumental variables estimator. This is the second of the “selection on unobservables” models, wherein individual fixed effects are included to sweep out unobserved heterogeneity, and state child labor laws are introduced as instruments for formal school-year work hours. In this model, identification is based on exogenous within-individual change in work hours attributable to the easing of child labor restrictions as youths age.

*** Table 11 Here ***

In Model 1, the impact of school-year work intensity on crime is negative and statistically significant ($b = -.0175, p < .001$). By instrumenting for within-individual change in work hours, the “work intensity effect” takes on both statistical and substantive significance. Indeed, it is over three times the magnitude of the estimate from its fixed-effects analog (Model 1 in Table 10). An increase of ten hours produces a 0.175 decrease in crime, which represents a 16.9 percent decrease from the sample average (.079-standard deviation).

In Models 2 and 3, the work intensity effect on crime becomes even larger (Model 2: $b = -.0309, p < .001$; Model 3: $b = -.0282, p < .001$). In all other respects, this model

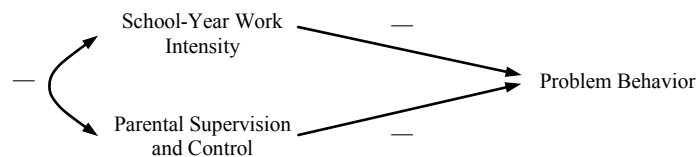
closely resembles its fixed-effects counterpart, with the exception that changing to dropout status is unrelated to crime in this model, as are local unemployment and unemployment insurance.

Thus, *instrumenting for school-year work hours using state child labor laws as instrumental variables produces a work intensity effect that is several orders of magnitude larger than that produced by a fixed-effects model*. Depending on which specification is chosen, the estimate for work intensity is over three-and-a-half times as large (Model 1) or over eight-and-a-half times as large (Models 2 and 3). The difference is due entirely to the fact that the fixed-effects instrumental variables (FEIV) estimator isolates change in work intensity that is exogenous. Both models sweep out time-invariant individual differences (by within-transforming all covariates), with the difference that FE requires the assumption that change in work intensity is exogenous conditional on observed and unobserved heterogeneity. In other words, FE assumes that there are no omitted variables that change contemporaneously with work intensity that are also related to change in crime. Violation of this assumption results in inconsistency of FE. The FEIV model relaxes this assumption by assuming *a priori* that change in work intensity is endogenous, and using instrumental variables to isolate the portion of the change in work intensity that is not correlated with the structural disturbance. In all three models, Hausman tests reject the FE model in favor of a FE model with instrumental variables (e.g., Model 3: $H = 4.10, p < .001$). Thus, the FE model is indeed inconsistent because of omission of time-varying factors that are correlated with change in work intensity and crime.⁶

⁶ There are a number of possibilities for dynamic omitted variables in the fixed-effects model. For example, change in parental attachment and supervision, school performance and commitment, peer

Of added importance is the J -statistic for overidentification, which serves as a test of the validity of the exclusion of the instrumental variables from the structural model.⁷ If the instruments have been appropriately excluded from the structural model, this chi-square statistic should not exceed 9.49 using a 0.05 criterion, or 7.78 using a 0.10 criterion. In all cases, we fail to reject the null hypothesis that the instruments satisfy the orthogonality condition for their exclusion.⁸

associations, and time use are plausible candidates. As a simple illustration, consider change in parental supervision. With the omission of this variable, the direction of the bias in β_{FE} is predictable— β_{FE} is biased upward. We know from Chapter Two that an increase in work intensity is correlated with a decrease in parental supervision as employed youths gain a greater degree of autonomy. Thus, change in work intensity is negatively correlated with change in supervision and control, which in turn is negatively correlated with change in criminal behavior. The following graph illustrates this:



If this illustration is accurate, then by not controlling for change in parental supervision, it is easy to see how the estimated impact of work intensity on crime will be biased upward. Although the true relationship between work intensity and crime may be negative, the estimated coefficient will tend to also pick up the indirect positive correlation between work intensity and crime through parental supervision (the product of two negative “paths”), resulting in a much smaller “work intensity effect” on crime. Indeed, in the fixed-effects model, $b = -.0033$, whereas in the fixed-effects instrumental variables model, $b = -.0282$ (these estimates are from Model 3).

⁷ When the number of instruments exceeds the number of endogenous regressors, the model is said to be “overidentified.” With an overidentified model, it is possible to test whether some of the instruments are correlated with the structural disturbance, and thus whether the instruments have been appropriately excluded from the structural model. In the present case, there is a single endogenous regressor with five instrumental variables, meaning that there are four overidentifying restrictions.

⁸ The J -statistic is a standard routine in the `ivreg2` procedure in Stata 8.0 (for details and access to the module, see Baum et al., 2003). Although this test is asymptotically valid, it is common in practice to make a small-sample correction. This is done in the following way:

$$J' = J \left[\frac{NT - (N - 1) - K - 1}{NT} \right]$$

A regression-based procedure to test overidentifying restrictions is recommended by Davidson and MacKinnon (1993). They suggest regressing the IV residuals on the full set of instruments (that is, both the included and excluded instruments, or x_i and z_i , respectively). The test statistic is $N \cdot R^2$ (or in the current panel context, $NT \cdot R^2$) from this auxiliary regression, which is distributed chi-square with degrees of freedom equal to the number of overidentifying restrictions.

Summary

The annual crime models estimated in this section suggest that *inferences about the relationship between school-year work intensity and criminal behavior are sensitive to the assumptions that one is willing to make regarding “unobservables.”* First, if one is willing to assume that work hours are exogenous conditional on observed covariates (the random-effects model), there is a *positive* relationship between work intensity and crime. Second, if one is willing to assume that change in work hours is exogenous conditional on individual fixed effects (the fixed-effects model), there is actually a modest *negative* relationship between crime and the number of hours worked per week. Third, if one is willing to assume that controlling for individual fixed effects is not sufficient to render change in work intensity exogenous, and that state child labor laws can be used as instrumental variables to do so, there is actually a substantial, negative relationship between crime and work hours.

All objective criteria favor the “work intensity effect” from the fixed-effects instrumental variables model. In the context of annual crime, the random- and fixed-effects estimators are biased and inconsistent due to specification error. In other words, omitted stable and dynamic variables that are correlated with work intensity and crime produce erroneous inferences in these two models about the causal impact of working. The weight of the evidence thus supports the conclusion that *longer work hours during the school year have a substantial preventive effect on criminal behavior.* Moreover, this estimate has a causal interpretation, since change in school-year work hours is rendered exogenous in the fixed-effects instrumental variables model. That is, a within-individual

increase in work involvement while school is in session causally produces a within-individual decrease in criminal behavior.

As with other research (e.g., Abe, 1999; Bachman and Schulenberg, 1992; Crowley, 1984; Cullen et al., 1997; Mihalic and Elliott, 1997; Steinberg et al., 1993; Wright et al., 1997), I find in a random-effects model that youths who work longer hours during the school year do indeed engage in significantly more crime than youths who work fewer hours (or none at all). However, the findings herein suggest that this positive relationship is attributable entirely to selection bias, not to the fact that longer work hours are a cause of more extensive criminal involvement. On the contrary, longer work hours are a cause of *diminished* criminal involvement in the fixed-effects and fixed-effects instrumental variables models. In other words, youths who work longer hours are indeed more criminal, on average, but an increase in work hours produces a non-trivial decrease in crime.

RECENT SUBSTANCE USE MODELS

In the next set of empirical models, I evaluate the “work intensity effect” on recent substance use. The progression of this analysis follows that from the previous section. I first use a random-effects estimator, followed by a fixed-effects and then a fixed-effects instrumental variables estimator. As before, I will rely on Hausman specification tests to choose the most appropriate estimator.

Random Effects

Table 12 summarizes the results from a random-effects model of recent substance use. When only limited control variables are included in Model 1, work intensity is positively and significantly related to substance use ($b = .0054, p < .001$). Specifically, working ten hours longer per week increases the number of different types of substances used by 0.054, representing a 4.9 percent higher rate of substance use over the sample average (1.103), or 0.040-standard deviation ($.054 / 1.334$).

*** Table 12 Here ***

With individual control variables included in Model 2, the work intensity effect remains positive and significant ($b = .0039, p < .001$). These control variables imply an inverted U-shaped pattern of substance use in age, although the peak is 22.7 years of age, which is outside the range of the sample. This means that substance use is increasing in age, but doing so more slowly at older ages. Youths in larger households use fewer substances, while youths enrolled in high school use fewer substances compared to those that drop out or graduate. Relative to youths living with two parents, those living with a single mother or without a mother figure tend to engage in more substance use, while those living independently use fewer substances. Residential location does not seem to predict substance use (nor crime).

In Model 3, the work intensity effect changes little with the inclusion of county control variables ($b = .0038, p < .001$). The individual control variables also exhibit the same patterns as in Model 2. Youths residing in counties with higher unemployment engage in less substance use, consistent with the results for crime. Youths residing in

counties with more manufacturing jobs also engage in more substance use, consistent with the crime models.

Thus, as with annual crime, the adverse “work intensity effect” on substance use is also replicated among youths in the NLSY. At its strongest (Model 1), this effect constitutes a 4.9 percent higher rate of substance use with ten additional hours.

Fixed Effects

Table 13 provides results from a fixed-effects model of recent substance use. Model 1 demonstrates the positive and significant relationship between within-individual change in work hours and change in substance use ($b = .0055, p < .001$). Specifically, an increase of ten hours above the mean increases substance use by 5.0 percent above the sample average (.041-standard deviation).

*** Table 13 Here ***

In Models 2 and 3, the work intensity effect remains positive and significant, and is almost identical in the two models (Model 2: $b = .0041, p < .001$; Model 3: $b = .0040, p < .001$). Unlike its random-effects counterpart, change in household size is unrelated to change in substance use, as is change to a single mother household or to living independently. On the other hand, change in residential location is significant in these models, implying that youths who change to a central city residence increase their substance use. In Model 3, change in several of the county control variables are related to change in substance use, including local unemployment, the number of manufacturing and government jobs, and unemployment insurance payments.

Thus, controlling for unobserved heterogeneity by including individual fixed effects results in relatively little change in the magnitude of the “work intensity effect” on recent substance use. Indeed, Hausman tests reject the random-effects estimates in favor of the fixed-effects estimates in only one model (Model 2), although the magnitude of the difference is barely perceptible. Therefore, *longer work hours during the school year significantly increase recent substance use*. This conclusion should be tempered, however, by the fact that the magnitude of this work intensity effect is extremely small, constituting, at most, a 5.0 percent increase in substance use with an increase of ten hours per week (Model 1).⁹

Fixed Effects Instrumental Variables

Table 14 reproduces the results from a fixed-effects instrumental variables model. Model 1 implies a work effect that is quite large relative to the random-effects and fixed-effects models ($b = .0437, p < .001$). Youths who work ten hours above average increase their substance use by 39.6 percent above the sample mean (.328-standard deviation). This estimate is approximately eight times that from the random-effects (Table 12) and fixed-effects (Table 13) models.

*** Table 14 Here ***

⁹ The conclusion from the fixed-effects model that an increase in work intensity results in an increase in substance use is contrary to the findings of Paternoster et al. (2003) and Apel et al. (2003) that intensive school-year work has no impact. As mentioned earlier, I use a continuous measure of work intensity in these analyses, rather than a binary indicator for intensive work. Additionally, the substance use response variables used in these analyses are referenced during the 30 days prior to the interview, whereas in the earlier studies it is referenced since the last interview (or ever prior to the initial interview). Moreover, I use a four-item substance use variety scale as the response variable, whereas these earlier studies used a three-item variety scale (cigarettes, alcohol, marijuana). When I re-estimated Models 1-3 using the same three-item variety scale, the “work intensity effect” on substance use was consistently positive using binary and continuous measures of work intensity. However, when I re-estimated the models using only the first three waves, I fully replicated Paternoster et al.’s and Apel et al.’s findings of null effects. It seems that the additional information available in the fourth and fifth waves (when respondents are older) alters conclusions about the impact of school-year work intensity on substance use.

The work intensity effect remains significant and of a similar magnitude in Model 2 ($b = .0445, p < .001$) and Model 3 ($b = .0429, p < .001$). The individual control variables demonstrate that change in educational attainment is also related to change in substance use, but in a different way than in previous models. Change to high school dropout status produces a significant decrease in substance use. Although a change to living independently significantly decreases substance use, other changes in family structure are unrelated. Change in central city residence is also related to an increase in substance use, consistent with the fixed-effects results.

The fixed-effects instrumental variables results for recent substance use imply a substantial adverse “work intensity effect.” In addition, in all three models, Hausman tests reject the null hypothesis that the fixed-effects estimator is consistent. Consequently, we may on first inspection conclude that the impact of longer work hours on substance use is not only positive, but of a much larger magnitude than implied by a fixed-effects model (and even random effects). This series of results is far from conclusive, however. *The overidentification tests for all three models reject the null hypothesis that the instruments are asymptotically uncorrelated with the structural disturbance, and therefore the exclusion restrictions involving state child labor laws are not valid.* Rejection of the orthogonality condition implies that the fixed-effects instrumental variables estimator is inconsistent in this context, and that it leads to erroneous inferences about the relationship between work intensity and substance use.¹⁰

¹⁰ Also available in the NLSY are measures of *annual* substance use for smoking cigarettes, drinking alcohol, and using marijuana, with reference periods since the last interview (or ever prior to the first interview). I created a three-item variety scale and re-estimated the models in Tables 12-14 using this as the response variable. All substantive results were replicated using this measure. Moreover, the overidentifying restrictions were also rejected using this measure.

Summary

The recent substance use models estimated in this section suggest that *longer work hours during the school year are associated with higher rates of substance use*. That is, the “sign” of the work intensity effect is consistently positive, irrespective of the assumptions one makes regarding “unobservables.” The magnitude varies, however, as a function of what estimator is used. The random- and fixed-effects models suggest a rather modest effect, and the fixed-effects instrumental variables model suggests a quite substantial effect. However, in this latter model, the instruments do not satisfy the orthogonality condition that permits their exclusion from the structural model. In words, the instruments are not “good” when used in the context of substance use. Because the fixed-effects instrumental variables results are invalid, the random-effects estimates are favored in Models 1 and 3, while the fixed-effects estimate is favored in Model 2 (on the basis of Hausman specification tests).

It should be noted, moreover, that because of the poor performance of the fixed-effects instrumental variables estimator, the identification problem cannot as yet be resolved. Although individual fixed effects sweep out time-stable omitted variables (“unobserved heterogeneity”), the potential presence of omitted *dynamic* variables continues to pose an inferential risk, since change in school-year work intensity remains endogenous even in the fixed-effects instrumental variables model. In other words, *the work intensity effect on substance use is not identified in these models, and thus cannot (and should not) be interpreted in a causal manner*.

SENSITIVITY ANALYSIS

In this section, I evaluate the sensitivity of the empirical estimates from previous sections by estimating a series of auxiliary models. These models take one of two forms: (1) problem behavior-specific outcomes and (2) alternative model specifications.

Behavior-Specific Models

Table 15 summarizes conclusions from a series of models in which each individual problem behavior is treated as a response variable. Recall, for example, that annual crime is a 22-item variety scale, and that recent substance use is a four-item variety scale. There are thus 26 distinct problem behavior indicators which are treated as dependent variables in auxiliary fixed-effects instrumental variables regressions. In addition to binary indicators, frequencies are available in the NLSY for seven measures of crime and all four measures of substance use.¹¹ To simplify presentation, I report simply whether or not the work intensity coefficient is statistically significant in each auxiliary model (using $\alpha = .10$, two-tailed), and if so, its direction. I also denote those models in which the overidentification test rejects the null hypothesis ($\alpha = .10$), suggesting that the instruments may not be “good” in that particular model. Model 1 is the same as before, including a control for exposure time, age dummies, and state dummies, Model 2 adds individual control variables, and Model 3 adds county control variables.

¹¹ The frequencies for five crime measures (vandalism, major theft, “other” property crime, aggravated assault, selling drugs) are referenced since the last interview, or the year prior to the initial interview. For the frequency of carrying a handgun, the reference period is the 30 days prior to the interview. For the frequency of arrest, the reference period is since the last interview, or the ever prior to the initial interview. The frequencies for all four substance use measures are referenced during the 30 days prior to the interview.

*** Table 15 Here ***

In the top panel of Table 15 under the “prevalence” column are the binary indicators of annual crime. The most important finding is that work intensity is not positively related to the prevalence of any type of crime. Note that change in school-year work intensity is negatively and significantly related to change in 12 of the 22 indicators in at least one model. These vary in seriousness to include comparatively minor property crime (petty and major theft, vandalism, petty and major shoplifting), serious property crime (vehicle theft, petty burglary), dealing in stolen property (“other” property crime, illegal income from stolen property and “other” property crime), and violent crime (aggravated assault). Work intensity appears to have no relationship with the prevalence of larceny, robbery, arrest, and offenses related to selling drugs. Particularly interesting is that for all three indicators for selling drugs, I reject the null hypothesis that the instrumental variables are appropriately excluded from the structural model; I return to this below.

Under the “frequency” column in the top panel are the annual crime frequencies. Only two of these response variables are consistently related to school-year work intensity: vandalism and major theft. There is some discrepancy for “other” property crime, aggravated assault, and carrying a gun, in that work intensity is significantly related to the prevalence but not the frequency of these criminal behaviors. Unexpectedly, longer work hours are associated with a significant increase in the frequency of selling drugs, but only in Model 1, as the addition of control variables eliminates this relationship. The pattern of results for annual crime thus suggests that school-year work hours are most consistently and strongly related to a reduction in relatively minor

property crimes such as vandalism and theft. Importantly, however, *there is no evidence that an increase in school-year work intensity increases any type of delinquent or criminal behavior, with the possible exception of drug selling.* On the contrary, work intensity consistently has either no relationship or a negative relationship with individual criminal behaviors.

In the bottom panel of Table 15 are the recent substance use measures. First, note that the work intensity effect is positive and statistically significant in all substance use models, irrespective of whether prevalence or frequency is treated as the response variable. Second, note that the test of overidentifying restrictions rejects the null hypothesis in all of the marijuana use and binge drinking models. Considering that the instrumental variables in the drug selling models in the top panel posed similar endogeneity problems, it would seem that *the comparatively more serious substance use (and selling) behaviors are ill suited to excluding state child labor laws for identification purposes.* Thus, the poor performance of the fixed-effects instrumental variables model for the recent substance use variety scale (Table 14) is attributable largely to the inclusion of marijuana use and binge drinking. On the other hand, the model appears to perform quite well for smoking cigarettes and drinking alcohol.¹²

In Table 16, I summarize the empirical results for all relevant models (RE, FE, FEIV), specification tests, and fit statistics using smoking and drinking frequency as dependent variables. In the top panel are the smoking frequency models. School-year work hours are positively and significantly related to smoking in all three random-effects

¹² To ensure that the overidentification test is not sensitive to the composition of the substance use scale, I re-estimated the recent substance use models from Table 14 using a two-item variety scale that is the sum of the indicators for smoking cigarettes and drinking alcohol in the last 30 days. The test of overidentifying restrictions was still rejected in all four models. Thus it is only when these two behaviors are treated separately that the instruments can be considered “good.”

(RE) models. This is also true in all three fixed-effects (FE) specifications, and the coefficients are very similar with the relationship slightly stronger. However, Hausman tests fail to reject RE only in Model 1, implying that RE is no less consistent than FE. When instrumental variables are introduced, the adverse “work intensity effect” becomes quite large. Model 3 results suggest that an increase of ten hours per week above the (individual-specific) mean results in an increase of 2.98 additional days (above the sample mean) of cigarette smoking in the previous month, on average, representing a 50.0 percent increase from the sample mean (.266-standard deviation).

*** Table 16 Here ***

In the bottom panel of Table 16 are the drinking frequency models. The coefficient for school-year work hours is positive and significant in all three RE models. Similarly, in all three FE models the coefficient for work intensity is significantly positive, and slightly larger in magnitude than the corresponding RE estimates (as in the smoking models). Indeed, Hausman tests fail to reject RE in all models except Model 1. In the models estimated using FE with instrumental variables, the work intensity coefficient is positive and statistically, as well as substantively, significant. Model 3 implies that ten additional hours results in 1.40 additional days of alcohol use per month, representing a 62.6 percent increase from the sample mean (.302-standard deviation).

In sum, when smoking and drinking frequency are considered separately from the recent substance use variety scale, the results favor a deleterious “work intensity effect.” Moreover, we may place more confidence in the substance use frequency models, as the empirical results using the variety scale are negatively affected by the inclusion of

marijuana use and binge drinking. In subsequent models, then, I will consider smoking and drinking frequency as response variables over and above the variety scale.

Alternative Model Specification

Table 17 summarizes conclusions from a series of models in which various functional form alternatives for work intensity are introduced. I will describe each of these efforts and elaborate on those with important implications. As before, Model 1 includes limited control variables, Model 2 includes individual control variables, and Model 3 adds county control variables. First, since maximum work intensity is 67 (annual) and 70 (recent) hours per week, I censored work hours at three different thresholds: 60 hours, 50 hours, and 40 hours. All results were identical to the original models, suggesting that the model parameters are not adversely affected by outliers.

*** Table 17 Here ***

Second, I assessed non-linearity using dummy variables and a second-order polynomial.¹³ Using a single dummy variable for work status, the “work effect” on crime

¹³ Identification is more complicated with a quadratic endogenous regressor. Kelejian (1971; Kelejian and Oates, 1989:303-329) shows that obtaining the fitted value for the endogenous regressor from the first stage and then substituting the fitted value and its square into the second stage results in inconsistency. This is because the square of a linear projection is not equivalent to a linear projection on a square (Wooldridge, 2002). Kelejian suggests augmenting the first-stage model with squares and cross-products of the instruments (see also Wooldridge, 2002:230-237). An alert reader will notice that the instruments which I employ are binary and mutually exclusive, which means that squares and cross-products do not exist (in fact, the squares do exist but are perfectly collinear with the original instruments, and the cross-products produce all zeroes). However, I augment the first stage with dummy variables for the sequence of child labor laws that regulate employment during the 14-15 and 16-17 age periods. For example, a youth residing in New Jersey may not work more than 18 hours per week during the 14-15 age period, and not more than 40 hours per week during the 16-17 age period. Imagine that we have a hypothetical 14-year-old youth residing in New Jersey at the first wave. During waves one and two, this youth will be coded “1” on the “under-20 hours restriction” dummy, and during waves three and four, she will be coded “1” on the “40 or more hours restriction” dummy. In addition, during all four waves, she will be coded “1” on a single dummy variable for an under-20 hours restriction at age 14-15 and a 40 or more hours restriction at age 16-17. This is the best approximation to a cross-product available with the instruments used here.

remains negative and significant, but positive and significant for substance use. Using two dummy variables for work intensity (low intensity and high intensity), change to low-intensity work is not significantly different from non-work, whereas change to high-intensity work is associated with a significant reduction in criminal behavior (Model 3: $b = -1.2319, p < .01$). For substance use variety and smoking frequency, both work intensity indicators are positive and significant. To illustrate, consider Model 3 for smoking frequency. The coefficient for the moderate work indicator is 12.512 ($p < .01$), and for the intensive work indicator is 4.849 ($p < .10$), and the difference between these two coefficients is statistically significant. Thus, although employment *per se* increases smoking, it is change to low-intensity work that is particularly strongly related to an increase in smoking. The pattern is reversed for drinking, as high-intensity work significantly increases drinking frequency ($b = 4.2442, p < .01$), while low-intensity work is unrelated.

In the quadratic specification for annual crime, neither the linear term nor the quadratic term is significant in all three models. In the substance use models, the linear term is consistently positive and significant while the quadratic term is negative and significant in the substance use variety models, but largely non-significant in the substance use frequency models. There is thus some evidence for a non-linear relationship between work intensity and substance use, suggesting that relatively modest increases in work intensity produce substantively large increases in substance use, but this effect diminishes as change in work intensity becomes large.

Third, I explore other model specifications by controlling for the individual's level of work hours at time t , and by taking a natural log (+1) transformation of school-

year work hours. Since identification in a fixed-effects model is based only on within-individual change, the model ignores *the mean from which this change occurs*. Thus, in a fixed-effects model, the impact of an increase of five hours is treated the same despite whether that change is from an individual-specific mean of ten hours or an individual-specific mean of 25 hours. By including the level of work hours it is possible to control, to a limited extent, these differences across youths. The semi-log model accomplishes a similar goal by accounting for the percent change from the individual-specific mean. Thus, an increase of five hours from an individual-specific mean of ten hours represents a 50 percent ($5 / 10$) increase, whereas an increase of five hours from an individual-specific mean of 25 hours represents a 20 percent ($5 / 25$) increase. In both sets of models, the empirical results replicate the original findings. The results for the semi-log models are worth summarizing in some detail. Model 3 for annual crime implies that a ten percent increase from the (individual-specific) mean results in 2.44 fewer crimes ($b = -.2440$). The corresponding estimates for the remaining response variables are 3.24 for substance use variety, 19.98 for smoking frequency, and 9.17 for drinking frequency.

The next model introduces prior problem behavior as a regressor, allowing for a genuine “state dependent” effect (Nagin and Paternoster, 1991, 2000), or a causal effect of prior problem behavior on current problem behavior. This provides an important robustness test of the “work intensity effect” since it is acknowledged that past problem behavior is correlated with current work intensity (see Gottfredson, 1985; Mihalic and Elliott, 1997; Mortimer, 2003; Staff and Uggen, 2003; Steinberg and Avenevoli, 1998; Steinberg et al., 1993). However, the models suggest that with the inclusion of a lagged Y , the “work intensity effect” remains largely intact.

The next set of results evaluates the sensitivity of the coefficients to functional form assumptions by estimating a fixed-effects Poisson model. This is a reduced-form model in which the instrumental variables are directly substituted for school-year work intensity in the problem behavior models. The coefficients in a reduced-form model are the product of the coefficient(s) on the instrumental variable(s) from the first stage with the second-stage structural estimate. Since we know from the first-stage models that the instrumental variables are negatively related to school-year work intensity, we can infer the direction and significance of the second-stage “work intensity effect” from the reduced-form estimates. For example, in the annual crime models, the instrumental variables are all positively and significantly related to crime. This means that the “work intensity effect” at the second stage must be negative and significant (since the product of two negative values produces a positive value). In the recent substance use models, the instrumental variables are negative but are not jointly (nor individually) significant. However, they are negative and jointly significant in the smoking and drinking frequency models, implying that the second-stage “work intensity effect” is positive and significant. Thus, for criminal behavior, smoking, and drinking, the results are insensitive to whether the model is specified in a linear or non-linear form.

Estimation in First Differences. As a final test of robustness, I estimate a model in first differences rather than fixed effects. The first-differences (FD) estimator is similar to the fixed-effects (FE) estimator, in that the individual effect is swept out of the model in both; this is just accomplished in a different way.¹⁴ The FE estimator does this by

¹⁴ A first-differences (FD) model is parameterized in the following way:

$$(y_{it} - y_{i,t-1}) = \alpha + \beta(x_{it} - x_{i,t-1}) + (\varepsilon_{it} - \varepsilon_{i,t-1})$$

$$\Delta y_{it} = \alpha + \beta \Delta x_{it} + \Delta \varepsilon_{it}$$

transforming all variables into deviations from within-panel means, while the FD estimator does it by computing the change from the previous time period. A comparison of results using these two estimators is often good practice, although it can be difficult to choose between the two if they give substantively different results (Wooldridge, 2002).

Under the FDIV specification, the results for annual crime are identical to the original (FEIV) specification, and the coefficients are well within sampling error. However, the FDIV results for the substance use response variables differ in important ways, so I will describe these in some detail. In Table 18, I provide the estimates from models of first differences and first differences with instrumental variables, as well as the random-effects coefficients for comparative purposes (from Table 16). In the top panel of Table 18 are the first-stage fit statistics for the FDIV model, showing that the models provide a satisfactory fit. An important finding from this table is that the estimates from a traditional first differences model are substantially smaller than those from their fixed-effects counterparts, and they are not within sampling error for smoking.¹⁵ A similar finding applies to FDIV and FEIV, but here the estimates are decidedly *not* within

where differences are taken from the previous time period rather than from panel means. Although first differences eliminate the unobserved individual effect, μ_i , doing so results in the loss of the first observation for all individuals. The working sample size is thus $N(T - 1)$ rather than NT as in the fixed-effects model. The first-differences instrumental variables estimator (FDIV) is a straightforward extension:

$$\Delta y_{it} = \alpha + \beta \Delta \hat{x}_{it} + \Delta \varepsilon_{it}$$

where the fitted “difference” is substituted into the second stage.

¹⁵ When the errors are serially independent, FE is efficient, whereas if the errors follow a random walk, FD is efficient (Wooldridge, 2002). Unfortunately, there is no formal statistical test for choosing between FD and FE (or between FDIV and FEIV). A Hausman specification test is inappropriate because both estimators are consistent under the alternative hypothesis. The metric I use to determine what is “substantively” different is whether or not 95 percent confidence intervals around the work intensity point estimates overlap. When comparing the FD estimates to the FE estimates for smoking, none of the confidence intervals overlap (and 99% confidence intervals barely overlap). When comparing the point estimates for drinking, a 95 confidence interval does not overlap in Model 1, but does overlap slightly in Models 2 and 3. Note that I use this only as a shorthand metric, and that it is inappropriate in a formal sense. Since the models are estimated using the same sample, the estimators have non-zero covariance. If this covariance is positive, the confidence intervals are wider than reported here and are thus more likely to overlap.

sampling error.¹⁶ For smoking frequency, the Hausman tests favor the FDIV models. Interestingly enough, all coefficients are negative, and the coefficients from Models 2 and 3 attain statistical significance. For drinking frequency, Hausman tests favor the FD models. Although the coefficient from Model 1 is marginally significant, the coefficients from Models 2 and 3 are non-significant. What is noteworthy is that, with but a single exception (Model 1 for drinking in FD), *the favored models imply that there is not a positive relationship between school-year work intensity and substance use*, and in two cases, longer work hours are actually associated with a significant *decrease* in substance use (Models 2 and 3 for smoking in FDIV).

*** Table 18 Here ***

Thus, empirical results from models estimated in first differences lead to different inferences about the relationship between school-year work intensity and substance use than from models estimated using individual fixed effects. There are at least four econometric explanations that can account for the more conservative (and largely null) estimates provided by FD/FDIV relative to FE/FEIV. I will consider each possibility in turn. One potential reason for the difference between FD/FDIV and FE/FEIV is that the FD/FDIV model loses important information by excluding the first observation for all individuals (in order to compute the difference between the first and second time periods). To address this, I re-estimated the FE/FEIV models excluding the first observation for each individual.¹⁷ In all cases, the coefficient for school-year work

¹⁶ When comparing the FDIV estimates to the FEIV estimates, *none* of the confidence intervals overlap. For example, using the estimates for smoking frequency in Model 3, the 95 percent confidence interval from the FDIV model is (-.1560, -.0209) and from the FEIV model is (.2478, .3483). Additionally, there is not even overlap between 99 percent confidence intervals for any of the models.

¹⁷ I did this in two different ways. I first re-estimated the existing FE and FEIV models by excluding the first observation for each individual. I then re-estimated the FE and FEIV models by re-

intensity was positive and statistically significant. Thus, the difference between FD/FDIV and FE/FEIV cannot be explained by the mechanics of the differencing procedure and the loss of information from the first observation.

A second possibility is that change in school-year work intensity is simply not exogenous in the substance use models, even with the exclusion of state child labor laws. Wooldridge (2002) warns that if FD/FDIV and FE/FEIV differ in ways that cannot be attributed to sampling error, the exogeneity assumption could be violated. In the event that this is true, FD/FDIV and FE/FEIV are both inconsistent and converge to different probability limits. Simply put, in this case the results provided by both estimators are wrong. This explanation would require that the state child labor laws be correlated with the structural disturbance. However, we have seen that the tests of overidentifying restrictions in FDIV and FEIV are not rejected in the smoking and drinking models. Thus, violation of the exogeneity assumption is unlikely.

A third possible explanation for the inferential divergence between FD/FDIV and FE/FEIV is the problem of serially correlated errors. The FE/FEIV estimator assumes that the model errors are serially uncorrelated, whereas the FD/FDIV estimator assumes that the errors follow a random walk. If in fact the model residuals are positively serially correlated, the FE/FEIV standard errors are biased downward as a function of the degree of dependence among the residuals (Greene, 2003; Wooldridge, 2002). The practical effect is to invalidate hypothesis testing by making the usual t -statistics too large, and thus increasing the likelihood of type I inferential errors. In the present context, this means that positive serial correlation can make it appear that there is a significant

computing individual change scores after omitting the first observation for each individual. Both sets of results were identical.

relationship between school-year work intensity and substance use, when in fact no causal relationship exists. The FD/FDIV model, on the other hand, assumes *a priori* the presence of first-order serial correlation, and the differencing procedure is intended to remove it. Testing for serial correlation is not routinely done in FE/FEIV, although Bhargava et al. (1982) propose a straightforward diagnostic. They suggest a Durbin-Watson statistic which they generalize to panel data as a way to test for serially correlated as well as random walk errors.¹⁸ Using this procedure, in all models I reject the null hypothesis of no serial correlation, but fail to reject the null hypothesis that the errors follow a random walk. Thus, there is indeed positive serial correlation in FE/FEIV biasing the standard errors downward, but not as much as implied by a random walk. When I implemented the GLS procedure outlined by Bhargava and colleagues, there was only minimal change in the substantive results for FE/FEIV, although the *t*-statistics did decrease slightly as expected in the presence of positive serial correlation.¹⁹ Thus, the difference between FD/FDIV and FE/FEIV cannot be attributed to serial correlation.

¹⁸ The *d*-statistic is estimated in the following manner:

$$d = \frac{\sum_{i=1}^N \sum_{t=2}^T (\tilde{\epsilon}_{it} - \tilde{\epsilon}_{it-1})^2}{\sum_{i=1}^N \sum_{t=1}^T \tilde{\epsilon}_{it}^2}$$

where *d* is sufficient to test for both serially correlated and random walk residuals. Bhargava et al. (1982) note that for large data sets (in *N*), it is not necessary to calculate upper and lower bounds for *d* when testing for serial correlation (traditional bounds do not apply in a panel context, since they rely on *N*, *T*, and *K*). Instead, since these bounds are tight in moderately large samples, all that is required is to determine whether *d* is less than two (in the case of positive serial correlation) or greater than two (in the case of negative serial correlation). The null hypothesis is that $\rho = 0$, implying no serial correlation. In the three FE smoking models, *d* is approximately 1.57, while in the three FE drinking models, *d* is approximately 1.73. Similar results were obtained when I estimated the FEIV models. Thus, in all models I reject the null hypothesis of no serial correlation. If this hypothesis test is rejected, Bhargava and colleagues then suggest testing whether the residuals form a random walk, with the null hypothesis now that $\rho = 1$. Since the upper and lower bounds that they provide for this hypothesis test are less than one, I also reject the null hypothesis that the errors form a random walk. An initial estimate of ρ can be recovered by using

$$\hat{\rho} = 1 - \frac{d}{2}$$

Using this formula, the estimate of first-order serial correlation is 0.215 in the smoking frequency models and 0.135 in the drinking frequency models.

¹⁹ Bhargava et al. (1982) suggest transforming all variables in the following manner:

A fourth possibility is the problem of measurement error. In the presence of classic measurement error (i.e., error that is random), conventional estimators suffer from an attenuation bias that drives estimates toward zero (Greene, 2003). Measurement error tends to be exacerbated in FD/FDIV, as it relies on period-to-period variation in the explanatory variables. Although FE/FEIV is also adversely affected by measurement error, transformation of the regressors into deviations from panel means makes the estimates less sensitive to the “noise” present in year-to-year change, and thus less inconsistent than FD/FDIV (McKinnish, 2000). As an informal diagnostic test, I re-estimated the FD/FDIV models taking two-year differences, then three-year differences, and so on (see McKinnish, 2000).²⁰ In the smoking frequency models, the magnitude of the coefficients systematically increases with the length of the difference, suggesting that measurement error is leading to some attenuation bias in FD/FDIV. Indeed, the FD estimates converge toward the FE estimates. However, the FDIV estimates are still substantially smaller than their FEIV counterparts. It should be noted as well that the marginally significant negative coefficient in Models 2 and 3 is not robust to the differencing length. In the drinking frequency models, there is less systematic variation in the magnitude of the “work intensity effect” as the length of the difference increases. Therefore, although measurement error does appear to produce some attenuation bias in FD/FDIV (especially for smoking), the substantive conclusions remain essentially unchanged. The FDIV estimates are still substantially smaller than the FEIV estimates.

$$\begin{aligned} & \sqrt{1-\hat{\rho}^2} y_{it}, \sqrt{1-\hat{\rho}^2} x_{it} \quad t=1 \\ & (y_{it} - \hat{\rho}y_{it-1}), (x_{it} - \hat{\rho}x_{it-1}) \quad t > 1 \end{aligned}$$

Panel means are then subtracted off these transformed variables, and estimation carried out as usual.

²⁰ In the presence of measurement error, taking longer differences increases the “signal to noise” variance ratio, which in turn produces larger coefficients. I am indebted to Seth Sanders and Jeff Smith for suggesting the possibility of measurement error, and for directing me to the paper by McKinnish (2000) that documents this problem and proposes the auxiliary FD/FDIV models that I use here to diagnose it.

The conclusion thus far remains that the preferred (on the basis of specification tests) FD/FDIV models imply no relationship between school-year work intensity and substance use, in contrast to the preferred FE/FEIV models, and this conclusion is robust to potential violations of key model assumptions. Aside from econometric explanations for the difference between FD/FDIV and FE/FEIV, there is a theoretical argument that can be made in favor of FD/FDIV over FE/FEIV. Recall that FE/FEIV transforms all variables into deviations from panel means in order to sweep out unobserved heterogeneity, whereas FD/FDIV does so by computing a change score from the previous time period. Although both accomplish a similar goal (that of eliminating the individual effect), there are two very important differences. First, there are differences in how the “work intensity effect” is identified and interpreted. FE/FEIV suggests that *during periods in which youths work longer-than-average hours during the school year, they engage in a higher-than-average rate of substance use*. In contrast, FD/FDIV suggests that *an increase in work hours from the previous time period has no effect on change in substance use since that time period*. In other words, FE/FEIV is identified on change from panel means, while FD/FDIV is identified on year-to-year change. Second, there are differences in the assumption about unobserved heterogeneity. FE/FEIV assumes that unobservables are fixed or stable over the entire panel, whereas FD/FDIV assumes that unobservables are specific to adjacent time periods. These are not trivial interpretive differences. The assumptions of FE/FEIV may be tenable during adulthood, when change is more orderly. However, during the period of adolescence, change can be much more episodic and erratic. On this basis, then, the estimates provided by FD/FDIV may be more appropriate.

In the final analysis, a cautious reader may prefer the conclusion that the “work intensity effect” on substance use is sensitive to how the model is specified. Although it is fairly clear from this sensitivity analysis that longer work hours do not consistently reduce substance use, it is also not the case that longer work hours invariably increase substance use. On the other hand, the conclusion about the “work intensity effect” on criminal behavior is unequivocal. Longer work hours during the school year consistently and substantially decrease criminal involvement.

CHAPTER FIVE: DISCUSSION AND CONCLUSION

In this study, I have endeavored to provide a reasonable estimate of the causal effect of school-year work intensity on crime and substance use. To date, dozens of studies have found that youths who work the longest hours while in high school are at the greatest risk for problem behavior. The empirical literature is so unambiguous that one is easily tempted to proclaim the deleterious “work intensity effect” on problem behavior an empirical fact. However, a cautious reading of the literature urges some restraint on the interpretation of this consistently positive association, as it is plagued by what Manski (1995) refers to as the “identification problem.” Because all prior research is observational in nature, and all prior research until recently has been restricted to controlling for observed sources of heterogeneity, the *causal significance* of the adverse “work intensity effect” is in some dispute. Consequently, researchers are not in a position to definitively rule out the possibility that the positive relationship between work intensity and problem behavior is a spurious one, or that perhaps the true causal relationship is actually negative. Indeed, Brame et al. (forthcoming) demonstrate that the *sign* of the “work effect” on delinquency is unidentified, and differs as a function of the assumptions that researchers are willing to make about the impact of unobservables on the probability of problem behavior and the probability of being employed. Their sensitivity analysis showed that if an unobserved “crime trait” is associated with a higher probability of being employed during adolescence, and also a higher probability of engaging in crime, the “work effect” could in fact be negative.

This study is a direct outgrowth of two recent empirical analyses that sought to directly confront this identification problem. Tyler (2003) used state child labor laws as

instrumental variables to identify the impact of school-year work intensity on achievement test scores. Paternoster et al. (2003) used a fixed-effects estimator to identify the impact of within-individual change in intensive work during the school year on within-individual change in delinquency, substance use, and problem behavior. In the present study, I merged the empirical methods from these two prior studies. First, I employed state child labor laws governing the maximum number of hours of work per week allowed for enrolled minors during the school year as instrumental variables for school-year work intensity. These laws vary by state and by age eligibility. For example, all states impose the most restrictive laws on the employment of 14 and 15 year olds, but relax these laws by the time youths are ages 16 and 17. Different states impose different restrictions at these ages, however, and by age 18 child labor laws expire in all states. I thus take advantage of the fact that states vary at the cross-sectional level in the restrictiveness of child labor laws during the 14-15 and 16-17 age periods. Second, I followed a panel of youths through these age ranges, and thereby used the within-individual increase in school-year work intensity attributed to the easing of child labor laws to identify the causal impact of work intensity on crime and substance use.

In the remainder of this chapter, I summarize the results from a series of empirical models designed to assess the strength of the evidence for causality between adolescent work intensity and problem behavior. This is followed by a discussion of the implications of the findings for criminological theory as well as public policy. I close the chapter with a discussion of avenues for continued research.

YOUTH EMPLOYMENT AND PROBLEM BEHAVIOR

In direct response to the identification problem, I employed three different estimation strategies along with specification tests to evaluate the strength of the claim that longer work hours during the school year are causally related to elevated involvement in problem behavior. First, I used a random-effects estimator, which is the panel equivalent to a cross-sectional design that assumes that selection bias in the “work intensity effect” can be eliminated by controlling for a variety of observed covariates. Second, I used a fixed-effects estimator, which assumes that controlling for observed covariates does not eliminate all possible sources of bias, but that including individual-specific intercepts to adjust for unobserved time-invariant factors does so. Third, I used a fixed-effects instrumental variables estimator, which assumes that the “work intensity effect” remains contaminated by unobserved dynamic factors, but that exclusion restrictions involving state child labor laws are sufficient to render change in work intensity exogenous.

School-Year Work Intensity and Crime

Empirical models based on a strict “selection on observables” strategy show that longer work hours are associated with *more* criminal behavior. However, this positive correlation is due entirely to unobserved heterogeneity, since an adjustment for individual fixed effects shows that longer work hours are associated with a *slight decrease* in criminal behavior. Even this modest negative correlation may be biased and inconsistent due to the omission of relevant explanatory variables that change over time along with work intensity and crime. When the portion of change in work hours that is exogenous to

crime is isolated, the results show that longer work hours are associated with a *substantial reduction* in criminal behavior. Specification testing suggests that these last empirical models—the fixed-effects instrumental variables models—are the most appropriate, and that the results provided by random- and fixed-effects models are inconsistent.

Based on the empirical results in the previous chapter, I can conclude with a good deal of confidence that, among youths in the NLSY, *the increase in work hours attributable to change in child labor law eligibility significantly reduces criminal behavior*. It appears to be the case that the preventive “work intensity effect” is stronger for comparatively less serious forms of property crime, primarily vandalism, theft, shoplifting, and offenses dealing with stolen goods. It is perhaps no coincidence that it is precisely these types of criminal behavior that peak during adolescence. More serious forms of crime such as burglary, robbery, assault, and handgun possession are less consistently associated with change in work intensity. Of the 116 crime-specific models estimated, in only one did work intensity significantly increase crime (Model 1 for drug selling frequency), and in all others it was associated with either no change or a decrease in criminal conduct. Not only is the work intensity effect insensitive to how crime is measured, it is insensitive to how work hours are operationalized. Irrespective of whether work intensity is measured dichotomously, ordinally, or continuously, its relationship to crime appears to be invariant. Thus, even changing to the most intensive work hours reduces crime.

The relationship between youth employment and crime in this study provides an intriguing counterpoint to previous studies of the topic. Virtually all of these studies

support the conclusion that higher work intensity has a criminogenic influence by increasing youth involvement in minor delinquency (e.g., theft, vandalism, phony ID) and serious delinquency (e.g., Index crime, interpersonal aggression, assault), and by increasing rates of police contact and arrest (see Abe, 1999, 2001; Agnew, 1986; Bachman et al., 1986, 2003; Bachman and Schulenberg, 1992, 1993; Bartko and Eccles, 2003; Crowley, 1984; Cullen et al., 1997; Heimer, 1995; Huang et al., 2001; Mihalic and Elliott, 1997; Ploeger, 1997; Resnick et al., 1997; Ruggiero, 1984; Shannon, 1982, 1988; Steinberg and Avenevoli, 1998; Steinberg and Dornbusch, 1991; Steinberg et al., 1993; Tanner and Krahn, 1991; Wright et al., 1997, 2002; for null effects or even negative effects of work intensity on crime and arrest, see Apel et al., 2003; Brame et al., forthcoming; Good et al., 1986; Gottfredson, 1985; Paternoster et al., 2003; Staff and Uggen, 2003; Uggen, 2000a). Indeed, Wright et al. (2002:12) conclude that “the more adolescents become embedded in work, the more deeply they become embedded in delinquency.” The present study suggests, however, that these prior “work intensity effects” are predominately driven by self-selection. That is, youths with a higher propensity to engage in criminal behavior are more likely to work and to work the longest hours, and this propensity for delinquency, crime, and arrest may actually *precede* work involvement (see Gottfredson, 1985; Mihalic and Elliott, 1997; Staff and Uggen, 2003; Steinberg and Avenevoli, 1998; Steinberg et al., 1993). However, once these crime-prone youths enter the formal work force, their longer work hours actually inhibit their criminal involvement. Moreover, the instrumental variables estimates provided in this study suggest that this reduction in crime is *causally* related to their heavier work involvement.

In a later section, I give some thought to potential theoretical mechanisms than can account for this *beneficial* “work intensity effect” on crime.

School-Year Work Intensity and Substance Use

Empirical models that control strictly for observables show that higher work intensity is associated with more extensive substance use. When individual fixed effects are introduced to control for time-invariant unobservables, the deleterious “work intensity effect” does not disappear, and in fact changes very little. When change in state child labor law eligibility is further introduced, the impact of work intensity on substance use increases considerably. However, because of the poor performance of the instrumental variables in these models and their correlation with the disturbance in the structural model, these empirical results are invalid. Additional analyses demonstrated that the inclusion of marijuana use and binge drinking was responsible for this. On the other hand, the use of specific substance use behaviors like smoking and drinking frequency mitigates this problem with the instrumental variables. My initial conclusion, then, is that the increase in work hours attributable to change in child labor law eligibility significantly increases the frequency of monthly cigarette and alcohol use. This conclusion harmonizes with virtually all other research finding that higher work intensity exacerbates substance use (see Bachman et al., 1981, 1986, 2003; Bachman and Schulenberg, 1992, 1993; Greenberger et al., 1981; Hansen and Jarvis, 2000; Huang et al., 2001; Johnson, forthcoming; McMorris and Uggen, 2000; Mihalic and Elliott, 1997; Mortimer, 2003; Mortimer et al., 1996; Mortimer and Johnson, 1998; Resnick et al., 1997; Ruggiero, 1984; Safron et al., 2001; Staff and Uggen, 2003; Steinberg and

Dornbusch, 1991; Steinberg et al., 1982, 1993; Tanner and Krahn, 1991; for null effects of work intensity on substance use, see Apel et al., 2003; Gottfredson, 1985; Paternoster et al., 2003; Ploeger, 1997). Indeed, Mortimer and colleagues (1996) find that the greatest concern about the consequences of intensive work involvement during high school is with alcohol use.¹

Unfortunately, causal inference is extremely sensitive to how this model is specified. When an alternative identification strategy is employed using first differences in place of fixed effects, the “work intensity effect” is statistically (and substantively) indistinguishable from zero. This unexpected difference cannot be fully explained by the mechanics of the differencing procedure, violation of the exogeneity assumption, serial correlation, or measurement error. Indeed, the difference between results for the two estimators is surprisingly robust; thus, neither set of results is favored on technical (econometric) grounds. However, there may be theoretical reasons to favor the null work effect on substance use provided by the first-differences models. This procedure may lend itself well to modeling behavior during the period of adolescence, when physiological, emotional, affiliative, and behavioral changes are taking place rapidly, reminiscent of G. Stanley Hall’s (1904:xiii) now famous dictum: “Development is less gradual and more saltatory, suggestive of some ancient period of storm and stress when old moorings were broken and a higher level attained.” This may be particularly true concerning adolescent behavior like substance use, which is so much more fluid and experimental during this life stage. In the context of this instability, it is desirable to temporally link change in work intensity with change in substance use, and to relax the

¹ It bears mentioning, however, that McMorris and Uggen (2000; also Mortimer and Johnson, 1998) find that the impact of high work intensity during high school on alcohol use does not persist into early adulthood.

assumption that unobservables are time-invariant over the 12-22 age span considered here, both of which first differences accomplish.² Ultimately, the question of what set of empirical results to believe must await further research using the NLSY and similar data sets. As it stands, then, the most balanced conclusion at the present time may simply be that *the empirical results are equivocal with respect to the causal impact of school-year work intensity on substance use.*

Potential Threats to Causal Inference

One potential threat to the finding that higher work intensity reduces criminal behavior is that the result is specific to the NLSY cohort. In other words, it reflects a period effect rather than a genuine causal effect of employment on crime. The NLSY youths were born in the first half of the 1980s (1980-1984) and were completing high school at the turn of the century. At the time that they were reaching the age of initiation into the formal labor market, the United States was in the midst of a period of unprecedented economic growth and prosperity. Indeed, among the fastest growing occupations in the lowest earnings group (which we may safely assume includes adolescents) were sales and service workers in the retail trade and service industries (Ilg and Haugen, 2000). The booming economy of the mid- to late-1990s thus offered more expansive work opportunities to young people. In comparison, most prior surveys of adolescents were conducted on samples of youths completing high school in the late

² If this line of reasoning is accurate, it has important implications for future longitudinal studies of adolescent behavior and the transition to adulthood. Fixed effects may be sufficient during periods of relative orderliness of change in life circumstances such as during adulthood. However, the assumptions of this model underlying the temporal linkage of causal and response variables, as well as the temporal stability of unobserved heterogeneity, may be a bit too restrictive when the analytical focus is on the adolescent life stage. These issues are worthy of additional research, and it seems that untangling them should be a priority as panel estimators become more common in criminological research.

1970s and early 1980s, when youth employment reached record levels (e.g., Bachman et al., 1981, 1986; Greenberger et al., 1981; Marsh, 1991; Mihalic and Elliott, 1997), or else on samples of youths finishing high school during the late 1980s and early 1990s, prior to the time when the U.S. began its period of growth (e.g., Bachman and Schulenberg, 1993; Mortimer et al., 1996; Steinberg and Dornbusch, 1991). Consequently, the beneficial “work intensity effect” on crime in the current study could be confounded with a larger secular trend of expanding economic opportunities for young people.³

The strength of this “period effect” critique of the present results is diminished, however, by two factors. First, other studies of more recent youth cohorts replicate the adverse “work intensity effect” on problem behavior. For example, recent studies using the Monitoring the Future survey rely on 8th, 10th, and 12th graders in the classes of 1991-1998 (Bachman et al., 2003; Safron et al., 2001); the Maryland Adolescent Development in Context study uses high-school seniors in 1997 (Bartko and Eccles, 2003); and a study using the National Longitudinal Study of Adolescent Health relies on youths attending grades 10-12 during 1996 (Johnson, forthcoming). Each of these studies finds that more time spent in the workplace corresponds with more extensive involvement in problem behavior. Second, in the current study, I show that the adverse “work intensity effect” is also replicated in the NLSY (as do Abe, 1999, 2001; Apel et al., 2003; Brame et al., forthcoming; Huang et al., 2001; Paternoster et al., 2003). When I employ a panel estimator with random effects—a longitudinal counterpart to the cross-sectional designs used in existing youth employment literature—longer work hours are positively and significantly associated with crime and substance use (refer back to Tables

³ On the other hand, it is just as easily argued that the findings from prior studies of an adverse “work intensity effect” could be confounded with a larger secular trend of high youth unemployment.

9 and 12). Therefore, I have greater confidence that the results reported herein are not a historical artifact, and can be attributed to the causal impact of work intensity on crime.

A second potential threat to causal inference is that the impact of work intensity on crime (and the absence of a consistent impact of work intensity on substance use) is actually an estimate of the “local average treatment effect” (LATE) or, when the endogenous regressor is continuous as in the present case, the weighed “average causal response” (see Angrist and Imbens, 1995; Imbens and Angrist, 1994). The most appropriate interpretation is that longer work hours result in lower crime *among youths who increase their work hours in response to the easing of state child labor restrictions on weekly school-year work intensity*. In other words, the “work intensity effect” is estimated only for those respondents whose behavior can be manipulated by the instruments used in this analysis (Imbens and Angrist, 1994). A related concern is whether we would obtain a different estimate of the “work intensity effect” using different instrumental variables; in other words, whether the estimates are “instrument dependent.” An additional qualification is that the identifying power of the instrumental variables is based on the age-16 transition and, to a lesser extent, the age-18 transition, corresponding with the ages in which state child labor laws are relaxed to allow longer hours during school weeks in formal jobs. The question thus arises about who the “compliers” are and whether they are representative of the population of all youths (Angrist et al., 1996).⁴ If these youths comprise only a small proportion of the adolescent population, the “work intensity effect” estimated in this study could have little theoretical or practical significance.

⁴ As Angrist (2004:C52) maintains, “even internally valid estimates are less interesting if they are completely local, i.e., have no predictive value for populations other than the directly affected group.”

However, the very nature of youth employment and child labor law minimizes concern over the estimated “work intensity effect” being too local. For example, Figure 5 shows that youth employment increases slowly and steadily during the 14-15 age period, but that there is a rapid increase at age 16 and a more steady increase thereafter. The most plausible explanation for this discontinuity is that child labor laws are eased considerably at age 16, and that employers are more inclined to hire young people as a consequence. The fact that the federal child labor law governing school-year work involvement is likely to be operative for most 14 and 15 year olds (and expires thereafter) implies that the age-16 transition corresponds with a loosening of work restrictions (and thus expanding work opportunities) that applies to *all youths* and not to some circumscribed subpopulation. Therefore, the group of “compliers” from whom the estimated work intensity effect is derived constitutes a large proportion of the adolescent population.

YOUTH EMPLOYMENT AND CRIMINOLOGICAL THEORY

To this point, there has been virtual agreement that an extensive work commitment is responsible for higher rates of crime and substance use, among other forms of problem behavior (e.g., school misconduct). Numerous criminological theories can be marshaled to explain the criminogenic “work intensity effect.” Among the most popular causal mechanisms invoked are that intensive youth employment (1) disrupts effective family functioning (social control theory), (2) erodes school performance and educational commitment (social control theory), (3) introduces additional stress during an already stressful life stage (general strain theory), (4) contributes to antisocial friendship formation (learning theory), (5) negatively alters the way that youths spend their leisure

time (routine activity theory), and (6) offers a new domain for deviant conduct as well as funding for a deviant lifestyle (opportunity theory). Other theories maintain that the positive relationship is spurious; in other words, the true relationship is actually a null one. These theories claim that an inability to delay gratification underlies the correlation between work intensity and problem behavior, but differ according to whether this underlying process is limited to the adolescent life stage (precocious development theory) or is stable across the life span (propensity theory).

The present findings suggest that neither of these sets of theories is appropriate to explain the relationship between school-year work intensity and criminal behavior. *The relationship between work intensity and crime is neither positive nor spurious. It is, in fact, negative.* This interesting empirical result demands that criminologists refocus the theoretical lens on the expected relationship between employment during adolescence and involvement in crime from a position emphasizing the potential *harmful* effects of work involvement to one emphasizing the potential *benefits*. There are two theoretical issues that must be attended to, at least where adolescent crime and delinquency is concerned: (1) understanding the differential selection process, and (2) explaining the crime-preventive effect of longer work hours. These two tasks go hand in hand, as an understanding of the causal impact of work intensity on crime necessitates an understanding of the selection problem. As we will see, selection is an essential part of the causal story.

Selection into the Youth Labor Market

We have seen that there is non-random selection into the formal youth labor market, such that youths at highest risk for crime and substance use are most likely to make this transition during the middle teenage years, and to also work the longest hours while on the job. This is most obviously demonstrated by the fact that work intensity is positively related to crime in the random-effects model, but negatively related to crime in the fixed-effects and fixed-effects instrumental variables models. The implication is that the positive relationship observed in prior studies is entirely a consequence of failure to control for time-invariant differences between youths. Fixed-effects models, however, are fundamentally agnostic about what exactly these unobservables entail, as they treat the unobserved individual effect as a nuisance parameter that is simply swept out of the model.

Previous research provides some guidance about potential sources of heterogeneity, although the list is quite extensive. This may in part explain why prior studies have not completely eliminated the adverse “work intensity effect”—it is simply not possible to control for all potential sources of selection bias or heterogeneity. To provide some context for the following discussion, Appendix E provides a more rich description of working and non-working youths, as well as youths working at varying levels of intensity, during the 14-16 age span.⁵ A casual comparison of workers and non-workers shows several things. First, workers are at a comparative advantage relative to

⁵ I selected out 12-13 year olds from the first interview and constructed their work history from age 14 to age 16. I used this subsample because at the first interview, all predictors of employment are prospective, since work histories are not collected until age 14. Moreover, for these younger cohorts, a wide array of risk and protective factors of interest to criminologists is available. I first created a work status indicator coded “1” if a youth worked at all during that period. I then created an ordinal measure of mean work intensity during this three-year period (1 = 1-10 hours; 2 = 11-20 hours; 3 = 21-30 hours; 4 = 31+ hours). All variables provided in Appendix E1 are measured at the first interview. Variable definitions are provided in Appendix E2.

non-workers with respect to their location in the social structure, consistent with much prior research (e.g., National Research Council, 1998; Schoenhals et al., 1998; U.S. Department of Labor, 2000). For example, workers are far more likely to be white, to have employed parents (especially an employed mother), to have well educated parents (some college experience), to live in a permanent dwelling (house, condo, or farm vs. apartment), and to have higher family income and more assets. Second, workers are better students, as indicated by their higher achievement scores and lower likelihood of being behind in school. Third, workers are subject to slightly less parental social control early on, as they confide less in their parents and are subject to more permissiveness and less inductiveness (although attachment and monitoring are unaffected). Fourth, workers are also more likely to have deviant peers, to be autonomous (going out on dates, working in an informal job), and to experiment with minor delinquency and substance use, but nothing serious enough to lead to arrest. Thus, the image of the would-be worker (compared to the non-worker) is that of a good student from a middle-class background, subject to less parental control (but far short of detachment) and thus more easily tempted in the company of deviant friends to engage in minor delinquency and experimental drug use. This image harmonizes with a number of delinquency theories, the most prominent of which might include social control, learning, and routine activity theories. It is also consistent with Matza's (1964) notion of the adolescent in a state of "drift," Hagan's (1991) image of the middle-class "party subculture," and Moffit's (1993; Moffit et al., 2001) theory of "adolescence-limited" antisocial behavior.⁶ From each of these

⁶ Moffit (1993:686) notes that a hallmark of adolescence-limited antisocial behavior is the lack of cross-situational consistency: "For example, they may shoplift in stores and use drugs with friends but continue to obey the rules at school." This seems to be the pattern observed here. Prior to working,

viewpoints, the delinquent is neither fully committed nor uncommitted to delinquent action, but instead behaves in response to an occasional loosening of institutional control and in the pursuit of adventure and excitement (see also Campbell, 1969). Accordingly, employment is but one aspect of the greater freedom accorded to would-be workers by their parents, and a modestly higher level of minor problem behavior is an inconsequential byproduct of this freedom.

A comparison of adolescent workers by their mean work intensity over the 14-16 age span, however, reveals that they are a quite heterogeneous lot. In contrast to the findings from the comparison between workers and non-workers, youths who work over 20 hours per week are uniformly disadvantaged with respect to predictors of problem behavior. For example, they are more likely to be minority, to live in impermanent housing, to have unemployed fathers and parents with lower education, and to live in households with lower income, higher rates of program participation, and fewer assets. They are also far less likely to live in intact households, to perform well in school, and to be subject to parental social control. Finally, they have less-than-conventional friends and more extensive involvement in delinquency and substance use, leading to a far higher probability of arrest. On the other hand, youths who restrict their work involvement to ten hours or fewer, on average, are with few exceptions the most advantaged compared to other workers and even to non-workers.⁷ Thus, the image of the would-be intensive

employed adolescents are performing at a high level academically, but on occasion “flirt” with deviance when away from school.

⁷ Compared to workers and non-workers alike, the most moderate workers (1-10 hours) rate the highest on protective factors and the lowest on risk factors for delinquency and substance use. Not surprisingly, then, they also have the lowest rates of involvement in delinquency and substance use. This is consistent with Bachman and Schulenberg’s (1993) contention that no work experience at all is not necessarily better than a light work commitment. The only notable exception is that these moderate workers are granted more autonomy by their parents compared to non-workers (decision-making autonomy, dating, informal work), but within the context of more responsive and authoritative parenting (attachment,

worker (compared to other workers and non-workers) is that of a disengaged student from a lower-class background (but with indicators of family functioning not consistently related to this work pattern). Their antisocial behavior is qualitatively different, evidencing greater seriousness which leads to a higher probability of police contact. This image is consistent with Bachman and Schulenberg's (1993) characterization of intensive adolescent employment as one symptom of a "syndrome" of problem behavior. This syndrome is apparent prior to the work transition, early indicators being academic failure and more serious problem behavior, with employment occurring relatively late in the syndrome sequence (Bachman and Schulenberg, 1993). An early *desire* for high levels of work involvement also presages actual attainment of that goal, and this preference for long work hours is itself related to early school failure and problem behavior (Bachman et al., 2003).

In sum, at 12 and 13 years of age—prior to entry into the formal economy—it is possible to distinguish would-be workers from non-workers, and would-be intensive workers from moderate workers, on the basis of many of the known risk factors for crime and substance use. Youths who will go on to exhibit a pattern of intensive work during the middle teenage years (21-30 or 31+ hours per week) are distinguished by their measurably higher risk for problem behavior during early adolescence. For example, they are less structurally advantaged, more disengaged from school, and more delinquent prior to their transition to formal work. It is this early predisposition for problem behavior that

monitoring, inductiveness). A light work commitment may thus be characterized by the healthiest balance—a "developmental match"—between freedom and control vis-à-vis their parents (see Baumrind, 1978; Eccles et al., 1991). Indeed, their work pattern is a reflection of this balance—being employed is one aspect of their exercise of freedom and autonomy, but the limits placed on their involvement demonstrates the continued control exerted by their parents.

has probably been confounded with estimates of the “work intensity effect” in prior research.

The Causal Impact of Investment in Work

With the foregoing findings in mind, we are in a better position to explain how an increase in work intensity can causally reduce criminal involvement. The fact of the matter is that intensive workers are at high risk on manifold predictors of adolescent problem behavior well before they begin working. Foremost among these is early educational failure, signifying a premature detachment or disengagement from school.⁸ With this in mind, there are at least two theoretical mechanisms that can account for the “work intensity effect” on criminal behavior.

One possible explanation is that work experience is viewed as a substitute arena for the development of skills and networks, or human and social capital, respectively. In short, investment in work may compensate for disinvestment in education, and early success in the workplace may offset perceived failure in the classroom. This may be especially true in urban neighborhoods, where youths are exposed to extremely low-quality schools and pervasive economic pressures, face a far higher risk of dropping out, and are presented with widespread illegal opportunities (see Leventhal et al., 2001). In

⁸ The origin of this detachment is subject to debate. For the present discussion, I simply take it as a given. It could be that detachment from school is a manifestation of low ability, which leads to dislike of school and teachers or a perceived irrelevance of education for one’s future (Hirschi, 1969; Hirschi and Hindelang, 1977; Ward and Tittle, 1994). It could also be that low ability produces a negative emotional state that leads to a desire to escape from the constraints imposed by the educational system (Agnew, 1992; Brezina, 1996). On the other hand, it is possible that poor performance creates negative parental and teacher appraisals of ability, which are adopted by the adolescent as self-appraisals of ability, and which then lead to diminished school commitment (Heimer and Matsueda, 1994; Matsueda, 1992; Triplett, 1993). Or, low self-control or impulsivity could underlie school detachment, as youths who discount the long-term rewards of schooling are the least likely to satisfy the academic requirements, and thus the most likely to disengage from this imposed system of restraints (Gottfredson and Hirschi, 1990; Wilson and Herrnstein, 1985).

the short run, more desirable work opportunities may flow from this early work experience (see Entwisle et al., 2000). This upward mobility may result from a combination of work-related skills and personal connections acquired on the job. This does not necessarily mean that educationally disinvested youths completely eschew the classroom for the workplace. On the contrary, recent work by Entwisle and colleagues (2004) suggests that many of these youths (about 40%) will eventually complete their education upon realizing the value that this credential possesses for advancement in their economic roles (by temporarily “stopping out” instead of permanently “dropping out”).

A second, related explanation is that the workplace constitutes a new domain of institutional control over behavior. By withdrawing from educational pursuits, youths forgo an important dimension of structure and discipline in their daily lives, and thus neutralize the social control function that this institution serves (see Hirschi, 1969). More intensive involvement in work may fill this void by imposing a greater degree of order and conformity to conduct standards in the workplace, with more concrete and severe consequences for violations than might be encountered in the educational system (e.g., cut in wages, assignment to less desirable shifts, termination). These standards could, perhaps unknowingly on the part of the employee, generalize to other non-work contexts as youths become more embedded in employment (see Newman, 1999). Moreover, workplace adults may fulfill the supervisory and socializing role formerly occupied by teachers.

Both of these theoretical explanations view adolescents as agentic; in other words, as being actively engaged in the construction of their own life course (see Elder, 1998), rather than being buffeted about by forces over which they exercise little control or

making decisions that have permanent consequences. The theoretical emphasis should be on how young people respond to and manage the opportunities and constraints they face within the institutions of family, school, and work. The conceptual tools of a life-course perspective—context, timing, interdependency, and agency—offer a promising starting point.

In sum, theoretical explanations of the relationship between youth employment and crime must account for two things. First, youths select themselves into the labor market, and youths who select themselves into the highest-intensity work are at far higher risk for problem behavior. Second, change in the number of hours of employment is actually associated with a decline in crime. The latter assertion will require new theorizing about youth employment and its consequences, some initial attempts at which I have provided here by drawing largely on themes that currently exist in the literature. Whatever theoretical mechanism is provided to account for the “work intensity effect” on problem behavior, it is clear that with respect to crime, there is continuity between youth employment and early adult employment. In other words, the direction of the “work intensity effect” does not change as the transition to adulthood approaches (see Wright et al., 2002).

YOUTH EMPLOYMENT AND PUBLIC POLICY

Presently the federal government, under the auspices of the Fair Labor Standards Act of 1938, imposes a maximum 18-hour work week on all 14 and 15 year olds under its jurisdiction, and no weekly hours restriction on 16 and 17 year olds. The National Research Council (1998), however, has recommended that federal child labor laws

governing school-year work hours be extended to 16 and 17 year olds, and specifically that a 20-hour work restriction be imposed, consistent with the “intensive work” cut-off used in most prior research. The justification for these restrictions lies with the fact that long hours are empirically linked with a host of adverse developmental outcomes, not the least of which is crime and substance use. However, the current study suggests that the emphasis on work intensity as a causal agent of increased problem behavior is overdrawn. In fact, with respect to adolescent crime, it is entirely misplaced, as a within-individual increase in work intensity actually reduces criminal behavior.⁹

Where the current study also diverges from previous empirical work is in the use of existing public policies to investigate the causal link between youth employment and problem behavior; the very policies that are currently the subject of scholarly debate. That is, I exploit variation across age and across jurisdictions in applicable child labor laws in order to generate within-individual variation in school-year work intensity. Although this study is fundamentally agnostic about the optimal restrictions on youth work intensity, it is the first to incorporate the existing restrictions into a model of youth problem behavior.¹⁰

⁹ Although the current study relied on state-level child labor laws to identify the “work intensity effect” on problem behavior, the fact that the instruments for 14 and 15 year olds had identical effects on school-year work intensity at the first stage strongly suggests that identification actually derives from the federal child labor law. This may be because for most employment at these ages, the federal law is binding, as the employers who are inclined to hire these youths are likely to be under the jurisdiction of the Fair Labor Standards Act.

¹⁰ One important issue that remains unresolved is the degree of compliance with federal and state child labor laws. The stronger relationship between child labor laws and work intensity among 14 and 15 year olds may be due in part to stricter enforcement of employment policies for these younger adolescents in order to protect them from harm. Or, business owners may be particularly sensitive to the bad publicity that can result from violations or workplace accidents that occur within the context of violations. In future work, it may be possible to obtain state and federal enforcement data related to the number of compliance officers employed by labor departments, the number of inspections that are carried out, and the monetary penalties assessed for violations.

The finding that youths who work the most intensively during mid-adolescence are much more disengaged from school has important practical implications. Contrary to recent accounts, intensive employment does not detach youths from school (see Steinberg, 1996). These youths are already detached one or two years prior to working and their work history is simply a reflection of this, not its cause (Bachman and Schulenberg, 1993). The relationship may, in fact, be working in the opposite direction. School disengagement may be a cause of higher investment in work experience, with work intensity an indication of the level of investment. In the short run, this “intensive employment as investment” may pay off by making additional work opportunities available through the human and social capital that such experience provides (Entwisle et al., 2000). However, in the long run, it may prove to be detrimental if youths completely detach from school by dropping out, as returns to high-school completion may be higher than the returns to high-school work experience (Hotz et al., 2002).

The policy solution lies not with imposing stricter child labor laws, as more restrictive statutes may do more harm than good. Child labor laws apply to *youths who are enrolled in school*. The educationally detached 16-17 year old who finds herself unable to gain the work experience that she seeks because of restrictive child labor laws may find that dropping out is the only alternative. In many states, doing so frees her from the jurisdiction of these laws, at least those governing school-year work hours.¹¹ The National Research Council’s (1998) recommendation may ultimately be counterproductive by making a bad situation worse.

¹¹ Although it is just a hunch at the moment, in future work we may be able to exploit variation across states in whether or not child labor laws for work hours apply to all minors, or only to enrolled minors. This could serve as a useful identifying instrument for high-school dropout when considered alongside age eligibility for dropout.

Public policy discussion may be more productive if it is sensitive to the issue of differential selection. This appears to be an emerging theme in research on the costs and benefits of youth employment, as Entwisle et al. (2000) have taken note of the increasing attention to selection bias and the increasing sophistication of analytic designs in the “fourth stage” of youth employment research.¹² Additional research along these lines would be fruitful indeed. The list of correlates of youth employment, and particularly intensive youth employment, is lengthy. Unfortunately, because of methodological weaknesses, the list of causes and consequences is extremely short. The current study considered one of the more robust correlates of youth employment—crime and substance use. The empirical findings demonstrate that researchers should reconsider the “facts” about adolescent work in light of the recent advances that have been made in analytical techniques such as instrumental variables estimation. Such research may challenge conventional wisdom but at the same time lead to fascinating new insights about the nature of youth employment as well as its short- and long-term consequences.

FUTURE RESEARCH

There are several additional lines of inquiry suggested by the current study. First is coming to terms with the issue of “treatment effect heterogeneity.” The estimated causal effect is, in reality, “an average of unit level causal effects of the treatment of interest” (Angrist et al., 1996:450). To the extent that there is heterogeneity in the work

¹² Entwisle et al. (2000) classify the “first stage” by the lack of systematic attention given to the nature of youth employment and thus the relative ignorance about its consequences. The “second stage” corresponded with explicit data-collection strategies to fill in this knowledge gap, and was dominated by economists interested in the labor-market consequences of youth employment. The “third stage” was dominated by developmental psychologists who broadened the scope of inquiry to include a wide variety of explicitly developmental outcomes related to family functioning, peer associations, and scholastic performance.

effect across the adolescent population, the estimated causal effect of work intensity on crime may not be representative of any one particular subpopulation. Future work should thus investigate whether and to what extent the “work intensity effect” varies as a function of such characteristics as gender and race, family economic background, and school disengagement.

Second, future work should more explicitly consider the community context of youth employment. Much adolescent work is in “naturally occurring” jobs (Greenberger and Steinberg, 1986), and these opportunities are by definition structured ecologically. Although not a central focus of the current study, such factors as unemployment, per capita personal income, and availability of jobs in certain industries (namely manufacturing, service, and government) significantly influenced the number of hours that youths committed to working. Moreover, several of these community factors directly influenced participation in problem behavior. Future research would thus benefit from efforts to better understand these processes.

Third, it is imperative that future work consider what causal effect work intensity has on other outcomes, particularly educational outcomes such as scholastic achievement and dropout. We have seen that, prior to labor market entry, youths who work at the highest intensity are most at risk of school failure. Thus, selection bias confounds estimates of the “work intensity effect” on educational performance. Considering that the loudest calls for youth employment reform are from advocates of the thesis that intensive employment leads to school disengagement and withdrawal (e.g., Steinberg, 1996), this seems a fruitful direction for additional research (à la Tyler, 2003).

Fourth, future work should consider identification strategies for other work characteristics, as youth employment does not vary solely along the dimension of work intensity. Although some research has recently considered other work dimensions, it has been restricted to traditional cross-sectional methods and is thus subject to the same selection bias critique leveled against studies of work intensity (e.g., Mortimer, 2003; Staff and Uggen, 2003). There are several possibilities for this line of research. First, state child labor laws governing work hours may directly affect other work dimensions such as wages, earnings, and type of occupation. Access to higher-paying and higher-quality work opportunities may be determined in part by the number of hours that youths may work. Employers may be reluctant to hire youths for these positions if they are not allowed to work more than 18 hours per week during the school year, for example. Thus, the age-16 and age-18 transitions may correspond not only with change in work hours, but also change in opportunities for more desirable and upwardly mobile positions. Second, it may be possible to exploit variation across states and across age in what are deemed to be “hazardous” occupations off limits to minors. For example, the Fair Labor Standards Act bans employment of 14 and 15 year olds in most work other than food service and retail occupations. At ages 16 and 17, youths may not work in certain manufacturing and mining jobs or with certain types of dangerous machinery. It may thus be possible to construct an instrument set consisting of age eligibility for certain skilled occupations. Third, variation in state minimum wage may be used as an instrumental variable for wages and earnings. In sum, additional exclusion restrictions using existing state labor policies can be used to estimate a system of multiple equations to determine

how other youth employment dimensions are causally related to involvement in problem behavior.

Fifth, researchers are well advised to consider the reciprocal causal impact of crime and substance use on work involvement. Although identification is equally, if not more, difficult in a non-recursive model, existing research provides several promising avenues for making defensible exclusion restrictions that do not rely on past behavior as an instrumental variable for current behavior (as do Thornberry and Christenson, 1984). For example, Levitt (1998) uses the relative punitiveness of juvenile and adult criminal sanctions around the age of majority to assess the deterrent effect of said sanctions on crime. Several researchers have successfully used cross-state variation in cigarette prices, tobacco control expenditure, clean indoor air laws, beer and liquor taxes, alcohol advertising, residence in dry counties, minimum legal drinking age laws, blood alcohol concentration (BAC) limits, and marijuana decriminalization statutes in a variety of substance use demand equations (e.g., Beenstock and Rahav, 2003; Chatterji, 2003; Chatterji and Markowitz, 2000; Cook and Tauchen, 1982; Dave and Kaestner, 2002; Dee, 2001; Dee and Evans, 2003; DeSimone, 2002; Evans and Ringel, 1999; Farrell et al., 2003; Liang and Chaloupka, 2002; Mullahy and Portney, 1990; Mullahy and Sindelar, 1996; Ohsfeldt and Morrisey, 1997; Ruhm, 1996; Saffer and Dave, 2003; Tauras and Liang, 2003). There are thus a number of variables that could be used as instrumental variables for crime and substance use (along with state child labor laws as instrumental variables for work intensity) in a simultaneous equations system that models feedback effects from problem behavior to youth employment.

CONCLUSION

This study was motivated by two factors—the consistency of the positive relationship between school-year work intensity and problem behavior, and the National Research Council’s (1998) recommendation that federal child labor laws be changed to allow fewer hours of employment while youths are enrolled in school. Drawing on recent econometric advances in the estimation of “treatment effects” when receipt of the treatment is endogenous, I used state child labor laws as instrumental variables in a fixed-effects panel model. This method arguably provides the best estimates to date of the causal relationship between youth employment and problem behavior.

The findings herein suggest that the causal relationship between school-year work intensity and crime is actually negative, thus challenging most prior empirical research and one of the justifications for the National Research Council’s policy recommendation. The relationship between school-year work intensity and substance use is less consistent, and tends to vary as a function of how the empirical model is formulated. There is support for the contention that longer work hours increase substance use (fixed effects estimation) and support for the contention that longer work hours have no effect on substance use (first differences estimation). Thus, only the results for crime are conclusive, suggesting that work involvement is indeed beneficial and that prior research has been plagued by the selection problem.

The next challenge is to better understand the processes of selection and causation that have been addressed here. Disentangling these two processes should be a priority for future youth employment research. Of particular theoretical and policy import where crime and delinquency are concerned is the school-work nexus, as supplementary

analysis suggests dual processes of detachment from one institution (the school) followed by re-attachment to another (the workplace). Criminological research would benefit from further elaboration of these processes, a first step toward which I have attempted in this study.

TABLE 1
Comparison of Employment Estimates from the National Longitudinal Survey of Youth
1997 with Existing Surveys of High-School Youths

	% Employed			% Working at High Intensity		
	10th Grade	11th Grade	12th Grade	10th Grade	11th Grade	12th Grade
National Longitudinal Survey of Youth 1997 (NLSY)						
Formal Work	25% (19 hrs.)	44% (22 hrs.)	56% (26 hrs.)	33%	42%	55%
	----- 38% ----- (23 hrs.)					
Any Work (Formal or Informal)	50%	59%	65%			
	----- 56% -----					
<hr/>						
Study Author(s), Year (Survey)						
<hr/>						
<u>Lower Range (40-50% employed)</u>						
Greenberger et al., 1981 (Orange County, California)	----- 40% -----					
Agnew, 1986 (Youth in Transition)		46% (21 hrs.)				
Steinberg and Dornbusch, 1991 (California and Wisconsin)	----- 45% -----			----- 28% -----		
<hr/>						
<u>Middle Range (50-70% employed)</u>						
Tanner and Krahn, 1991 (Canada)			70/57% ^a (18 hrs.)			
Mortimer, 2003 (Youth Development Study)	47% (17 hrs.)	58% (20 hrs.)	64% (21 hrs.)	28%	40%	40%
<hr/>						
<u>Upper Range (over 70% employed)</u>						
Bachman et al., 1986 (Monitoring the Future)			80%			39%
Bachman and Schulenberg, 1993 (Monitoring the Future)			74%			42%
Marsh, 1991 (High School and Beyond)	64% (6 hrs.)	70% (12 hrs.)	77% (13 hrs.)			
Wright et al., 2002 (Tri-Cities Survey)			86/75% ^a (18 hrs.)			

Note: ^a = the first figure denotes the percentage that held a job at some point during the current year, while the second figure denotes the percentage currently employed. Mean work intensity among employed youths is provided in parentheses (if provided in the original study). High-intensity work is defined as that over 20 hours per week.

TABLE 2
Descriptive Statistics Pooled across Five Waves ($N = 8,724$, $NT = 40,283$)

Variables	Range	Mean (Std. Dev.)	% Change
Problem Behavior			
Annual Crime	0–22	1.034 (2.228)	68.3
Recent Substance Use	0–4	1.103 (1.334)	77.2
Formal Work Hours during the School Year			
Annual School-Year Work Hours	0–67	14.445 (16.27)	89.9
Recent School-Year Work Hours	0–70	10.868 (15.82)	80.6
Individual-Level Control Variables			
Age	12.17–22.33	17.265 (2.173)	100.0
Household Size	1–17	4.178 (1.532)	68.6
Local Unemployment Rate	1.0–18.8	4.676 (2.082)	100.0
Educational Attainment:			
Enrolled in High School (ref.)	0–1	0.651 (0.477)	70.0
High-School Dropout or GED	0–1	0.096 (0.294)	18.9
High-School Graduate	0–1	0.253 (0.435)	54.1
Family Structure:			
Two Parents (ref.)	0–1	0.603 (0.489)	31.3
Single Mother	0–1	0.214 (0.410)	21.6
No Mother Figure	0–1	0.070 (0.255)	11.6
Live Independently	0–1	0.100 (0.300)	26.2
Residential Location:			
Suburban Area (ref.)	0–1	0.540 (0.498)	17.0
Central City	0–1	0.265 (0.441)	14.2
Rural Area	0–1	0.195 (0.396)	7.2
County Control Variables			
Local Unemployment Rate	1.00–18.80	4.677 (2.082)	100.0
Per Capita Personal Income (thousands)	11.55–84.27	25.928 (6.946)	98.2
Total Number of Jobs (thousands)	1.35–5513.90	508.635 (990.6)	98.0
Manufacturing Jobs (thousands)	0.01–700.88	58.926 (124.0)	98.0
Retail Jobs (thousands)	0.08–787.85	78.255 (142.3)	98.0
Service Jobs (thousands)	0.15–2098.94	179.177 (368.7)	98.0
Government Jobs (thousands)	0.16–607.88	59.584 (109.3)	98.0
Unemployment Insurance (thousands)	0.05–780.68	64.730 (130.9)	98.2
Medicaid, AFDC, & Food Stamps (thousands)	0.98–12042.74	793.912 (1873.4)	98.1

Note: “(ref.)” denotes the dummy variable reference category. Estimates are weighted. Standard deviations are not survey adjusted. The figures in the last column represent the percentage of the N respondents that change at least once during the five interviews.

TABLE 3
Problem Behavior Indicators Used in Scale Construction

Annual Crime Indicators	NLSY97 Definition
Vandalism	Purposely damaged or destroyed someone else's property.
Petty Theft (<\$50)	Stole something worth less than 50 dollars.
Petty Shoplifting	Took something from a store without paying for it.
Petty Larceny	Snatched someone's purse/wallet or picked someone's pocket.
Petty Burglary	Entered a locked house or building to steal something.
Petty Robbery	Used a weapon to steal something.
Major Theft (>\$50)	Stole something worth more than 50 dollars.
Major Shoplifting	Same definition as above.
Major Larceny	Same definition as above.
Major Burglary	Same definition as above.
Major Robbery	Same definition as above.
Vehicle Theft	Stole a motor vehicle (e.g., car, motorcycle) for own use or to sell it.
"Other" Property Crime	Committed other property crime (e.g., fencing, receiving, possessing, or selling stolen property).
Aggravated Assault	Attacked someone with the intent of seriously hurting them.
Selling Drugs	Sold or helped sell drugs.
Selling Marijuana	Sold marijuana or hashish (e.g., pot, grass, hash).
Selling Hard Drugs	Sold hard drugs (e.g., heroin, cocaine, LSD).
Carrying a Handgun	Carried a handgun (not a rifle or shotgun).
Income from Stolen Property	Received cash for stolen items.
Income from "Other" Property Crime	Received cash for fencing, receiving, possessing, or selling stolen property.
Income from Selling Drugs	Received cash for selling or helping to sell drugs.
Arrest for Criminal Offense	Arrested by police or taken into custody for an illegal or delinquent (not including for minor traffic violations).
<hr/>	
Recent Substance Use Indicators	
Smoked Cigarettes	Smoked a cigarette.
Drank Alcohol	Had a drink of an alcoholic beverage (e.g., can or bottle of beer, glass of wine, mixed drink, shot of liquor), not including childhood sips from an older person's drink.
Used Marijuana	Smoked marijuana (e.g., grass, pot).
Binge Drank	Had five or more drinks in a single occasion (at the same time or within hours of each other).

TABLE 4
Descriptive Statistics for Problem Behavior and Work Intensity, by Interview Year

Problem Behavior	Interview Year						Within Person
	Pooled	1997	1998	1999	2000	2001	
Annual Crime	1.034	1.806	1.058	0.833	0.783	0.615	1.058
<i>P</i> (Crime > 0)	.334	.526	.340	.292	.262	.231	.686
Mean Crime > 0	3.098	3.434	3.114	2.851	2.991	2.666	2.761
Recent Substance Use	1.103	.600	.960	1.191	1.321	1.499	1.104
<i>P</i> (Substance > 0)	.504	.297	.455	.541	.588	.661	.789
Mean Substance > 0	2.189	2.019	2.108	2.201	2.248	2.269	2.029
<hr/>							
Formal Work Hours during the School Year							
Annual School-Year Work Hours	14.445	3.604	10.131	15.265	20.903	23.672	14.492
<i>P</i> (Hours > 0)	.573	.219	.475	.622	.774	.818	.900
Mean Hours > 0	25.194	16.444	21.349	24.539	27.022	28.931	24.627
Recent School-Year Work Hours	10.868	2.075	7.071	11.551	16.295	18.459	10.876
<i>P</i> (Hours > 0)	.422	.129	.333	.462	.587	.634	.806
Mean Hours > 0	25.754	16.135	21.219	25.008	27.775	29.118	24.956
<hr/>							
Mean Age	17.265	14.837	16.459	17.409	18.465	19.456	17.267
# of Observations	40,283	8,587	8,190	7,996	7,843	7,667	8,724

Note: Estimates are weighted. For annual problem behavior, the 1997 interview references lifetime prevalence. For annual work intensity, the 1997 interview references since the 14th birthday. The figures in the final column are pooled within individuals across the five interviews. For example, 0.686 for “*P*(Crime > 0)” is the proportion of NLSY youths who report committing crime at least once across all five waves, with a mean 2.761 (“Mean | Crime > 0”) offense types when engaged in crime.

TABLE 5
Instrumental Variables for School-Year Work Hours in a Formal Job, as of January 1,
1997

Weekly Hours Restrictions during the School Year by Age Eligibility	# of States	States Where Applicable
<u>Restrictions for 14-15 Year Olds</u>		
Under 20 hours maximum	21	AL,AZ,CA,CT,DE,FL,IN,KY,LA,ME,MI,NJ,ND, OH,OK,PA,SC,TN,VA,WA,WI
20-39 hours maximum	10	AK,IL,IA,MD,MT,NH,NY,NC,OR,SD
40+ hours maximum	19	AR,CO,DC,GA,HI,ID,KS,MA,MN,MS,MO,NE,NV, RI,TX,UT,VT,WV,WY
No restriction	1	NM
<u>Restrictions for 16-17 Year Olds</u>		
20-39 hours maximum	9	CA,FL,ME,MI,NH,NY,PA,WA,WI
40+ hours maximum	12	AR,CO,CT,DC,IN,KY,MA,NJ,ND,OR,RI,VT
No restriction	30	AL,AK,AZ,DE,GA,HI,ID,IL,IA,KS,LA,MD,MN, MS,MO,MT,NE,NV,NM,NC,OH,OK,SC,SD,TN, TX,UT,VA,WV,WY

Source: *Monthly Labor Review* (Nelson, 1996-2003); National Research Council (1998:194-211); U.S. Department of Labor (online at <http://youthrules.dol.gov/states.htm>).

TABLE 6
Proportion of NLSY Respondents Subject to State Child Labor Laws, by Interview Year

Child Labor Law (Age Eligibility)	Interview Year						Within Person
	Pooled	1997	1998	1999	2000	2001	
Weekly hours restrictions							
(1) Under 20 hours maximum (14-15)	.115	.221	.215	.119	.005	.000	.391
(2) 20-39 hours maximum (14-15)	.045	.085	.082	.048	.002	.000	.152
(3) 40+ hours maximum (14-15)	.052	.100	.096	.056	.002	.000	.179
(4) 20-39 hours maximum (16-17)	.119	.090	.144	.147	.140	.072	.342
(5) 40+ hours maximum (16-17)	.052	.044	.061	.065	.061	.030	.150
# of Observations	40,283	8,587	8,190	7,996	7,843	7,667	8,724

Note: Estimates are weighted. Proportions refer to the proportion of the sample that resides in a state governed by each child labor law and that is also within the age-eligible range. The figures in the final column are pooled within individuals across the five interviews. For example, 0.391 for law (1) is the proportion of NLSY youths who are required to abide by the law during at least one of the five waves.

TABLE 7
First-Stage, Fixed-Effects Models of Annual School-Year Work Intensity

D.V. = Annual Work Hours	Model 1	Model 2	Model 3
Weekly Hours Restrictions			
Under 20 Hours Max., 14-15	-18.6331 (.5456)***	-16.5768 (.5232)***	-15.5437 (.5339)***
20-39 Hours Max., 14-15	-18.2638 (.5457)***	-16.1673 (.5654)***	-15.1121 (.5747)***
40+ Hours Max., 14-15	-18.7501 (.5315)***	-16.7412 (.5302)***	-15.7188 (.5410)***
20-39 Hours Max., 16-17	-2.3763 (.3239)***	-1.8776 (.3146)***	-1.8172 (.3148)***
40+ Hours Max., 16-17	-1.0314 (.4033)*	-.8126 (.3932)*	-.7667 (.3936)+
Individual Control Variables			
Age		3.9032 (.7041)***	4.0802 (.7060)***
Age Squared		-.0690 (.0197)***	-.0780 (.0198)***
Household Size		-.1346 (.0958)	-.1299 (.0961)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		7.7179 (.4164)***	7.5928 (.4150)***
HS Graduate		6.3217 (.3200)***	6.1193 (.3209)***
Family Structure:			
Two Parents (ref.)			
Single Mother		-.0826 (.3504)	-.1151 (.3501)
No Mother Figure		1.1461 (.5078)*	1.1150 (.5083)*
Live Independently		3.2532 (.4471)***	3.1439 (.4454)***
Residential Location:			
Suburban Area (ref.)			
Central City		-.5135 (.4418)	-.5587 (.4337)
Rural Area		-.6916 (.6338)	-.7234 (.6607)
County Control Variables			
Local Unemployment Rate			.3389 (.0565)***
Per Capita Personal Income			.1339 (.0392)***
Total Number of Jobs			.0030 (.0071)
Manufacturing Jobs			-.0367 (.0096)***
Retail Jobs			.0128 (.0213)
Service Jobs			.0013 (.0112)
Government Jobs			-.0198 (.0124)
Unemployment Insurance			-.0007 (.0036)
Medicaid, AFDC, & Food Stamps			.0002 (.0003)
<i>df</i>	31,499	31,489	31,479
<i>F</i> -test for instruments	263.86	191.22	162.46
Partial <i>R</i> -square for instruments	.0461	.0309	.0262

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies.

TABLE 8
First-Stage, Fixed-Effects Models of Recent School-Year Work Intensity

D.V. = Recent Work Hours	Model 1	Model 2	Model 3
Weekly Hours Restrictions			
Under 20 Hours Max., 14-15	-14.6663 (.6159)***	-13.0305 (.6264)***	-12.5402 (.6349)***
20-39 Hours Max., 14-15	-14.2493 (.6441)***	-12.6226 (.6590)***	-12.0740 (.6680)***
40+ Hours Max., 14-15	-14.1029 (.6539)***	-12.5305 (.6650)***	-12.0784 (.6738)***
20-39 Hours Max., 16-17	-2.6872 (.3396)***	-2.2720 (.3338)***	-2.2394 (.3346)***
40+ Hours Max., 16-17	-.8076 (.4657)+	-.6051 (.4574)	-.5711 (.4600)
Individual Control Variables			
Age		.2039 (.7397)	.2236 (.7393)
Age Squared		.0285 (.0211)	.0262 (.0211)
Household Size		-.0998 (.1071)	-.0999 (.1070)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		5.4011 (.4482)***	5.3429 (.4483)***
HS Graduate		5.6511 (.3516)***	5.5364 (.3532)***
Family Structure:			
Two Parents (ref.)			
Single Mother		-.2894 (.3932)	-.2712 (.3939)
No Mother Figure		1.2433 (.5543)*	1.2236 (.5565)*
Live Independently		3.5954 (.5188)***	3.5438 (.5192)***
Residential Location:			
Suburban Area (ref.)			
Central City		.1305 (.4986)	.1439 (.4944)
Rural Area		-1.3014 (.7067)+	-1.1863 (.7347)
County Control Variables			
Local Unemployment Rate			.1355 (.0607)*
Per Capita Personal Income			.0929 (.0423)*
Total Number of Jobs			-.0076 (.0080)
Manufacturing Jobs			-.0220 (.0109)*
Retail Jobs			.0302 (.0236)
Service Jobs			.0242 (.0128)+
Government Jobs			-.0283 (.0139)*
Unemployment Insurance			-.0035 (.0040)
Medicaid, AFDC, & Food Stamps			.0002 (.0003)
<i>df</i>	31,499	31,489	31,479
<i>F</i> -test for instruments	123.02	87.49	79.38
Partial <i>R</i> -square for instruments	.0266	.0175	.0156

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). *NT* = 40,283. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies.

TABLE 9
Random-Effects Models of the Effect of School-Year Work Intensity on Annual Crime

D.V. = Annual Crime	Model 1	Model 2	Model 3
School-Year Work Hours	.0020 (.0010)*	.0025 (.0010)*	.0024 (.0010)*
Individual Control Variables			
Age		.9669 (.1395)***	.9403 (.1399)***
Age Squared		-.0287 (.0037)***	-.0275 (.0037)***
Household Size		-.0080 (.0115)	-.0078 (.0115)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		.2903 (.0702)***	.2883 (.0702)***
HS Graduate		.1275 (.0458)**	.1202 (.0458)**
Family Structure:			
Two Parents (ref.)			
Single Mother		.2258 (.0435)***	.2284 (.0436)***
No Mother Figure		.3777 (.0734)***	.3813 (.0731)***
Live Independently		-.0197 (.0554)	-.0205 (.0552)
Residential Location:			
Suburban Area (ref.)			
Central City		-.0146 (.0439)	.0055 (.0449)
Rural Area		.0331 (.0557)	.0775 (.0592)
County Control Variables			
Local Unemployment Rate			-.0355 (.0087)***
Per Capita Personal Income			-.0019 (.0032)
Total Number of Jobs			.0006 (.0006)
Manufacturing Jobs			.0020 (.0010)*
Retail Jobs			-.0009 (.0021)
Service Jobs			-.0020 (.0010)+
Government Jobs			.0003 (.0010)
Unemployment Insurance			-.0005 (.0004)
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	40,227	40,217	40,207

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). *NT* = 40,283. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies.

TABLE 10
Fixed-Effects Models of the Effect of School-Year Work Intensity on Annual Crime

D.V. = Annual Crime	Model 1	Model 2	Model 3
School-Year Work Hours	-.0048 (.0011)***	-.0036 (.0011)**	-.0033 (.0011)**
Individual Control Variables			
Age		.7397 (.1252)***	.7188 (.1254)***
Age Squared		-.0216 (.0033)***	-.0208 (.0033)***
Household Size		-.0021 (.0154)	-.0016 (.0154)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		-.2448 (.0767)***	-.2277 (.0767)**
HS Graduate		.0936 (.0421)*	.1221 (.0429)**
Family Structure:			
Two Parents (ref.)			
Single Mother		.0877 (.0626)	.0890 (.0625)
No Mother Figure		.2111 (.0916)*	.2178 (.0913)*
Live Independently		-.2712 (.0701)***	-.2523 (.0698)***
Residential Location:			
Suburban Area (ref.)			
Central City		-.0307 (.0754)	-.0177 (.0760)
Rural Area		.0516 (.1049)	.0223 (.1062)
County Control Variables			
Local Unemployment Rate			-.0228 (.0099)*
Per Capita Personal Income			-.0046 (.0052)
Total Number of Jobs			.0022 (.0013)+
Manufacturing Jobs			.0022 (.0016)
Retail Jobs			-.0077 (.0041)+
Service Jobs			-.0030 (.0020)
Government Jobs			-.0028 (.0024)
Unemployment Insurance			-.0008 (.0005)+
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	31,503	31,493	31,483
Hausman Test (FE vs. RE)	14.19	11.92	10.73

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies. The Hausman test is a t -statistic computed for "school-year work hours."

TABLE 11
Fixed-Effects Instrumental Variables Models of the Effect of School-Year Work Intensity
on Annual Crime

D.V. = Annual Crime	Model 1	Model 2	Model 3
School-Year Work Hours	-.0175 (.0044)***	-.0309 (.0057)***	-.0282 (.0062)***
<u>Individual Control Variables</u>			
Age		.8500 (.1311)***	.8275 (.1315)***
Age Squared		-.0242 (.0035)***	-.0235 (.0035)***
Household Size		-.0068 (.0158)	-.0057 (.0157)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		.0063 (.0941)	-.0074 (.0950)
HS Graduate		.3349 (.0670)***	.3276 (.0676)***
Family Structure:			
Two Parents (ref.)			
Single Mother		.0860 (.0653)	.0863 (.0647)
No Mother Figure		.2422 (.0959)*	.2447 (.0954)**
Live Independently		-.1613 (.0740)*	-.1589 (.0738)*
Residential Location:			
Suburban Area (ref.)			
Central City		-.0460 (.0789)	-.0344 (.0787)
Rural Area		.0398 (.1079)	.0079 (.1083)
<u>County Control Variables</u>			
Local Unemployment Rate			-.0082 (.0110)
Per Capita Personal Income			.0001 (.0055)
Total Number of Jobs			.0023 (.0013)+
Manufacturing Jobs			.0008 (.0017)
Retail Jobs			-.0071 (.0041)+
Service Jobs			-.0029 (.0020)
Government Jobs			-.0034 (.0025)
Unemployment Insurance			-.0007 (.0005)
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	31,503	31,493	31,483
Overidentification Test (Hansen's <i>J</i>)	2.38	4.05	4.10
Hausman Test (FEIV vs. RE)	4.56	4.68	5.00
Hausman Test (FEIV vs. FE)	2.98	3.28	4.10

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies. Hansen's J -statistic is distributed chi-square ($df = 4$). The Hausman test is a t -statistic computed for "school-year work hours."

TABLE 12
Random-Effects Models of the Effect of School-Year Work Intensity on Recent
Substance Use

D.V. = Recent Substance Use	Model 1	Model 2	Model 3
School-Year Work Hours	.0054 (.0006)***	.0039 (.0006)***	.0038 (.0006)***
<u>Individual Control Variables</u>			
Age		.6345 (.0693)***	.6327 (.0698)***
Age Squared		-.0140 (.0019)***	-.0138 (.0019)***
Household Size		-.0341 (.0062)***	-.0334 (.0062)***
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		.1461 (.0317)***	.1452 (.0317)***
HS Graduate		.1433 (.0285)***	.1425 (.0285)***
Family Structure:			
Two Parents (ref.)			
Single Mother		.0425 (.0246)+	.0436 (.0246)+
No Mother Figure		.0691 (.0355)+	.0721 (.0356)*
Live Independently		-.1074 (.0328)***	-.1062 (.0330)***
Residential Location:			
Suburban Area (ref.)			
Central City		-.0323 (.0254)	-.0132 (.0258)
Rural Area		-.0193 (.0331)	-.0182 (.0351)
<u>County Control Variables</u>			
Local Unemployment Rate			-.0090 (.0045)*
Per Capita Personal Income			-.0015 (.0021)
Total Number of Jobs			-.0001 (.0004)
Manufacturing Jobs			.0014 (.0006)*
Retail Jobs			.0005 (.0011)
Service Jobs			-.0004 (.0006)
Government Jobs			.0004 (.0006)
Unemployment Insurance			.0002 (.0002)
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	40,227	40,217	40,207

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies.

TABLE 13
Fixed-Effects Models of the Effect of School-Year Work Intensity on Recent Substance Use

D.V. = Recent Substance Use	Model 1	Model 2	Model 3
School-Year Work Hours	.0055 (.0006)***	.0041 (.0006)***	.0040 (.0006)***
<u>Individual Control Variables</u>			
Age		.4583 (.0644)***	.4737 (.0648)***
Age Squared		-.0118 (.0017)***	-.0124 (.0018)***
Household Size		-.0093 (.0076)	-.0087 (.0077)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		.1103 (.0340)***	.0983 (.0341)**
HS Graduate		.3671 (.0249)***	.3474 (.0252)***
Family Structure:			
Two Parents (ref.)			
Single Mother		.0418 (.0315)	.0403 (.0316)
No Mother Figure		.0758 (.0428)+	.0751 (.0429)+
Live Independently		-.0431 (.0353)	-.0512 (.0355)
Residential Location:			
Suburban Area (ref.)			
Central City		.0761 (.0369)*	.0728 (.0370)*
Rural Area		-.0598 (.0508)	-.0654 (.0526)
<u>County Control Variables</u>			
Local Unemployment Rate			.0201 (.0049)***
Per Capita Personal Income			.0044 (.0031)
Total Number of Jobs			.0007 (.0006)
Manufacturing Jobs			-.0019 (.0008)*
Retail Jobs			-.0002 (.0018)
Service Jobs			-.0009 (.0009)
Government Jobs			-.0019 (.0010)+
Unemployment Insurance			.0005 (.0003)*
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	31,503	31,493	31,483
Hausman Test (FE vs. RE)	1.60	2.43	1.25

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies. The Hausman test is a t -statistic computed for “school-year work hours.”

TABLE 14
Fixed-Effects Instrumental Variables Models of the Effect of School-Year Work Intensity
on Recent Substance Use

D.V. = Recent Substance Use	Model 1	Model 2	Model 3
School-Year Work Hours	.0437 (.0036)***	.0445 (.0045)***	.0429 (.0047)***
<u>Individual Control Variables</u>			
Age		.4458 (.0725)***	.4564 (.0724)***
Age Squared		-.0121 (.0020)***	-.0124 (.0020)***
Household Size		-.0041 (.0088)	-.0039 (.0088)
Educational Attainment:			
Enrolled in HS (ref.)			
HS Dropout or GED		-.1551 (.0476)***	-.1486 (.0478)**
HS Graduate		.0577 (.0441)	.0644 (.0442)
Family Structure:			
Two Parents (ref.)			
Single Mother		.0531 (.0353)	.0510 (.0351)
No Mother Figure		.0253 (.0504)	.0281 (.0501)
Live Independently		-.2119 (.0450)***	-.2072 (.0449)***
Residential Location:			
Suburban Area (ref.)			
Central City		.0723 (.0437)+	.0704 (.0434)
Rural Area		-.0172 (.0575)	-.0251 (.0606)
<u>County Control Variables</u>			
Local Unemployment Rate			.0071 (.0057)
Per Capita Personal Income			-.0009 (.0036)
Total Number of Jobs			.0011 (.0007)
Manufacturing Jobs			-.0004 (.0009)
Retail Jobs			-.0017 (.0021)
Service Jobs			-.0020 (.0011)+
Government Jobs			-.0006 (.0012)
Unemployment Insurance			.0006 (.0003)+
Medicaid, AFDC, & Food Stamps			.0000 (.0000)
<i>df</i>	31,503	31,493	31,483
Overidentification Test (Hansen's <i>J</i>)	14.48	16.86	16.52
Hausman Test (FEIV vs. RE)	10.78	11.21	8.41
Hausman Test (FEIV vs. FE)	10.75	11.16	8.38

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Not shown but included in the models are a constant, a control for exposure time, age dummies, missing data dummies for county controls, and state dummies. Hansen's *J*-statistic is distributed chi-square ($df = 4$). The Hausman test is a *t*-statistic computed for "school-year work hours."

TABLE 15
Summary of Fixed-Effects Instrumental Variables Estimates of the Effect of School-Year
Work Intensity on Individual Problem Behaviors

Problem Behavior Response Variable	Prevalence			Frequency		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Annual Crime:						
Vandalism	-	-	-	-	-	-
Petty Theft (under 50 dollars)	-	-	-			
Petty Shoplifting	-	-	-			
Petty Larceny	NS	NS	NS			
Petty Burglary	-	-	-			
Petty Robbery	NS	NS	NS			
Major Theft (over 50 dollars)	-	-	-	NS	-	-
Major Shoplifting	NS	-	NS			
Major Larceny	NS	NS	NS			
Major Burglary	NS	NS	NS			
Major Robbery	NS	NS	NS			
Vehicle Theft	-	-	-			
“Other” Property Crime	-	-	-	NS	-	NS
Aggravated Assault	-	-	-	NS	NS	NS
Selling Drugs	NS	NS	NS	+	NS	NS
Selling Marijuana	NS	NS	NS			
Selling Hard Drugs	NS	NS	NS			
Carrying a Handgun	-	-	-	NS	NS	NS
Income from Stolen Property	-	-	-			
Income from “Other” Property Crime	-	-	-			
Income from Selling Drugs	NS	NS	NS			
Arrest for Criminal Offense	NS	NS	NS	NS	NS	NS
Recent Substance Use:						
Smoking Cigarettes	+	+	+	+	+	+
Drinking Alcohol	+	+	+	+	+	+
Using Marijuana	+	+	+	+	+	+
Binge Drinking	+	+	+	+	+	+

Note: “-” indicates a negative and significant coefficient; “+” indicates a positive and significant coefficient; “NS” indicates a non-significant coefficient ($p < .10$, two-tailed tests). Boxed symbols indicate that the overidentification test is rejected.

TABLE 16
Fixed-Effects Models of the Effect of School-Year Work Intensity on the Frequency of Smoking Cigarettes and Drinking Alcohol

First-Stage Fit Statistics	Model 1	Model 2	Model 3
<i>F</i> -test for instruments	123.02	87.49	79.38
Partial <i>R</i> -square for instruments	.0266	.0175	.0156

Frequency of Smoking Cigarettes			
Estimation Procedure	Model 1	Model 2	Model 3
Random Effects	.0549 (.0035)***	.0439 (.0036)***	.0438 (.0035)***
Fixed Effects	.0665 (.0045)***	.0477 (.0047)***	.0479 (.0046)***
Fixed Effects Instrumental Variables	.2805 (.0161)***	.2869 (.0244)***	.2981 (.0256)***
Overidentification Test (Hansen's <i>J</i>)	6.78	6.62	7.34
Hausman Test (FE vs. RE)	4.02	1.27	1.35
Hausman Test (FEIV vs. RE)	14.34	10.05	10.01
Hausman Test (FEIV vs. FE)	3.01	4.77	3.86

Frequency of Drinking Alcohol			
Estimation Procedure	Model 1	Model 2	Model 3
Random Effects	.0170 (.0017)***	.0089 (.0017)***	.0087 (.0017)***
Fixed Effects	.0252 (.0025)***	.0110 (.0026)***	.0103 (.0026)***
Fixed Effects Instrumental Variables	.1472 (.0088)***	.1412 (.0126)***	.1397 (.0132)***
Overidentification Test (Hansen's <i>J</i>)	4.12	4.91	4.72
Hausman Test (FE vs. RE)	4.57	1.10	0.84
Hausman Test (FEIV vs. RE)	15.16	10.58	10.01
Hausman Test (FEIV vs. FE)	14.52	10.53	9.99

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 40,283$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Hansen's *J*-statistic is distributed chi-square ($df = 4$). The Hausman test is a *t*-statistic computed for "school-year work hours." To facilitate interpretation of the results, the highlighted coefficients are the "preferred" estimates according to the Hausman tests.

TABLE 17
Summary of Fixed-Effects Instrumental Variables Estimates of the Effect of School-Year Work Intensity on Problem Behavior with Alternative Model Specifications

Alternative Model Specification	Annual Crime			Recent Substance Use		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Censored Work Hours:						
60 Hours	-	-	-	+	+	+
50 Hours	-	-	-	+	+	+
40 Hours	-	-	-	+	+	+
Ordinal Work Hours:						
Dichotomous (Work vs. No Work)	-	-	-	+	+	+
Trichotomous Hours (1-20, 21+ vs. 0)	NS / -	NS / -	NS / -	+/+	+/+	+/+
Quadratic in Work Hours	NS / NS	NS / NS	NS / NS	+/-	+/-	+/-
Adjustment for Level of Work Hours	-	-	-	+	+	+
Logged (+1) Work Hours	-	-	-	+	+	+
Lagged Y	-	-	-	+	+	+
Poisson with Fixed Effects	-	-	-	NS	NS	NS
First Differences	-	-	-	+	NS	NS
Smoking Cigarettes						
Drinking Alcohol						
Alternative Model Specification	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Censored Work Hours:						
60 Hours	+	+	+	+	+	+
50 Hours	+	+	+	+	+	+
40 Hours	+	+	+	+	+	+
Ordinal Work Hours:						
Dichotomous (Work vs. No Work)	+	+	+	+	+	+
Trichotomous Hours (1-20, 21+ vs. 0)	+/+	+/+	+/+	NS / +	NS / +	NS / +
Quadratic in Work Hours	+/-	+ / NS	+ / NS	+/NS	+/NS	+/NS
Adjustment for Level of Work Hours	+	+	+	+	+	+
Logged (+1) Work Hours	+	+	+	+	+	+
Lagged Y	+	+	+	+	+	+
Poisson with Fixed Effects	+	+	+	+	+	+
First Differences	NS	-	-	NS	NS	NS

Note: “-” indicates a negative and significant coefficient; “+” indicates a positive and significant coefficient; “NS” indicates a non-significant coefficient ($p < .10$, two-tailed tests). Boxed symbols indicate that the overidentification test is rejected. The Poisson model with fixed effects is a reduced-form model, and the symbols represent the implied second-stage “work intensity effect” based on the joint significance and direction of the coefficients for the instrumental variables.

TABLE 18
 First-Differences Models of the Effect of School-Year Work Intensity on the Frequency
 of Smoking Cigarettes and Drinking Alcohol

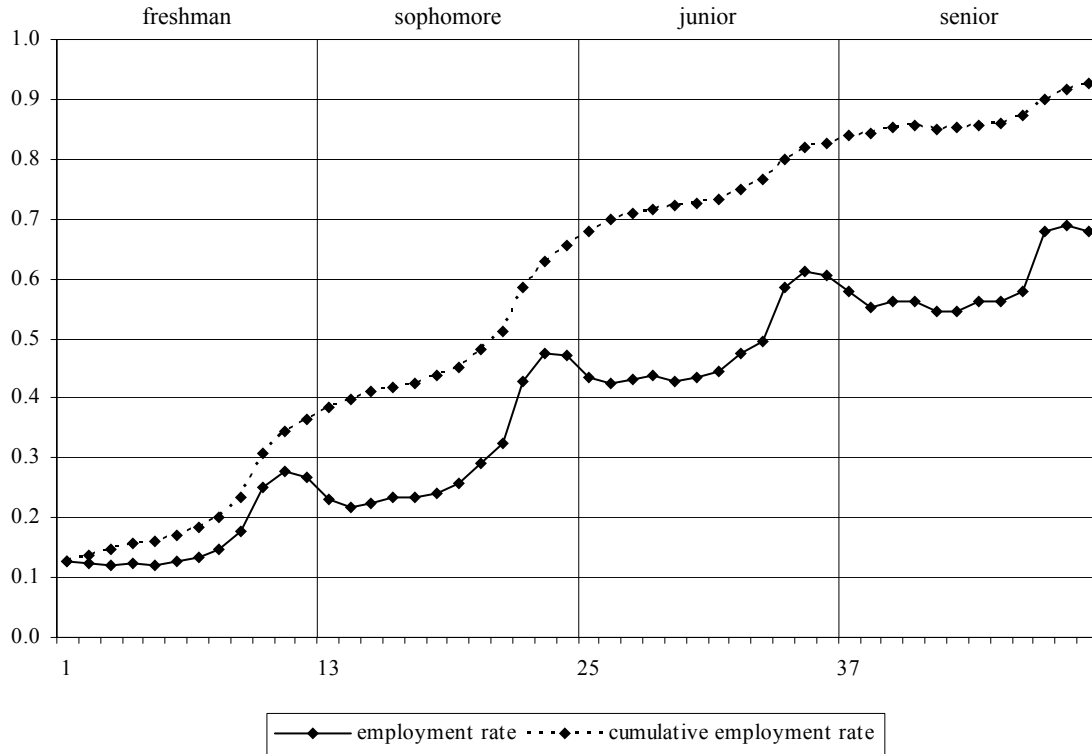
First-Stage Fit Statistics	Model 1	Model 2	Model 3
<i>F</i> -test for instruments	21.30	10.47	9.34
Partial <i>R</i> -square for instruments	.0043	.0017	.0016

Frequency of Smoking Cigarettes			
Estimation Procedure	Model 1	Model 2	Model 3
Random Effects	.0549 (.0035)***	.0439 (.0036)***	.0438 (.0035)***
First Differences	.0286 (.0045)***	.0255 (.0045)***	.0252 (.0045)***
First Differences IV	-.0487 (.0449)	-.0931 (.0362)***	-.0884 (.0413)**
Overidentification Test (Hansen's <i>J</i>)	6.78	6.62	7.34
Hausman Test (FD vs. RE)	9.21	6.66	6.79
Hausman Test (FDIV vs. RE)	3.01	4.77	3.86
Hausman Test (FDIV vs. FD)	2.26	4.15	3.33

Frequency of Drinking Alcohol			
Estimation Procedure	Model 1	Model 2	Model 3
Random Effects	.0170 (.0017)***	.0089 (.0017)***	.0087 (.0017)***
First Differences	.0051 (.0028)+	.0033 (.0028)	.0030 (.0028)
First Differences IV	-.0123 (.0201)	-.0240 (.0156)	-.0137 (.0186)
Overidentification Test (Hansen's <i>J</i>)	3.82	3.95	3.68
Hausman Test (FD vs. RE)	5.25	2.53	2.57
Hausman Test (FDIV vs. RE)	1.47	2.12	1.21
Hausman Test (FDIV vs. FD)	0.87	1.78	0.91

Note: + $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). $NT = 31,558$. Estimates are weighted. Survey-adjusted standard errors are in parentheses. Hansen's *J*-statistic is distributed chi-square ($df = 4$). The Hausman test is a *t*-statistic computed for "school-year work hours." To facilitate interpretation of the results, the highlighted coefficients are the "preferred" estimates according to the Hausman tests.

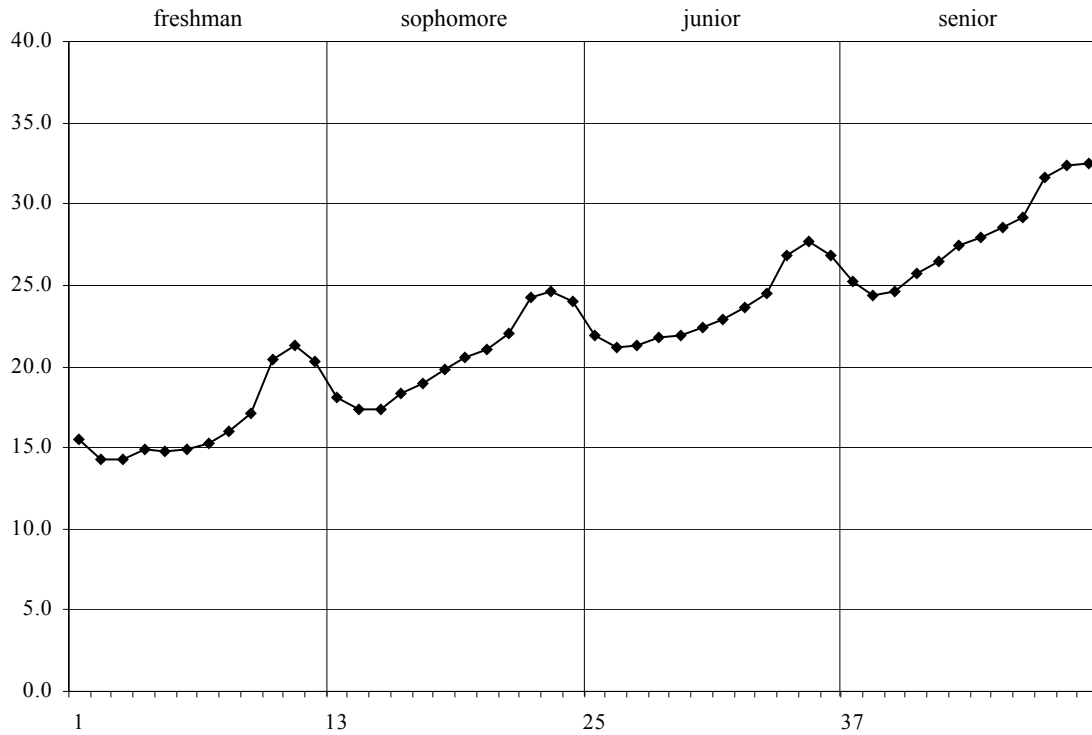
FIGURE 1
 Proportion of Youths Employed in Formal Jobs, by Month during High School



Note: $n = 8,984$. Estimates are weighted. The employment rate refers to the proportion of youths employed in a given month. The cumulative employment rate refers to the proportion of youths that have been employed at least once since the beginning of high school. Month 1 denotes September of a youth's freshman year of high school, month 13 the sophomore year, month 25 the junior year, and month 37 the senior year.

Source: National Longitudinal Survey of Youth 1997, rounds 1-3.

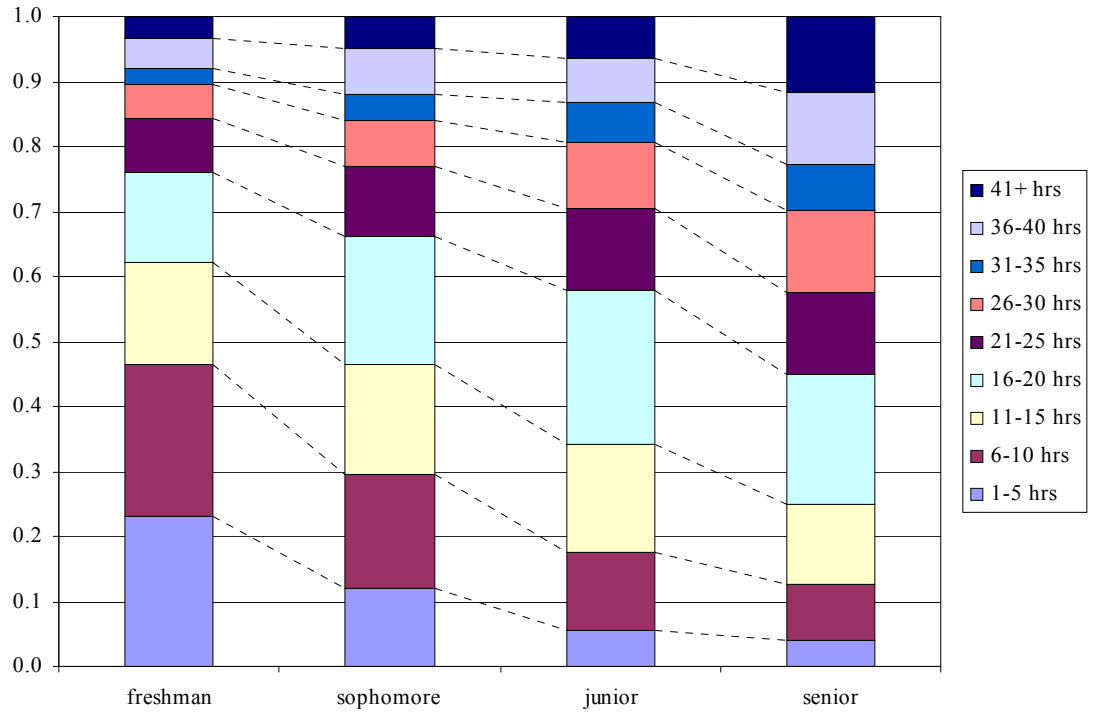
FIGURE 2
Continuous Mean Weekly Hours of Formal Employment, by Month during High School



Note: Estimates are weighted. Non-workers are excluded from the work intensity calculation. Month 1 denotes September of a youth's freshman year of high school, month 13 the sophomore year, month 25 the junior year, and month 37 the senior year.

Source: National Longitudinal Survey of Youth 1997, rounds 1-3.

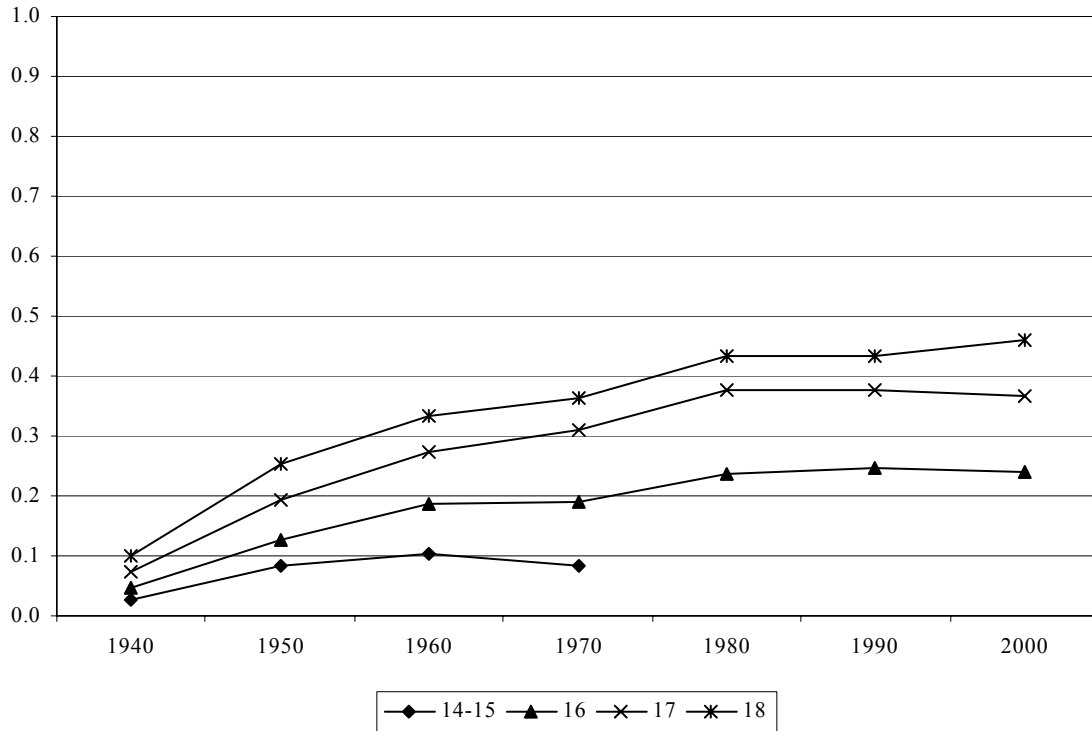
FIGURE 3
Categorical Mean Weekly Hours of Formal Employment, by Year of High School



Note: Estimates are weighted. Non-workers are excluded from the work intensity calculation. Work intensity is averaged over the academic year, from September to May.

Source: National Longitudinal Survey of Youth 1997, rounds 1-3.

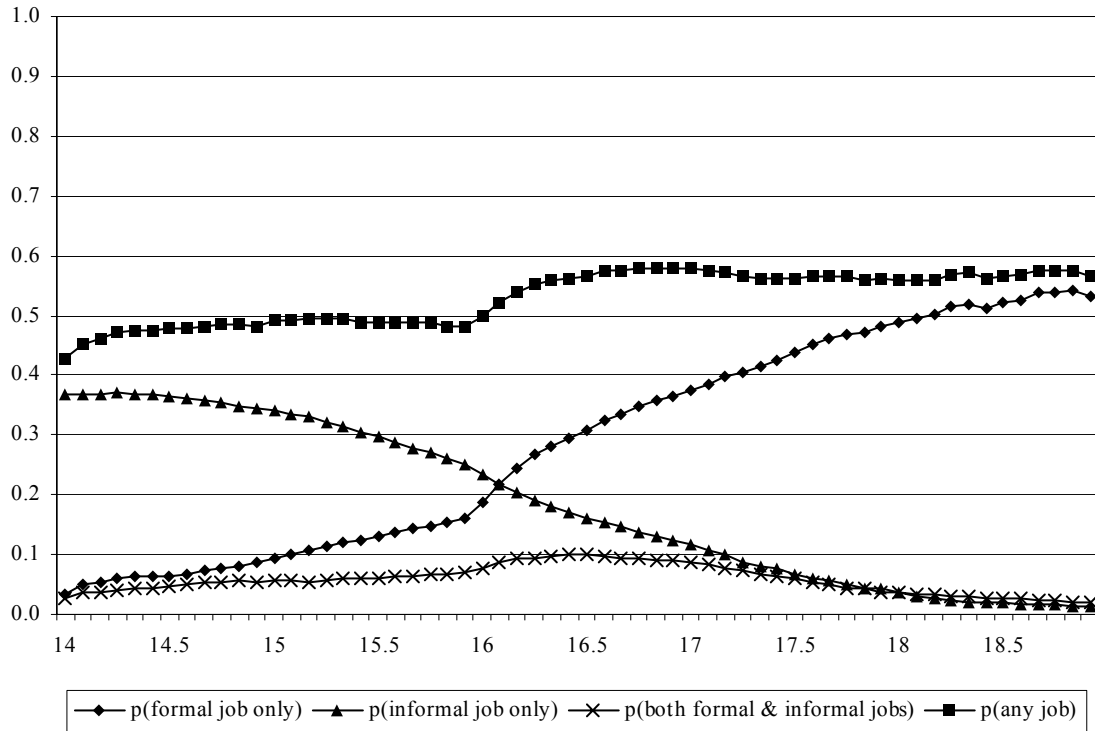
FIGURE 4
Proportion of Enrolled Youths Employed in Formal Work, by Age, 1940-2000



Note: Estimates are weighted. Employment rates for youths ages 14-15 are not available after 1970, and are illustrated here for comparative purposes only.

Source: Integrated Public Use Microdata Series, 1940-2000.

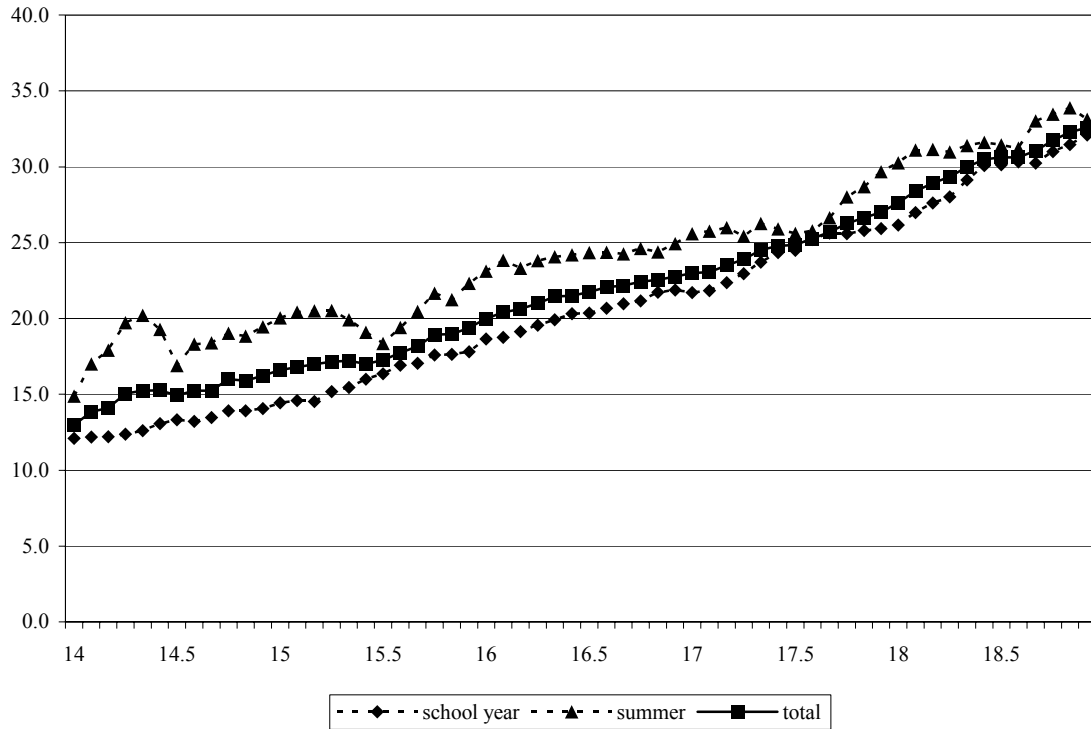
FIGURE 5
 Proportion of Youths Employed in Formal and Informal Jobs, by Age



Note: $n = 8,984$. Estimates are weighted. At any age, the sum of the data points for “formal job only,” “informal job only,” and “both formal and informal jobs” is the equivalent of “any job.”

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

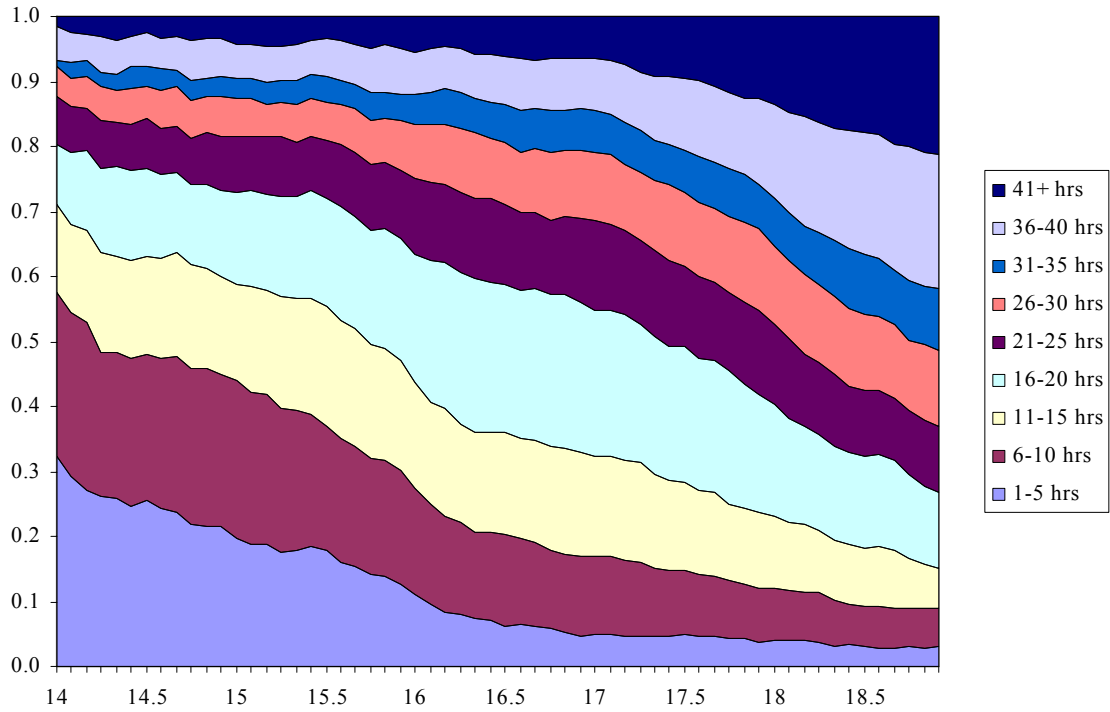
FIGURE 6
 Continuous Mean Weekly Hours of Formal Employment, by Age



Note: Estimates are weighted. Mean work intensity for summer employment is less stable due to computation from a smaller number of cases.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

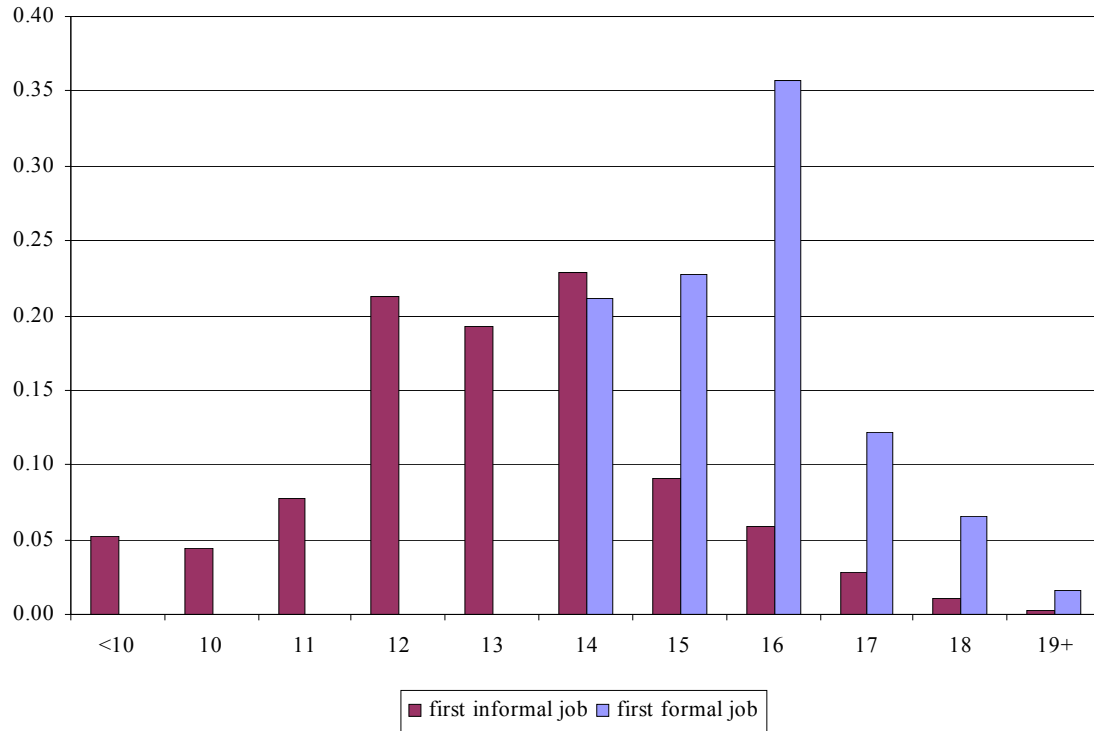
FIGURE 7
 Categorical Mean Weekly Hours of Formal Employment, by Age



Note: Estimates are weighted. Non-workers at each month are excluded from this figure. Summer months are included, but the inflation of work intensity estimates is modest. Only 25 percent of the observations at each month are during the summer.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

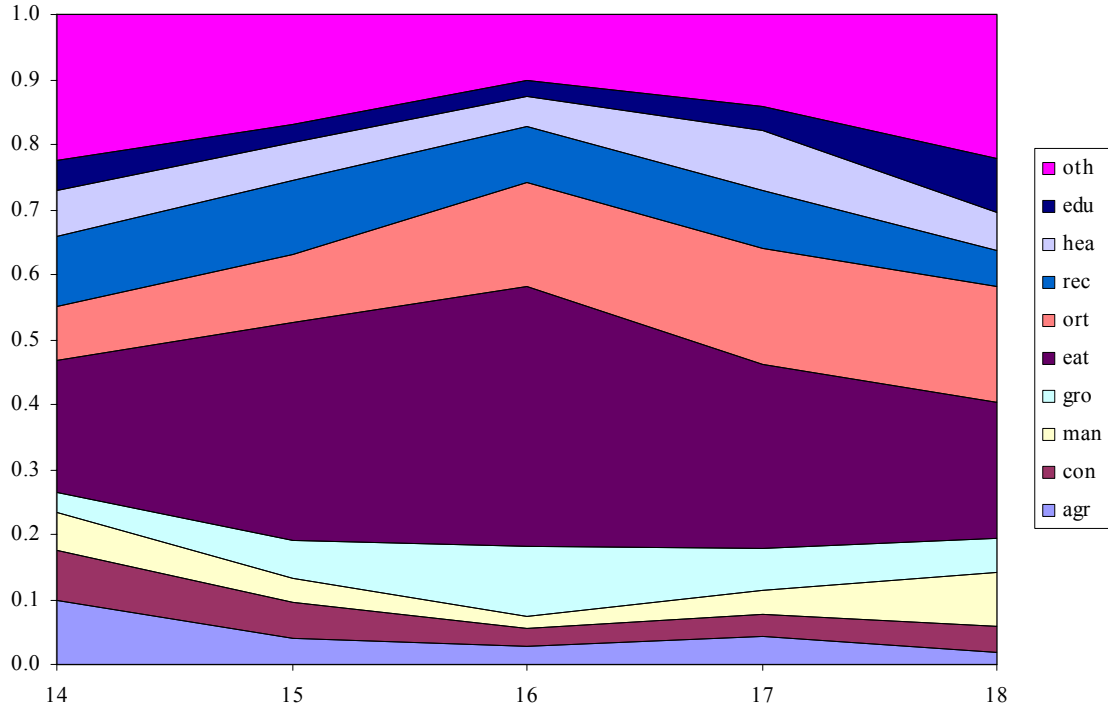
FIGURE 8
Age of First Employment in Formal and Informal Jobs



Note: $n = 7,552$ for first formal job; $n = 5,299$ for first informal job. Estimates are weighted. A portion of the total sample ($n = 8,984$) is censored due to missing data on age of first job, attrition, or having not yet taken a first job.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

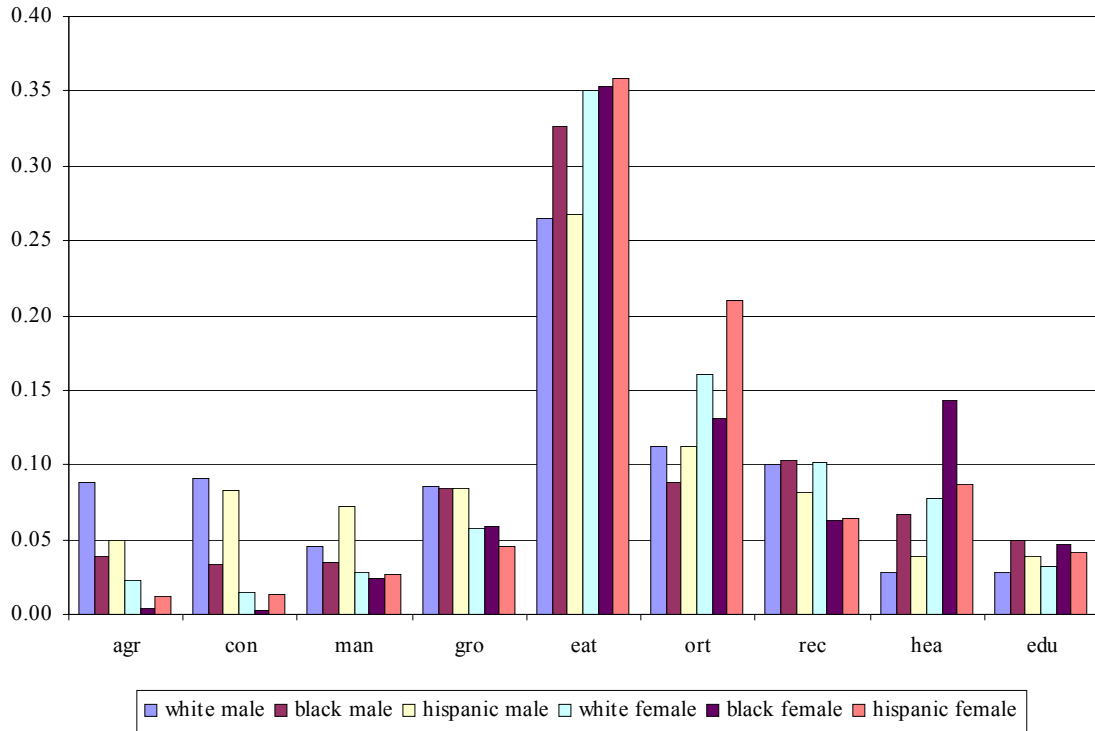
FIGURE 9
Share of All First Formal Jobs in Specific Industries, by Age



Note: $n = 7,366$. Estimates are weighted. Respondents contribute a single observation, depending on what age they acquired their first formal job. The figures represent the share of all first formal jobs at a given age in a specific industry. Job definitions include: “agr” = agricultural; “con” = construction; “man” = manufacturing; “gro” = grocery store; “eat” = eating and drinking establishment; “ort” = other retail trade; “rec” = entertainment and recreation service; “hea” = health and social service; “edu” = educational service; “oth” = other industry not elsewhere classified.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

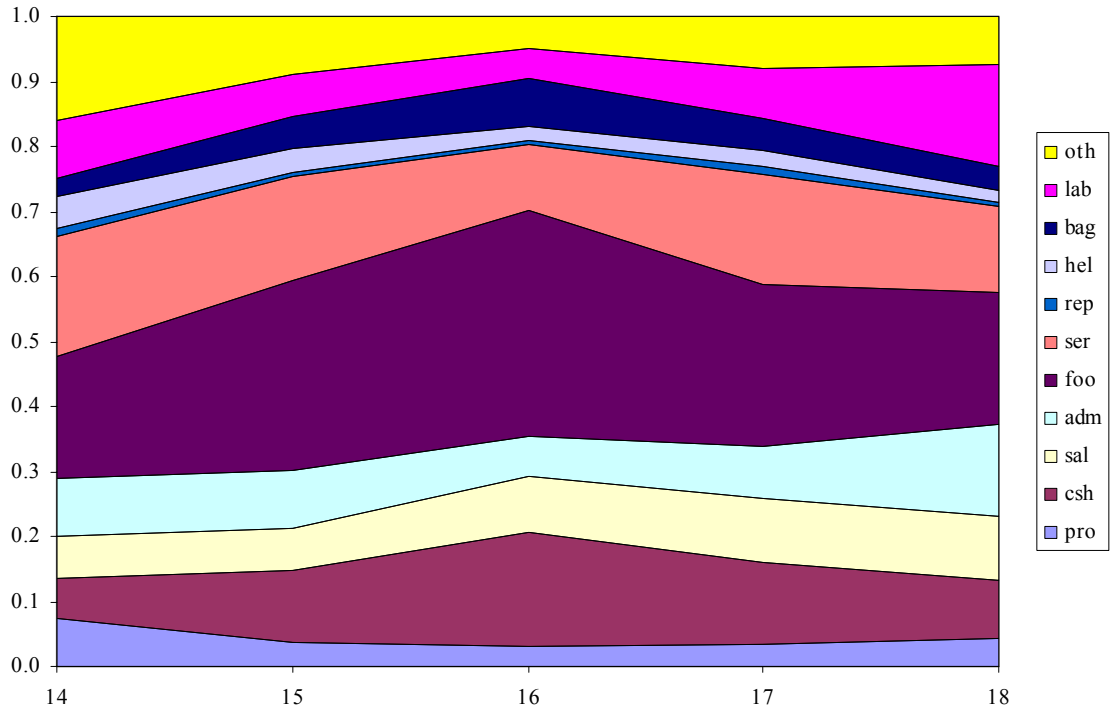
FIGURE 10
Share of All First Formal Jobs in Specific Industries, by Gender and Race



Note: Estimates are weighted. The figures represent the share of all first formal jobs in a specific industry. Job definitions include: “agr” = agricultural; “con” = construction; “man” = manufacturing; “gro” = grocery store; “eat” = eating and drinking establishment; “ort” = other retail trade; “rec” = entertainment and recreation service; “hea” = health and social service; “edu” = educational service. In order to conserve space, the “other” category is omitted from this graph.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

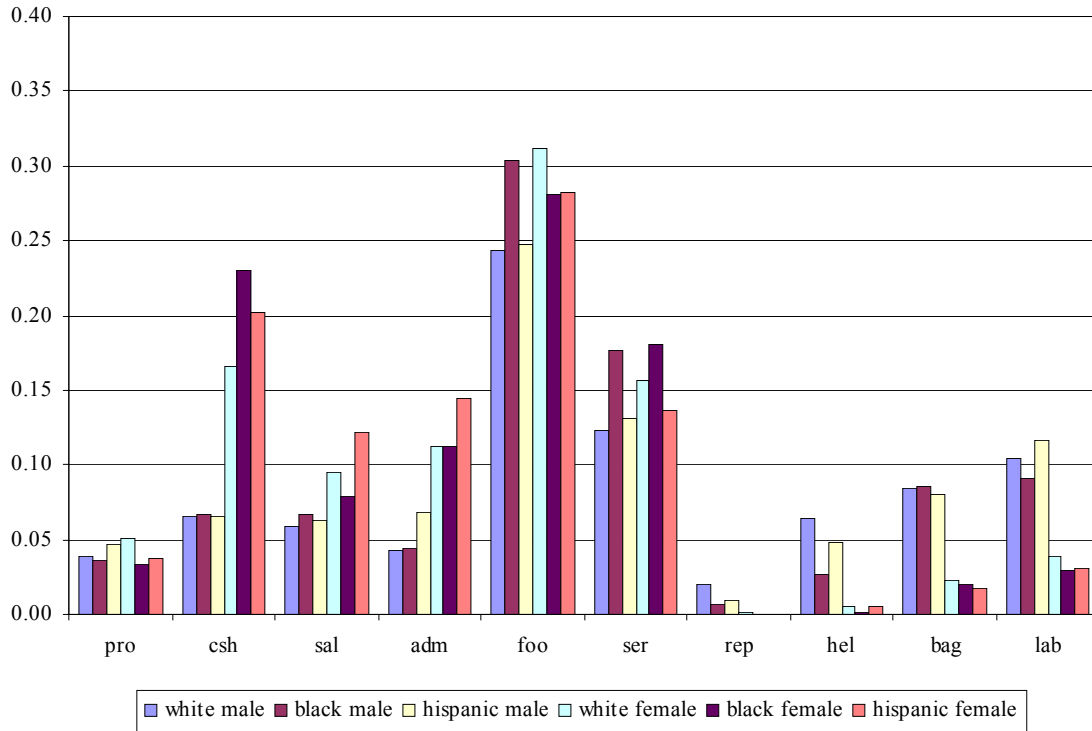
FIGURE 11
Share of All First Formal Jobs in Specific Occupations, by Age



Note: $n = 7,366$. Estimates are weighted. Respondents contribute a single observation, depending on what age they acquired their first formal job. The figures represent the share of all first formal jobs at a given age in a specific occupation. Job definitions include: “pro” = professional and managerial; “csh” = cashier; “sal” = sales; “adm” = administrative support; “foo” = food preparation and service; “ser” = other service; “rep” = repair, precision production, and craft; “hel” = construction helper; “bag” = stock handler or bagger; “lab” = other laborer; “oth” = other occupation not elsewhere classified.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

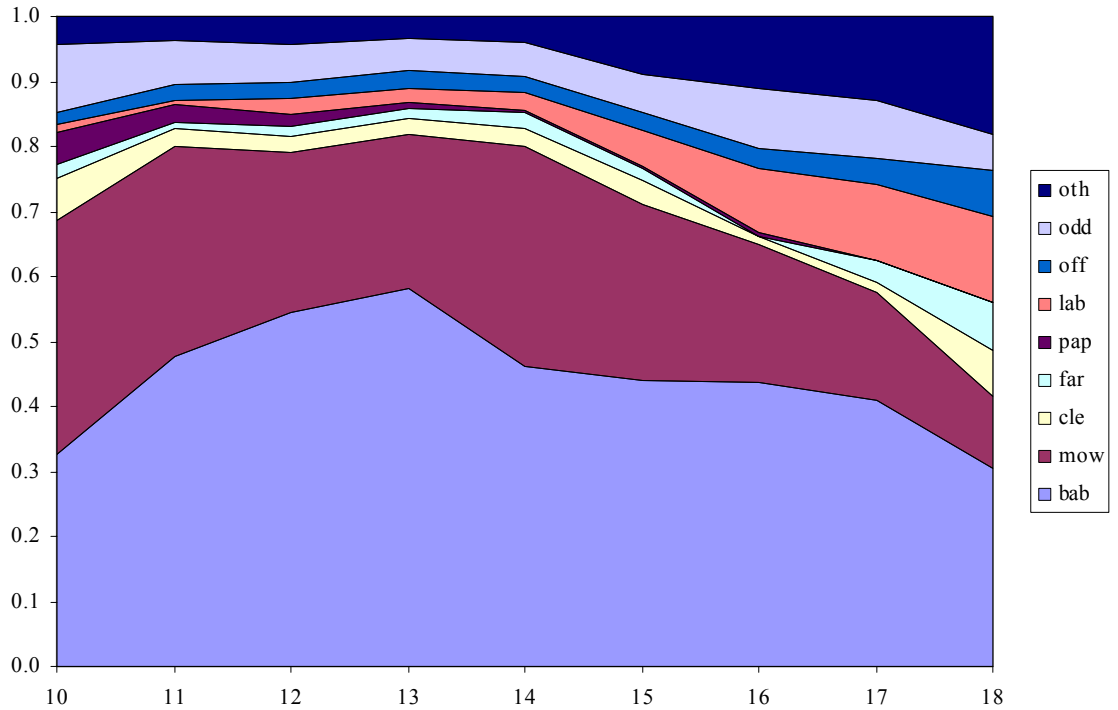
FIGURE 12
Share of All First Formal Jobs in Specific Occupations, by Gender and Race



Note: Estimates are weighted. The figures represent the share of all first formal jobs in a specific occupation. Job definitions include: “pro” = professional and managerial; “csh” = cashier; “sal” = sales; “adm” = administrative support; “foo” = food preparation and service; “ser” = other service; “rep” = repair, precision production, and craft; “hel” = construction helper; “bag” = stock handler or bagger; “lab” = other laborer. In order to conserve space, the “other” category is omitted from this graph.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

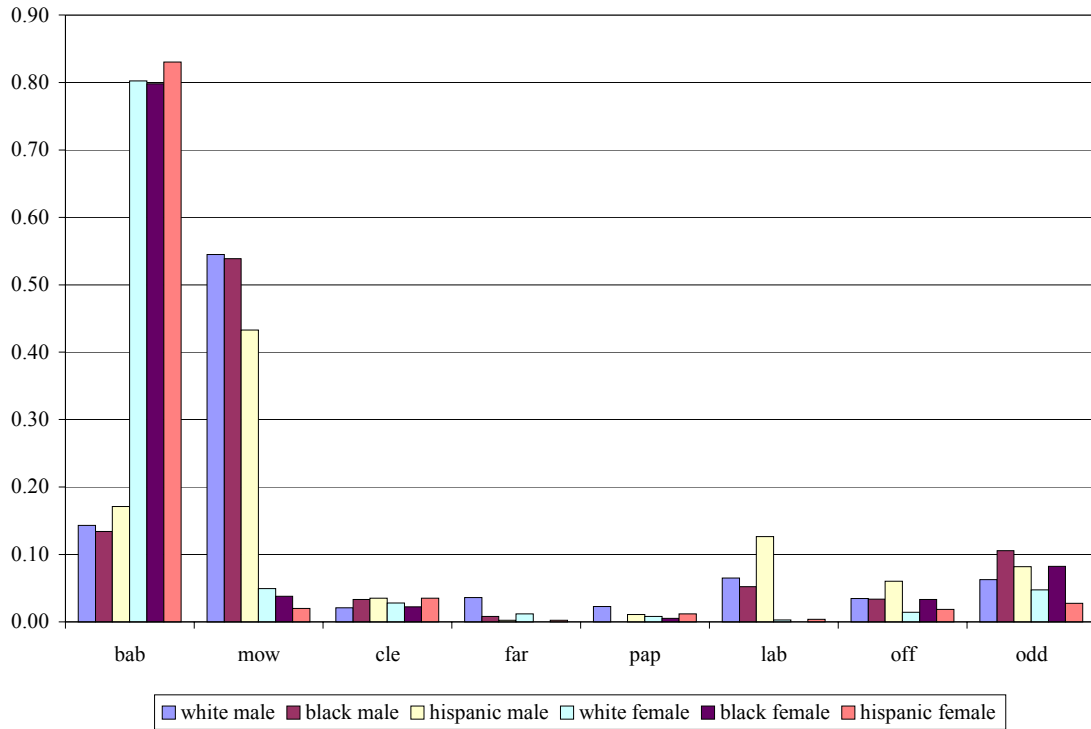
FIGURE 13
Share of All First Informal Jobs in Specific Occupations, by Age



Note: $n = 5,276$. Estimates are weighted. Respondents contribute a single observation, depending on what age they acquired their first informal job. The figures represent the share of all first informal jobs at a given age in a specific occupation. Job definitions include: “bab” = babysitting; “mow” = yard work; “cle” = cleaning; “far” = farm work; “pap” = paper route; “lab” = manual labor; “off” = office and sales; “odd” = odd jobs; “oth” = other occupation not elsewhere classified.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

FIGURE 14
Share of All First Informal Jobs in Specific Occupations, by Gender and Race

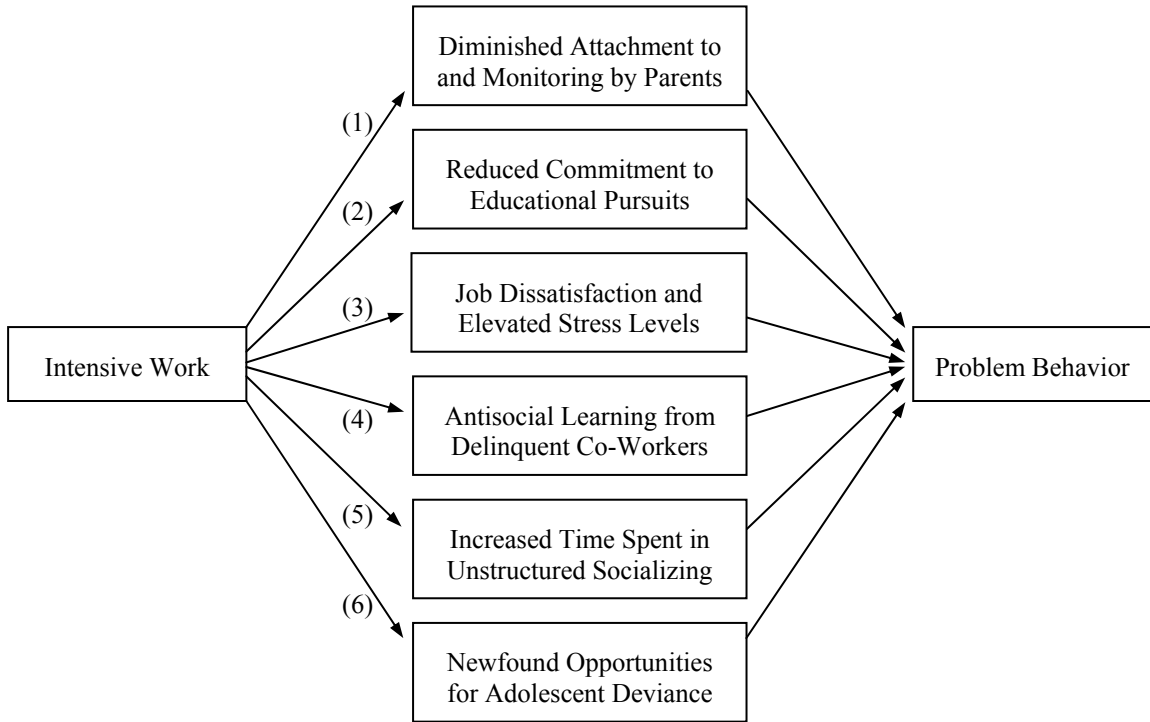


Note: Estimates are weighted. The figures represent the share of all first informal jobs in a specific occupation. Job definitions include: “bab” = babysitting; “mow” = yard work; “cle” = cleaning; “far” = farm work; “pap” = paper route; “lab” = manual labor; “off” = office and sales; “odd” = odd jobs. In order to conserve space, the “other” category is omitted from this graph.

Source: National Longitudinal Survey of Youth 1997, rounds 1-5.

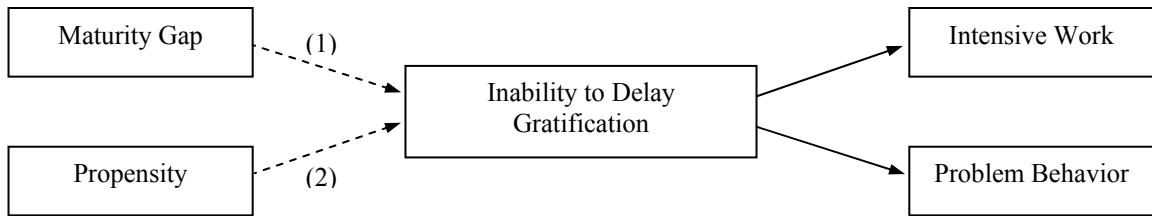
FIGURE 15

Diagram of Potential Mediating Causal Mechanisms to Explain the Positive Correlation between Intensive Work and Problem Behavior during Adolescence



Note: Distinct causal pathways are denoted numerically: (1) social control theory, (2) social control theory, (3) general strain theory, (4) differential association and social learning theories, (5) routine activity theory, and (6) opportunity theory.

FIGURE 16
Diagram of a Spurious Positive Correlation between Intensive Work and Problem Behavior during Adolescence



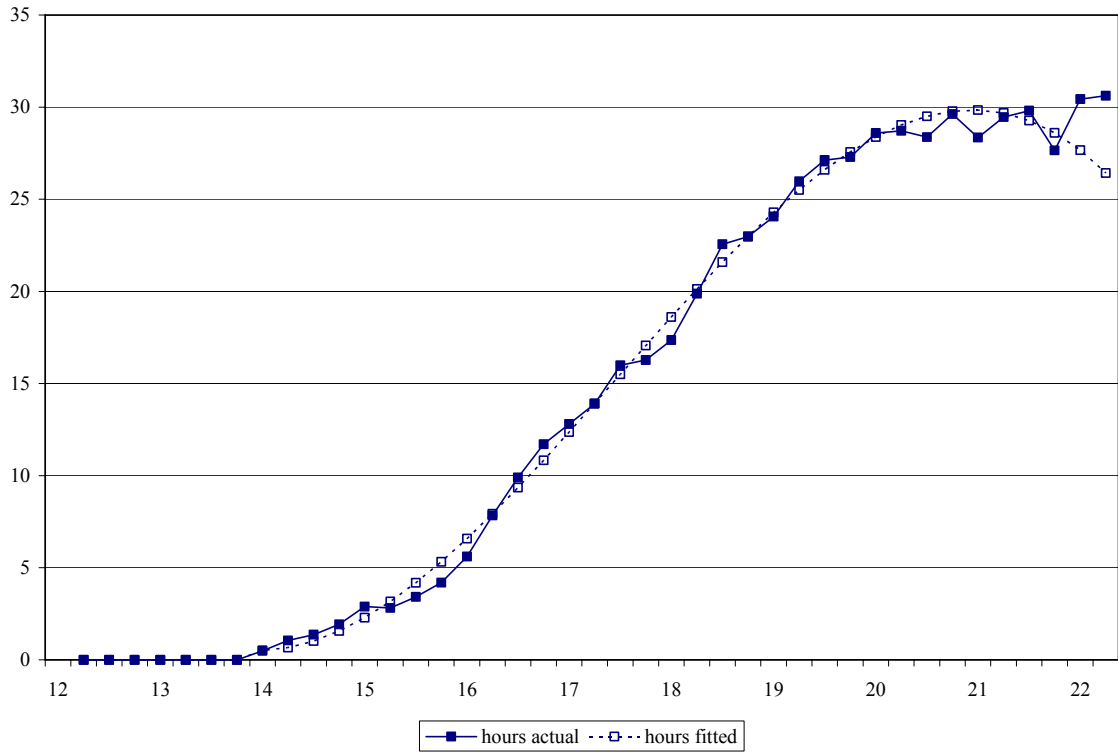
Note: The solid lines represent causal pathways. The dashed lines represent the mechanisms underlying the inability to delay gratification, denoted numerically: (1) precocious development theory, and (2) propensity theory.

FIGURE 17
 Problem Behavior in the NLSY Estimation Sample, by Age



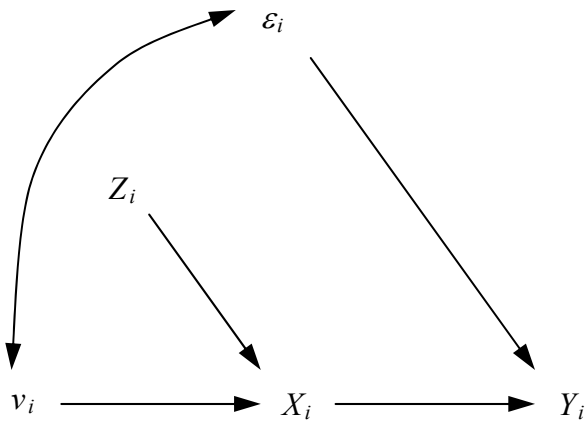
Note: Estimates are weighted. Figures are adjusted for exposure time. “Fitted” estimates are computed assuming a third-order polynomial in age.

FIGURE 18
School-Year Work Intensity in the NLSY Estimation Sample, by Age



Note: Estimates are weighted. Figures are adjusted for exposure time. “Fitted” estimates are computed assuming a third-order polynomial in age.

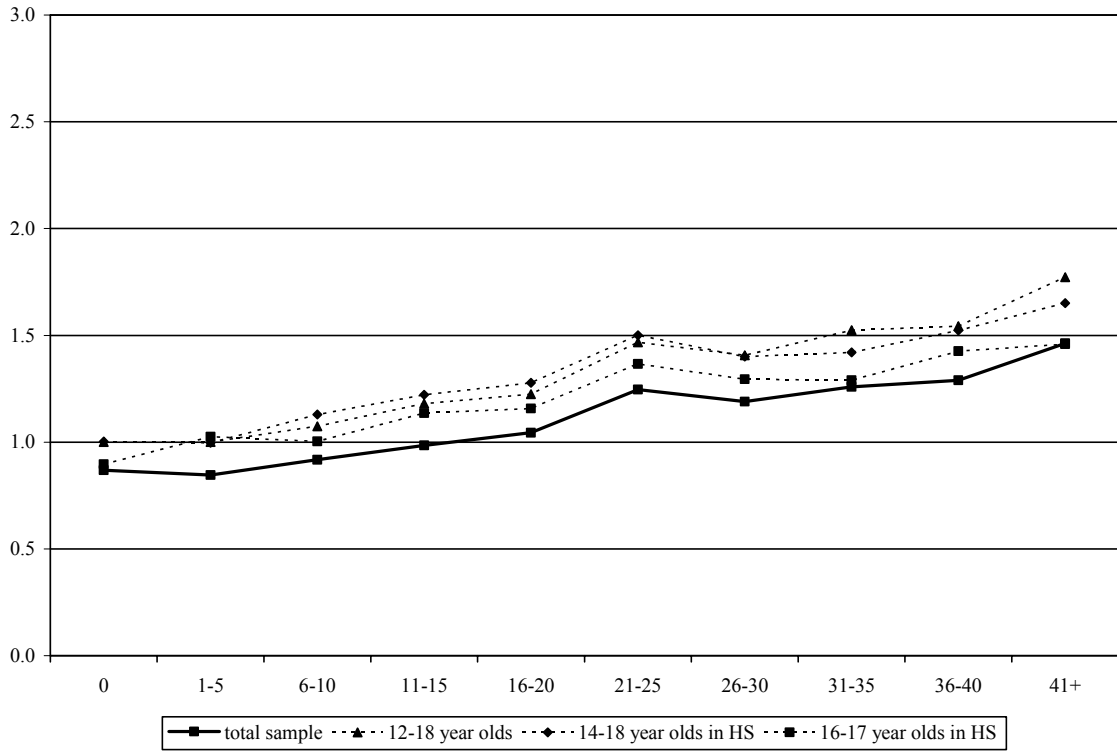
FIGURE 19
Diagram of the Logic of Instrumental Variables Estimation



Note: In the context of the relationship between employment and adolescent problem behavior, Y_i represents problem behavior, X_i represents work intensity, and Z_i represents the instrumental variable(s). Both ε_i and v_i are residuals from their respective paths, Y_i and X_i .

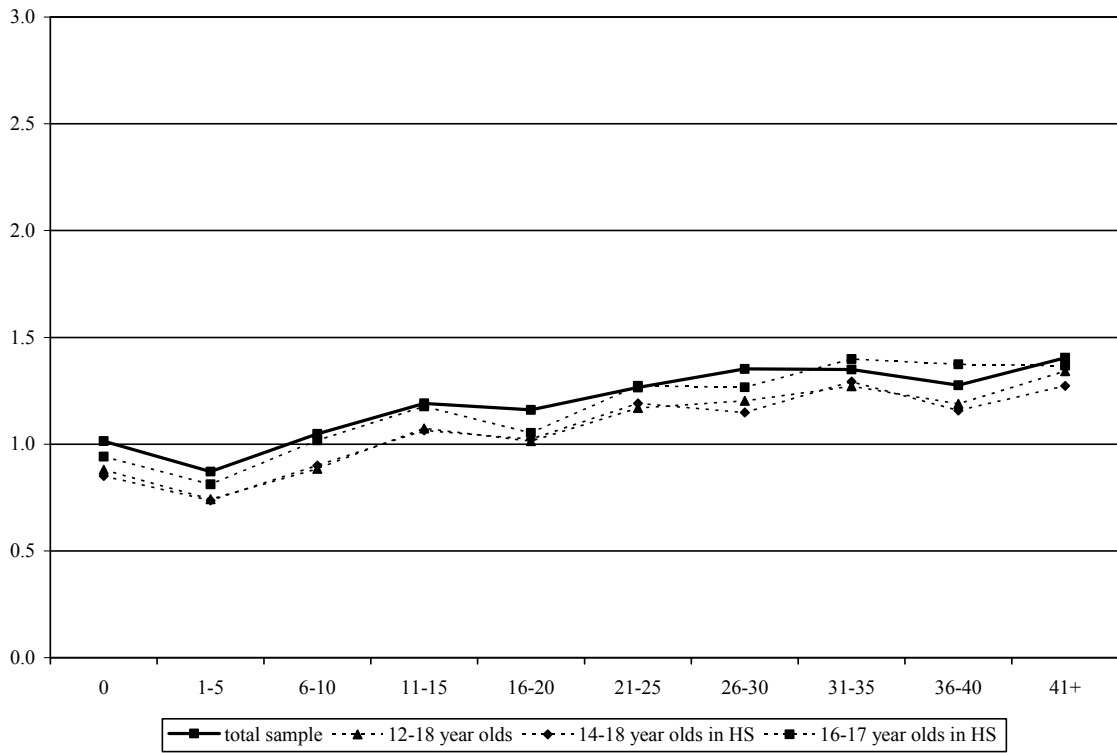
Source: Adapted from Winship and Morgan (1999:681, Figure 4).

FIGURE 20
Annual Crime (Variety) among NLSY Youths, by School-Year Work Hours



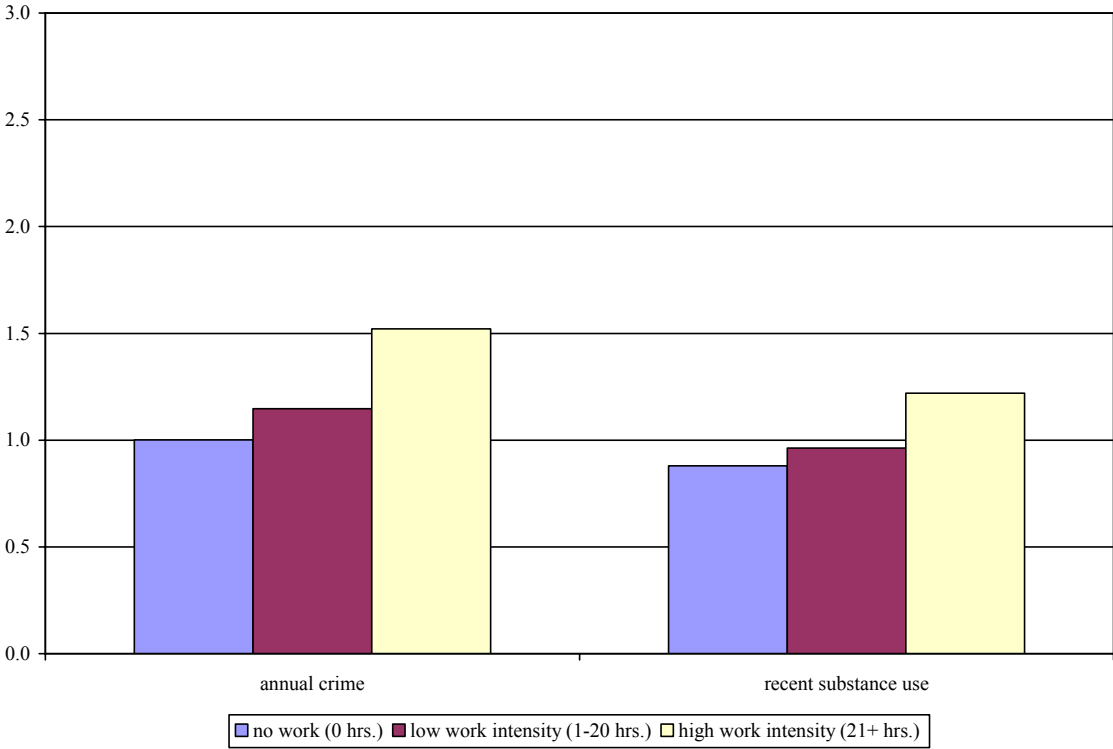
Note: Estimates are weighted. Figures are adjusted for age (linear, quadratic, cubic) and exposure time. The reference period is since the last interview (or ever prior to the initial interview).

FIGURE 21
 Recent Substance Use (Variety) among NLSY Youths, by School-Year Work Hours



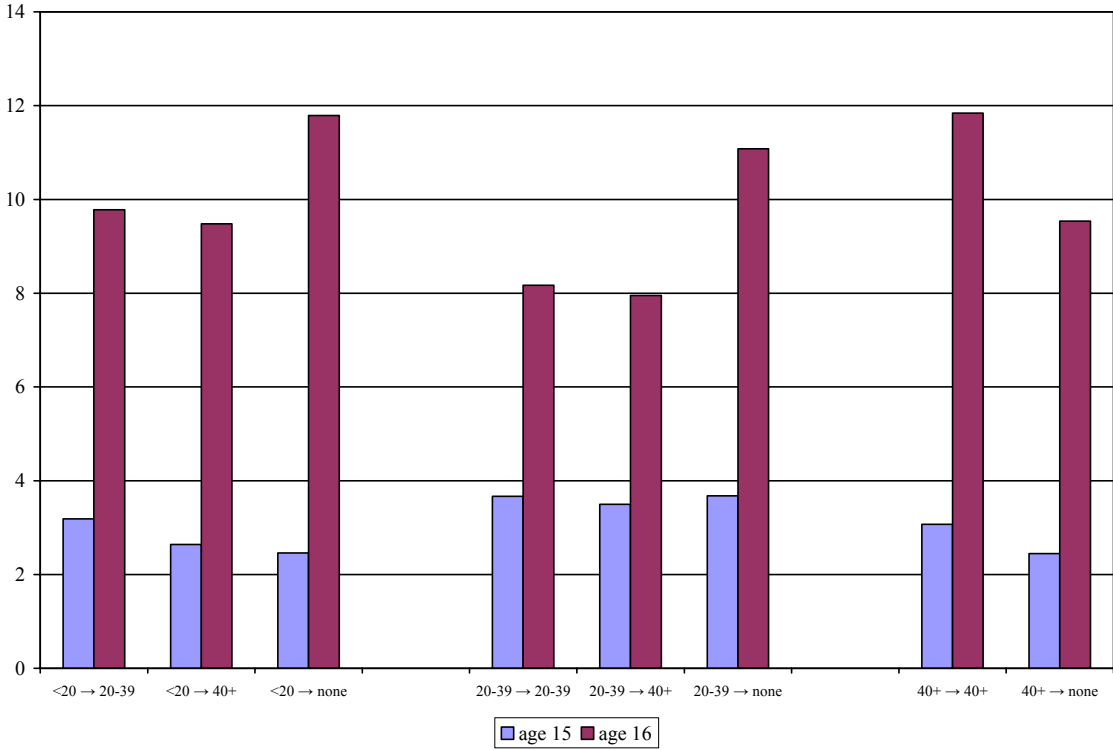
Note: Estimates are weighted. Figures are adjusted for age (linear, quadratic, cubic) and exposure time. The reference period is during the 30 days prior to the interview.

FIGURE 22
Problem Behavior among NLSY Youths, by Categorical Work Intensity



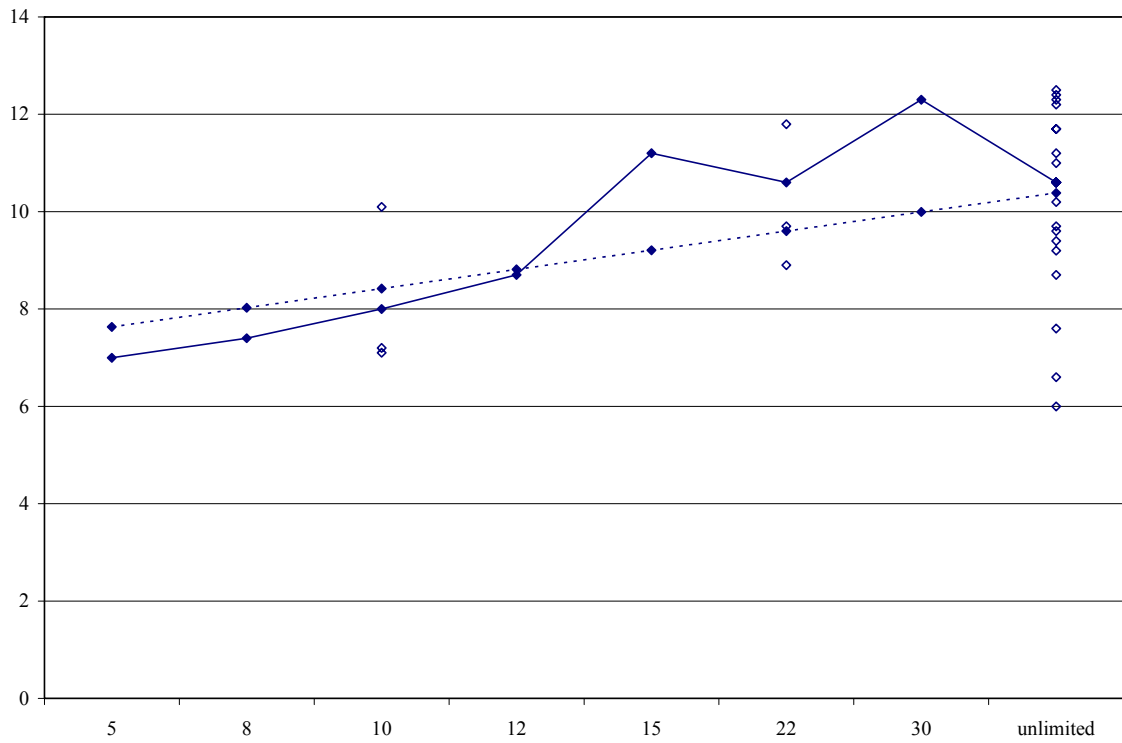
Note: Estimates are weighted. Figures are estimated using the total sample, and are adjusted for age (linear, quadratic, cubic) and exposure time. The reference period for annual crime is since the last interview (or ever prior to the initial interview), while the reference period for recent substance use is the 30 days prior to the interview.

FIGURE 23
 Mean School-Year Work Hours by Change in Weekly Hours Restrictions from 15 to 16
 Years of Age



Note: Estimates are weighted. The reference period for work hours is since the last interview (or in the year prior to the first interview). Figures are estimated using those person-year observations in which youths are 15 years old at one wave, and 16 years old the next ($NT = 3,354$).

FIGURE 24
 Mean Change in School-Year Work Hours by Change in Weekly Hours Restrictions from
 15 to 16 Years of Age



Note: Estimates are weighted. The solid line connects the actual means, the dashed line connects the fitted means, and individual states are denoted by an open diamond. Change in annual school-year work hours is the response variable, but the graph is almost identical when using change in recent school-year work hours.

APPENDIX A
Variable Definitions

Variable	Definition
Problem Behavior	
Annual Crime	Sum of 22 binary indicators for delinquent/criminal behavior since the last interview (or ever prior to the initial interview): (1) Vandalism, (2) petty theft (under 50 dollars), (3) petty shoplifting, (4) petty larceny, (5) petty burglary, (6) petty robbery, (7) major theft (over 50 dollars), (8) major shoplifting, (9) major larceny, (10) major burglary, (11) major robbery, (12) vehicle theft, (13) “other” property crime (e.g., receiving, possessing, selling stolen property), (14) income from stolen property, (15) income from “other” property crime, (16) aggravated assault (i.e., assault with the intention of inflicting serious harm), (17) selling drugs, (18) selling marijuana, (19) selling hard drugs (heroin, cocaine, LSD, other drugs), (20) income from selling drugs, (21) carrying a handgun, (22) arrest for a delinquent or criminal offense (not including arrest for minor traffic violations).
Recent Substance Use	Sum of four binary indicators for substance use in the last 30 days: (1) Smoked cigarettes, (2) had one or more drinks of alcohol (e.g., can or bottle of beer, glass of wine, mixed drink, shot of liquor), (3) smoked marijuana, (4) drank five or more drinks in a single setting.
Formal Work Hours during the School Year	
Annual School-Year Work Hours	Mean number of hours worked per week in a formal job during the school year (September 1 to May 31) since the previous interview (or since age 14 at the initial interview).
Recent School-Year Work Hours	Mean number of hours worked per week in a formal job during the school year (September 1 to May 31) during the four weeks prior to the interview week.
Individual Control Variables	
Age	Youth’s age in years (continuous) as of interview.
Household Size	Number of people that currently live in household.
Educational Attainment	
Enrolled in High School (ref.)	=1 if youth is currently enrolled in high school.
High-School Dropout or GED	=1 if youth is not enrolled in school and has no diploma or GED, or is not enrolled in school but has GED.
High-School Graduate	=1 if youth is not enrolled in school but has a high-school diploma or college degree, or is currently enrolled in a college or graduate program.
Family Structure	

Two Parents (ref.)	=1 if youth lives with both biological parents, or with a biological and step-parent.
Single Mother	=1 if youth lives only with biological mother.
No Mother Figure	=1 if youth lives only with biological father or with other family member (e.g., adoptive or foster parents, other relative).
Live Independently	=1 if youth does not live with a family member.
Residential Location	
Suburban Area (ref.)	=1 if youth lives in MSA not in the central city.
Central City	=1 if youth lives in MSA in central city.
Rural Area	=1 if youth does not live in MSA.
County Control Variables	
Local Unemployment Rate	Unemployment rate (continuous) for local labor market.
Per Capita Personal Income	Personal income per capita in 1997 dollars (in thousands).
Total Number of Jobs	Total number of full-time and part-time jobs (in thousands).
Manufacturing Jobs	Total number of manufacturing jobs (in thousands).
Retail Jobs	Total number of retail jobs (in thousands).
Service Jobs	Total number of service jobs (in thousands).
Government Jobs	Total number of government jobs (in thousands).
Unemployment Insurance	Total value of payments issued under state-administered unemployment insurance programs in 1997 dollars (in thousands).
Medicaid, AFDC, & Food Stamps	Total value of transfer payments issued for public assistance medical care, Aid to Families with Dependent Children, and food stamps, in 1997 dollars (in thousands).

Note: "(ref.)" denotes the dummy variable reference category.

APPENDIX B1

Maximum Hours and Days per Week Permitted in Non-Agricultural Employment under State Child Labor Laws, as of January 1, 1997

State	Youths 14-15			Youths 16-17		
	Daily Hours	Weekly Hours	Days Per Week	Daily Hours	Weekly Hours	Days Per Week
Alabama	8 (3)	40 (18)	6	--	--	--
Alaska	(9) ^a	(23)	6	--	--	6
Arizona	8 (3)	40 (18)	--	--	--	--
Arkansas	8	48	6	10	54	6
California	8 (3)	40 (18)	6	8 (4)	48 (28)	6
Colorado	8 (6)	40	--	8	40	--
Connecticut*	8 (3)	40 (18)	6	8/9 ^b	48	6
Delaware	8 (4)	40 (18)	6	12 ^a	--	--
District of Columbia	8	48	6	8	48	6
Florida	8 (3)	40 (15)	6	(8)	(30)	(6)
Georgia	8 (4)	40	--	--	--	--
Hawaii	8 (10) ^a	40	6	--	--	--
Idaho	9	54	--	--	--	--
Illinois	8 (8) ^a	48 (24)	6	--	--	--
Indiana	8 (3)	40 (18)	--	8/9 ^c (8)	30/48 ^c (30/40) ^c	6
Iowa	8 (4)	40 (28)	--	--	--	--
Kansas	8	40	--	--	--	--
Kentucky*	8 (3)	40 (18)	--	8 (6)	40	(6)
Louisiana	8 (3)	40 (18)	6	--	--	--
Maine	8 (3)	40 (18)	6	10 (4)	50 (20)	6
Maryland	8 (4)	40 (23)	--	12 ^a	--	--
Massachusetts	8	48	6	9	48	6
Michigan	10	48 (48) ^a	6	10	48 (48) ^a	6
Minnesota	8	40	--	--	--	--
Mississippi	8	44	--	--	--	--
Missouri	8 (3)	40	6	--	--	--
Montana	8 (3)	40 (18/23) ^c	--	--	--	--
Nebraska	8	48	--	--	--	--
Nevada	8	48	--	--	--	--
New Hampshire	8 (3)	48 (23)	--	--	48 (30)	6 (6)
New Jersey	8 (3)	40 (18)	6	8	40	6
New Mexico*	--	--	--	--	--	--
New York	8 (3)	40 (18/23) ^c	6	8 (4)	48 (28)	6
North Carolina	8 (3)	40 (18/23) ^c	--	--	--	--
North Dakota	8 (3)	40 (18)	6	8	48	6
Ohio	8 (3)	40 (18)	--	--	--	--
Oklahoma	8 (3)	40 (18)	--	--	--	--
Oregon	8 (3)	40 (18/23) ^c	--	--	44	--
Pennsylvania	8 (4)	44 (18)	6	8	44 (28)	6
Rhode Island	8	40	--	(9)	(48)	--
South Carolina	8 (3)	40 (18)	--	--	--	--
South Dakota	8 (4)	40 (20)	--	--	--	--
Tennessee	8 (3)	40 (18)	--	--	--	--
Texas	8	48	--	--	--	--
Utah	8 (4)	40	--	--	--	--
Vermont*	8	40	6	9	50	--
Virginia	8 (3)	40 (18)	--	--	--	--
Washington	8 (3)	40 (16)	6	8 (4/6) ^c	48 (20/28) ^c	6
West Virginia*	8	40	6	--	--	--
Wisconsin	8 (4)	40 (18)	6	8 (5)	50 (26)	6
Wyoming	8	56	--	--	--	--

Note: * = child labor law was amended after 1997 (CT, KY, NM, VT, WV); ^a = hours of combined school and work (AK, DE, HI, IL, MD, MI); ^b = different hours allowed in different occupations (CT); ^c = less restrictive hours allowed with written parental permission or other approved exception (IN, MT, NY, NC, OR). Figures in parentheses represent maximum work commitment allowed during school days/weeks.

Source: *Monthly Labor Review* (Nelson, 1997-2003); National Research Council (1998:194-211); U.S. Department of Labor (online at <http://youthrules.dol.gov/states.htm>).

APPENDIX B2
Time-of-Work Restrictions in Non-Agricultural Employment under State Child Labor
Laws, as of January 1, 1997

State	Youths 14-15	Youths 16-17
Alabama	7:00am-9:00pm (7:00am-7:00pm)	(5:00am-10:00pm) ^a
Alaska	5:00am-9:00pm	
Arizona	6:00am-11:00pm (6:00am-9:30pm)	
Arkansas	6:00am-9:00pm (6:00am-7:00pm)	(6:00am-11:00pm)
California	7:00am-9:00pm (7:00am-7:00pm)	5:00am-12:30pm (5:00am-10:00pm)
Colorado	(5:00am-9:30pm)	
Connecticut*	7:00am-9:00pm	6:00am-11/10pm ^b
Delaware	7:00am-9:00pm (7:00am-7:00pm)	
District of Columbia	7:00am-9:00pm (7:00am-7:00pm)	6:00am-10:00pm
Florida	7:00am-9:00pm (7:00am-7:00pm)	(6:30am-11:00pm)
Georgia	6:00am-9:00pm	
Hawaii	6:00am-9:00pm (7:00am-7:00pm)	
Idaho	6:00am-9:00pm	
Illinois	7:00am-9:00pm (7:00am-7:00pm)	
Indiana	7:00am-9:00pm (7:00am-7:00pm)	6am-12am/10pm ^c (6:00am-11:30/10pm) ^d
Iowa	7:00am-9:00pm (7:00am-7:00pm)	
Kansas	(7:00am-10:00pm)	
Kentucky*	7:00am-9:00pm (7:00am-7:00pm)	6:00am-1:00am (6:30am-11:30pm)
Louisiana	7:00am-9:00pm (7:00am-7:00pm)	
Maine	7:00am-9:00pm (7:00am-7:00pm)	5:00am-12:00am (7:00am-10:00pm)
Maryland	7:00am-9:00pm (7:00am-8:00pm)	
Massachusetts	6:30am-9:00pm (6:30am-7:00pm)	6:00am-12:00am (6:00am-10:00pm)
Michigan	7:00am-9:00pm	6:00am-11:30pm (6:00am-10:30pm)
Minnesota	7:00am-9:00pm	(4:30/5am-11:30/11pm) ^e
Mississippi	6:00am-7:00pm	
Missouri	7:00am-9:00pm (7:00am-7:00pm)	
Montana	7:00am-9:00pm (7:00am-7:00pm)	
Nebraska	6:00am-10:00pm	
Nevada		
New Hampshire	7:00am-9:00pm	
New Jersey	7:00am-9:00pm (7:00am-7:00pm)	(6:00am-11:00pm)
New Mexico*		
New York	7:00am-9:00pm (7:00am-7:00pm)	6:00am-12:00am (6:00am-12am/10pm) ^c
North Carolina	7:00am-9:00pm (7:00am-7:00pm)	(5:00am-11:00pm) ^f
North Dakota	7:00am-9:00pm (7:00am-7:00pm)	
Ohio	7:00am-9:00pm (7:00am-7:00pm)	(7:00am-11:00pm)
Oklahoma	7:00am-9:00pm (7:00am-7:00pm)	
Oregon	7:00am-9:00pm (7:00am-7:00pm)	
Pennsylvania	7:00am-10:00pm (7:00am-7:00pm)	6:00am-1:00am (6:00am-12:00am)
Rhode Island	6:00am-9:00pm (6:00am-7:00pm)	6:00am-1:30am (6:00am-11:30pm)
South Carolina	7:00am-9:00pm (7:00am-7:00pm)	
South Dakota	(10:00pm) ^g	
Tennessee	6:00am-9:00pm (7:00am-7:00pm)	(6:00am-12am/10pm) ^e
Texas	5:00am-12:00am (5:00am-10:00pm)	
Utah	(5:00am-9:30pm)	
Vermont*	6:00am-7:00pm (7:00am-7:00pm)	
Virginia	7:00am-9:00pm (7:00am-7:00pm)	
Washington	7:00am-9:00pm (7:00am-7:00pm)	5:00am-12:00am (7:00am-10:00pm)
West Virginia*	5:00am-8:00pm (7:00am-7:00pm)	
Wisconsin	7:00am-11:00pm (7:00am-8:00pm)	5:00am-12:30am (7:00am-11:00pm)
Wyoming	5:00am-12:00am (5:00am-10:00pm)	5:00am-12:00am ^h

Note: * = child labor law was amended after 1997 (CT, KY, NM, VT, WV); ^a = applies to all youths until age 19 as of 2000 (AL); ^b = different times apply to different occupations (CT); ^c = applicable only to 16 year olds, with no restriction for 17 year olds (IN); ^d = 17 year olds allowed to work until later time, whereas 16 year olds must abide by the more restrictive time (IN); ^e = less restrictive times allowed with written parental permission (MN, NY, TN); ^f = no restriction with written parental approval (NC); ^g = restriction after 10:00 p.m. on school night, with no morning restriction (SD); ^h = applicable to females only (WY). Times denote the range of hours in which employment is permitted. Figures in parentheses represent time-of-work restrictions during school days/weeks.

Source: *Monthly Labor Review* (Nelson, 1997-2003); National Research Council (1998:194-211); U.S. Department of Labor (online at <http://youthrules.dol.gov/states.htm>).

APPENDIX B3
Amendments to State Child Labor Laws after 1997

<u>Maximum Hours and Days Per Week Restrictions</u>						
State (Year of Change)	Youths 14-15			Youths 16-17		
	Daily Hours	Weekly Hours	Days Per Week	Daily Hours	Weekly Hours	Days Per Week
Connecticut (1998)	8 (3)	40 (18)	6	8/9 ^a (6)	48 (32)	6
Kentucky (2002)	8 (3)	40 (18)	--	8 (6)	40 (30/40) ^b	(6)
New Mexico (1998)	--	40 (18)	--	--	--	--
Vermont (2001)	8 (3)	40 (18)	6		No restrictions	
West Virginia (2002)	8 (3)	40 (18)	--	--	--	--

<u>Time-of-Work Restrictions</u>				
State (Year of Change)	Youths 14-15		Youths 16-17	
	Daily Hours	Days Per Week	Daily Hours	Days Per Week
Connecticut (1998)	7:00am–9:00pm	(7:00am–7:00pm)	6am–12am/11pm ^a	(6:00am–11/10pm) ^a
Kentucky (2002)	7:00am–9:00pm	(7:00am–7:00pm)	6:00am–1:00am	(6:30am–10:30pm)
New Mexico (1998)	7:00am–9:00pm	(7:00am–7:00pm)		
Vermont (2001)	7:00am–9:00pm	(7:00am–7:00pm)		
West Virginia (2002)	7:00am–9:00pm	(7:00am–7:00pm)		

Note: ^a = different hours allowed in different occupations (CT); ^b = less restrictive hours allowed with written parental permission. Shaded figures highlight the amendments that were introduced.

Source: *Monthly Labor Review* (Nelson, 1997-2003); National Research Council (1998:194-211); U.S. Department of Labor (online at <http://youthrules.dol.gov/states.htm>).

APPENDIX B4
Child Labor Laws for Work Hours in a Formal Job, by State, as of January 1, 1997

Child Labor Law (from Table 4)						
State	Under 20 Hrs. (14-15)	20-39 Hrs. (14-15)	40+ Hrs. (14-15)	20-39 Hrs. (16-17)	40+ Hrs. (16-17)	# Laws
Alabama	+					1
Alaska		+				1
Arizona	+					1
Arkansas			+		+	2
California	+			+		2
Colorado			+		+	2
Connecticut	+				+	2
Delaware	+					1
District of Columbia			+		+	2
Florida	+			+		2
Georgia			+			1
Hawaii			+			1
Idaho			+			1
Illinois		+				1
Indiana	+				+	2
Iowa		+				1
Kansas			+			1
Kentucky	+				+	2
Louisiana	+					1
Maine	+			+		2
Maryland		+				1
Massachusetts			+		+	2
Michigan	+			+		2
Minnesota			+			1
Mississippi			+			1
Missouri			+			1
Montana		+				1
Nebraska			+			1
Nevada			+			1
New Hampshire		+		+		2
New Jersey	+				+	2
New Mexico						0
New York		+		+		2
North Carolina		+				1
North Dakota	+				+	2
Ohio	+					1
Oklahoma	+					1
Oregon		+			+	2
Pennsylvania	+			+		2
Rhode Island			+		+	2
South Carolina	+					1
South Dakota		+				1
Tennessee	+					1
Texas			+			1
Utah			+			1
Vermont			+		+	2
Virginia	+					1
Washington	+			+		2
West Virginia			+			1
Wisconsin	+			+		2
Wyoming			+			1

Note: "+" indicates the law is in effect. Measures of centrality for # of laws: mode = 1; median = 1; mean = 1.392.

APPENDIX C
Restrictions to Obtain NLSY Estimation Sample

	<i>N</i>	<i>NT</i>
Initial sample	8,984	44,920
Non-attrited sample	8,984	41,543
Case-wise deletions		
17 years of age or younger at first interview	8,980	41,522
At least two complete interviews	8,765	41,307
Cell-wise deletions		
Valid data for problem behavior measures	8,764	40,652
Valid data for work intensity	8,764	40,550
Valid data on state of residence	8,764	40,323
Final case-wise deletions		
At least two complete interviews	8,724	40,283
% of initial sample	97.1	89.7
% of non-attrited sample	97.1	97.0

Note: Percentages are unweighted.

APPENDIX D

Interview Sequence of NLSY Estimation Sample ($N = 8,724$, $NT = 40,283$)

Interview Sequence	N	% of N	NT	% of NT
Five Complete Interviews				
11111	6,554	75.1	32,770	81.3
Four Complete Interviews				
11110	472	5.4	1,888	4.7
11101	276	3.2	1,104	2.7
11011	232	2.7	928	2.3
10111	243	2.8	972	2.4
01111	87	1.0	348	0.9
Subtotal	1,310	15.1	5,240	13.0
Three Complete Interviews				
11100	216	2.5	648	1.6
11010	65	0.7	195	0.5
11001	76	0.9	228	0.6
10101	30	0.3	90	0.2
10011	87	1.0	261	0.6
10110	41	0.5	123	0.3
01011	9	0.1	27	0.1
00111	11	0.1	33	0.1
01110	8	0.1	24	0.1
01101	10	0.1	30	0.1
Subtotal	553	6.3	1,659	4.1
Two Complete Interviews				
11000	177	2.0	354	0.9
10100	44	0.5	88	0.2
10010	28	0.3	56	0.1
10001	46	0.5	92	0.2
01001	2	0.0	4	0.0
00011	4	0.0	8	0.0
01100	4	0.0	8	0.0
01010	2	0.0	4	0.0
Subtotal	307	3.5	614	1.5

Note: "1" denotes that the respondent completed the interview; "0" denotes that the respondent did not complete the interview. Percentages are unweighted.

APPENDIX E1
Prospective Predictors of Work Status and Work Intensity during the 14-16 Age Span

1997 Predictors	Employed 14-16?		Mean Work Intensity			
	No (N = 794)	Yes (N = 2,062)	1-10 Hrs. (N = 463)	11-20 Hrs. (N = 823)	21-30 Hrs. (N = 479)	31+ Hrs. (N = 297)
Demographic Variables						
Male ^{a,b}	47.7%	52.9%	48.4%	50.5%	56.4%	62.3%
Race/Ethnicity:						
White ^{a,b}	62.0%	73.4%	78.0%	76.4%	67.7%	65.5%
Black ^{a,b}	22.2%	13.4%	10.3%	12.1%	16.2%	18.0%
Hispanic ^{a,b}	15.0%	11.8%	9.9%	10.6%	14.6%	14.2%
Age at First Interview ^b	13.13 (0.47)	13.14 (0.50)	13.02 (0.51)	13.15 (0.52)	13.17 (0.48)	13.30 (0.44)
Residential Characteristics						
Housing Type:						
House, Condo, or Farm ^{a,b}	78.6%	83.1%	88.8%	83.6%	78.1%	79.5%
Apartment ^{a,b}	13.1%	9.4%	5.7%	9.6%	11.4%	11.9%
Other Housing ^b	8.3%	7.5%	5.5%	6.8%	10.4%	8.6%
Residential Mobility	0.88 (0.47)	0.88 (0.61)	0.89 (0.49)	0.90 (0.65)	0.88 (0.63)	0.82 (0.65)
Residential Location:						
Suburbs ^b	53.7%	53.4%	59.2%	56.2%	45.5%	47.8%
Central City	27.7%	26.0%	23.5%	25.9%	29.0%	26.1%
Rural ^b	18.6%	20.5%	17.3%	17.9%	25.5%	26.1%
Census Region:						
Northeast ^{a,b}	14.2%	19.3%	19.0%	21.4%	19.5%	12.6%
Midwest ^a	19.4%	26.8%	25.4%	28.6%	26.7%	24.0%
South ^{a,b}	42.8%	32.5%	28.8%	29.8%	35.5%	42.6%
West ^b	23.6%	21.4%	26.7%	20.2%	18.3%	20.9%
Family Economic Background						
Mother Figure Employed ^a	64.4%	75.4%	77.7%	75.3%	74.7%	72.1%
Father Figure Employed ^{a,b}	87.5%	91.8%	95.8%	91.9%	90.4%	85.4%
Mother Figure Education:						
Less than High School ^{a,b}	19.7%	15.6%	12.6%	13.0%	19.9%	23.1%
High School Completion	36.4%	34.4%	30.7%	36.0%	34.8%	35.6%
Some College ^a	36.0%	40.3%	43.5%	40.4%	38.2%	37.6%
Some Graduate ^b	7.9%	9.6%	13.3%	10.6%	7.1%	3.7%
Father Figure Education:						
Less than High School ^{a,b}	22.9%	14.6%	10.6%	12.7%	20.4%	19.6%
High School Completion ^b	32.0%	34.1%	28.1%	35.2%	37.6%	37.6%
Some College ^{a,b}	32.8%	38.6%	43.9%	39.5%	33.6%	32.5%
Some Graduate ^b	12.4%	12.7%	17.4%	12.6%	8.3%	10.4%
Household Income ^{a,b}	46,401.56 (43,842.31)	52,153.97 (45,388.06)	58,014.43 (46,033.66)	53,619.19 (46,833.23)	46,420.74 (39,657.42)	46,109.14 (47,163.60)
Low-Income Household ^{a,b}	31.2%	22.0%	16.1%	19.6%	26.7%	32.6%
Received Government Aid ^b	47.9%	46.7%	36.8%	45.4%	52.7%	59.1%
Participated in Head Start ^{a,b}	17.6%	13.9%	9.4%	12.5%	19.0%	18.3%
No Medical Insurance ^{a,b}	12.3%	9.5%	7.9%	7.9%	10.1%	16.4%
Number of Family Assets ^{a,b}	2.51 (1.62)	2.91 (1.63)	3.19 (1.70)	3.01 (1.62)	2.67 (1.58)	2.50 (1.52)
Early First Birth ^{a,b}	27.1%	21.8%	16.9%	19.2%	29.3%	26.7%
Foreign-Born Parent(s) ^a	13.9%	10.0%	9.8%	9.7%	9.7%	11.7%
Family Composition						
Household Size ^a	4.61 (1.45)	4.46 (1.36)	4.47 (1.39)	4.44 (1.35)	4.48 (1.38)	4.44 (1.27)
Family Structure:						
Both Biological Parents ^b	53.3%	54.6%	63.8%	55.5%	47.1%	47.9%
Step Parent	12.7%	14.8%	13.5%	14.8%	17.2%	12.9%
Biological Mother Only ^b	25.4%	24.0%	17.4%	24.1%	28.1%	28.3%
Biological Father Only	3.2%	3.2%	2.7%	3.1%	3.6%	4.1%
Other Arrangement ^{a,b}	5.0%	3.1%	2.4%	2.4%	3.3%	6.3%
Deceased Parent(s) ^a	6.5%	3.4%	2.4%	3.7%	3.8%	4.1%

Relational & Instrumental Control

Confide in Parent(s) ^{a,b}	65.2%	57.7%	65.3%	54.7%	53.2%	60.8%
Attachment to Mother Figure ^b	6.40 (1.63)	6.36 (1.81)	6.58 (1.78)	6.34 (1.80)	6.23 (1.87)	6.26 (1.76)
Attachment to Father Figure ^b	6.06 (1.96)	6.17 (2.11)	6.35 (2.08)	6.14 (2.10)	5.91 (2.19)	6.30 (2.01)
Monitoring by Mother Figure ^b	2.54 (1.17)	2.55 (1.27)	2.75 (1.28)	2.48 (1.29)	2.49 (1.26)	2.51 (1.21)
Monitoring by Father Figure ^b	1.81 (1.39)	1.82 (1.52)	1.99 (1.56)	1.77 (1.55)	1.66 (1.45)	1.96 (1.43)
Decision-Making Autonomy ^{a,b}	1.69 (0.90)	1.84 (0.92)	1.80 (0.94)	1.90 (0.92)	1.83 (0.95)	1.77 (0.85)
Permissive Disciplinary Style ^{a,b}	0.84 (0.83)	0.96 (0.85)	0.81 (0.82)	1.01 (0.87)	1.05 (0.85)	0.91 (0.80)
Inductive Disciplinary Style ^{a,b}	1.93 (0.95)	1.85 (0.96)	2.00 (0.96)	1.81 (0.96)	1.73 (0.97)	1.92 (0.92)
Parental Discard	0.80 (1.48)	0.73 (1.53)	0.68 (1.50)	0.69 (1.47)	0.93 (1.70)	0.69 (1.43)

School Performance & Disengagement

PIAT Math Percentile ^{a,b}	48.18 (32.92)	57.03 (33.56)	61.76 (35.09)	58.91 (33.10)	52.72 (32.10)	49.69 (32.96)
Repeated a Grade ^{a,b}	14.7%	9.4%	7.6%	7.6%	11.9%	14.7%
Skipped a Grade	1.2%	1.1%	1.3%	1.5%	0.8%	0.0%
Suspended from School ^b	19.9%	17.9%	11.6%	15.7%	21.8%	30.2%
Number of School Fights ^b	0.37 (0.94)	0.37 (0.98)	0.25 (0.74)	0.31 (0.89)	0.47 (1.15)	0.60 (1.21)
Number of Times Late	0.95 (2.55)	1.00 (3.02)	0.91 (3.02)	0.99 (2.79)	0.93 (2.83)	1.30 (3.80)
Number of Times Absent ^b	3.70 (4.99)	3.64 (4.59)	3.55 (4.66)	3.70 (4.59)	3.32 (3.97)	4.24 (5.33)
Positive School Attitudes ^b	5.27 (1.35)	5.26 (1.44)	5.40 (1.36)	5.28 (1.44)	5.13 (1.44)	5.12 (1.57)

Religiosity & Time Use

Religious Preference:						
Catholic ^b	29.1%	28.3%	27.9%	32.4%	25.9%	20.4%
Baptist ^{a,b}	23.9%	19.0%	16.3%	16.4%	24.6%	22.3%
Protestant ^a	31.6%	36.6%	39.7%	36.2%	35.1%	34.5%
Other Religion	5.2%	5.1%	5.2%	5.0%	3.9%	7.1%
No Religious Affiliation ^b	10.1%	11.1%	10.9%	10.0%	10.5%	15.8%
Hours Spent on Homework	1.25 (1.50)	1.18 (1.82)	1.27 (2.24)	1.21 (1.64)	1.13 (2.08)	1.01 (0.78)
Hours Spent in Extracurricular Activities ^{a,b}	0.30 (0.64)	0.42 (0.86)	0.52 (1.05)	0.39 (0.81)	0.34 (0.78)	0.44 (0.79)
Hours Spent Reading for Pleasure	0.93 (2.51)	0.77 (2.32)	0.85 (2.85)	0.78 (2.39)	0.64 (1.09)	0.81 (2.64)
Hours Spent Watching T.V. ^{a,b}	2.68 (2.35)	2.48 (2.18)	2.16 (1.80)	2.51 (2.49)	2.79 (2.13)	2.45 (1.69)
Household Chores	5.62 (1.87)	5.64 (1.82)	5.65 (1.91)	5.63 (1.79)	5.66 (1.76)	5.63 (1.84)
Family Activities ^a	10.30 (4.26)	9.74 (4.41)	9.96 (4.41)	9.71 (4.51)	9.42 (4.34)	9.96 (4.24)

Peer Association & Behavioral Autonomy

Antisocial Peer Affiliation ^{a,b}	0.67 (1.09)	0.81 (1.30)	0.57 (1.13)	0.84 (1.34)	0.91 (1.38)	0.94 (1.27)
Friends in a Gang ^b	14.3%	14.9%	10.6%	16.1%	16.7%	15.5%
Unsupervised Dating ^{a,b}	25.8%	37.6%	29.1%	37.8%	44.2%	41.2%
Earned an Allowance	58.4%	59.4%	58.4%	62.2%	57.8%	54.9%
Worked in an Informal Job ^{a,b}	42.2%	56.4%	58.0%	57.8%	55.9%	49.5%

Early Problem Behavior

Ran Away from Home ^b	5.2%	6.2%	4.5%	5.5%	8.0%	8.9%
Lifetime Delinquency Variety ^{a,b}	1.11 (1.85)	1.27 (2.10)	0.86 (1.74)	1.32 (2.12)	1.36 (1.90)	1.72 (2.69)
Involved in Minor Property Crime ^{a,b}	35.6%	40.8%	30.3%	43.2%	46.6%	42.3%
Involved in Serious Property Crime	1.8%	1.5%	1.2%	1.4%	1.4%	2.3%
Involved in Income-Generating Crime ^b	6.9%	7.7%	5.6%	7.4%	7.8%	12.0%
Committed Aggravated Assault ^b	14.9%	14.6%	10.1%	13.9%	16.4%	22.4%
Carried a Handgun ^{a,b}	6.3%	8.3%	5.6%	8.3%	7.4%	15.1%
Arrested for a Delinquent Offense ^b	3.8%	3.1%	1.7%	2.7%	3.9%	5.4%
Lifetime Substance Use Variety ^{a,b}	0.44 (0.75)	0.60 (0.91)	0.39 (0.76)	0.63 (0.94)	0.71 (0.92)	0.74 (0.97)
Smoked Cigarettes ^{a,b}	18.3%	27.6%	16.7%	28.5%	34.4%	32.6%

Drank Alcohol ^{a,b}	20.5%	25.5%	18.5%	26.9%	27.6%	30.2%
Used Marijuana ^b	5.7%	7.4%	3.3%	7.6%	8.7%	11.6%
Recent Substance Use Variety ^{a,b}	0.16 (0.53)	0.24 (0.71)	0.16 (0.60)	0.26 (0.74)	0.27 (0.73)	0.29 (0.74)
Smoked Cigarettes ^{a,b}	5.5%	10.7%	7.2%	10.5%	12.6%	14.5%
Drank Alcohol	6.6%	7.4%	5.4%	8.6%	7.6%	6.9%
Binge Drank	1.9%	3.0%	1.7%	3.0%	4.1%	3.6%
Used Marijuana ^a	1.8%	3.0%	1.9%	3.6%	2.5%	4.5%

Note: ^a = difference between workers and non-workers is significant at $p < .10$ (two-tailed t -test); ^b = differences between work intensity groups are globally significant at $p < .10$ (F -test). Sample is composed of 2,856 youths 12-13 years old at the first interview with valid data on work status and work intensity. All statistics are computed using those cases with non-missing values. Standard deviations for continuous variables are in parentheses.

APPENDIX E2

Variable Definitions for Prospective Predictors of Work Status and Work Intensity

Variable	Definition
Demographic Variables	
Male	=1 if youth is male.
Race/Ethnicity:	
White	=1 if youth is non-Hispanic white.
Black	=1 if youth is non-Hispanic black.
Hispanic	=1 if youth is Hispanic.
Age at First Interview	Age in years at the first (1997) interview (continuous).
Residential Characteristics	
Housing Type:	
House, Condo, or Farm	=1 if youth lives in a house, condo, townhouse, row house, farm, or ranch.
Apartment	=1 if youth lives in an apartment or flat.
Other Housing	=1 if youth 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.
Residential Location:	
Suburbs	=1 if youth lives in MSA not in the central city.
Central City	=1 if youth lives in MSA in the central city.
Rural	=1 if youth does not live in MSA.
Census Region:	
Northeast	=1 if youth lives in Northeast region.
Midwest	=1 if youth lives in Midwest region.
South	=1 if youth lives in South region.
West	=1 if youth lives in West region.
Family Economic Background	
Mother Figure Employed	=1 if mother figure is employed.
Father Figure Employed	=1 if father figure is employed.
Mother Figure Education:	
Less than High School	=1 if mother figure did not complete high school.
High School Completion	=1 if mother figure finished the 12th grade.
Some College	=1 if mother figure completed up to four years of college.
Some Graduate	=1 if mother figure completed up to eight years of college.
Father Figure Education:	
Less than High School	=1 if father figure did not complete high school.
High School Completion	=1 if father figure finished the 12th grade.
Some College	=1 if father figure completed up to four years of college.
Some Graduate	=1 if father figure completed up to eight years of college.
Household Income	Gross household income in the previous year.
Low-Income Household	=1 if gross household income is less than \$5,000 per person.
Received Government Aid	=1 if parent(s) ever received government aid since age 18 or since oldest child was born.
Participated in Head Start	=1 if youth ever attended an official, government-sponsored Head Start program.
No Medical Insurance	=1 if youth is not covered by health insurance that includes physician or hospital care.
Number of Family Assets	Number of different assets owned by parent(s): (1) real estate property, (2) pre-paid tuition savings account, (3) savings in a pension or retirement plan, (4) checking or savings account, (5) investments in government savings bonds or certificates of deposits, (6) investments in stocks or mutual funds, (7) automobiles or boats, (8) other assets or savings.
Early First Birth	=1 if biological mother was 19 or younger when first child born.
Foreign-Born Parent(s)	=1 if parent was not born in the United States.
Family Composition	
Household Size	Number of people that currently live in household.
Family Structure:	
Both Biological Parents	=1 if youth lives with both biological parents.
Step Parent	=1 if youth lives with a biological and step-parent.
Biological Mother Only	=1 if youth lives only with biological mother.
Biological Father Only	=1 if youth lives only with biological father.
Other Arrangement	=1 if youth has some other family arrangement (e.g., adoptive or foster parents, grandparent or other relative).
Deceased Parent(s)	=1 if at least one of youth's biological parents is deceased.

Relational & Instrumental Control

Confide in Parent(s)	=1 if youth turns to parents first with emotional or relationship problems.
Attachment to Mother Figure	Sum of the number of statements about mother figure with which youth agrees or strongly agrees: (1) I think highly of her, (2) she is a person I want to be like, (3) I really enjoy spending time with her, (4) she usually praises me for doing well, (5) she usually criticizes me or my ideas (reverse coded), (6) she usually helps me do things that are important to me, (7) she usually blames me for her problems (reverse coded), (8) she usually makes plans with me and cancels for no good reason (reverse coded).
Attachment to Father Figure	Same as above, but with respect to father figure.
Monitoring by Mother Figure	Sum of the number of persons in youth's life in which mother figure knows most things: (1) my close friends, (2) my close friends' parents, (3) who I am with when I am not at home, (4) my teachers and what I am doing in school.
Monitoring by Father Figure	Same as above, but with respect to father figure.
Decision-Making Autonomy	Sum of the number of behaviors in which youth participates in setting limits either alone or jointly with parents: (1) how late youth stays out at night, (2) what kinds of T.V. shows and movies youth watches, (3) who youth can hang out with.
Permissive Disciplinary Style	Sum of the number of behavioral limits (curfew, T.V./movies, who youth hangs out with) in which parent responds to violation by (1) letting youth decide, (2) ignoring it, pretending it didn't happen, or letting youth get away with it, or (3) sulking, pouting, or giving youth the silent treatment.
Inductive Disciplinary Style	Sum of the number of behavioral limits (curfew, T.V./movies, who youth hangs out with) in which parent(s) respond to violation by (1) discussing it calmly or (2) taking away a privilege, grounding, or giving chores.
Parental Discord	Sum of the number of statements about parental interaction style in which mother usually (1) is fair and willing to compromise in a disagreement (reverse coded), (2) screams or yells when angry, (3) expresses love and affection (reverse coded), (4) insults or criticizes, (5) encourages or helps spouse do things that are important to him/her (reverse coded), (6) blames spouse for problems. Same items scored for father.

School Performance & Disengagement

PIAT Math Percentile	Math percentile on the Peabody Individual Achievement Test (PIAT).
Repeated a Grade	=1 if youth has ever repeated a grade.
Skipped a Grade	=1 if youth has ever skipped a grade.
Suspended from School	=1 if youth has ever been suspended from school.
Number of School Fights	Number of times youth got into a physical fight at school.
Number of Times Late	Number of times youth was late for school without an excuse.
Number of Times Absent	Number of times youth was absent from school.
Positive School Attitudes	Sum of the number of statements about school with which youth agrees or strongly agrees: (1) teachers are good, (2) teachers are interested in the students, (3) disruptions by other students get in the way of learning (reverse coded), (4) students are graded fairly, (5) there is a lot of cheating on tests and assignments (reverse coded), (6) discipline is fair, (7) I feel safe at school.

Religiosity & Time Use

Religious Preference:	
Catholic	=1 if youth's religious preference is Catholic.
Baptist	=1 if youth's religious preference is Baptist.
Protestant	=1 if youth's religious preference is Protestant (e.g., Methodist, Lutheran, Presbyterian, Episcopal).
Other Religion	=1 if youth's religious preference is some other category (e.g., Jewish, Mormon, Hindu/Buddhist, Wicca).
No Religious Affiliation	=1 if youth claims no religious affiliation.
Hours Spent on Homework	Number of hours per weekday that youth typically spends doing homework.
Hours Spent in Extracurricular Activities	Number of hours per weekday that youth typically spends taking extra classes or lessons (e.g., music, dance, foreign language).
Hours Spent Reading for Pleasure	Number of hours per weekday that youth typically spends reading for pleasure.
Hours Spent Watching T.V.	Number of hours per weekday that youth typically spends watching television.
Household Chores	Number of days in the week that youth gets housework done (e.g., cleaning up after dinner, doing dishes, taking out the trash).
Family Activities	Sum of the number of weekdays that youth typically: (1) eats dinner with the family, (2) does something fun with the family (e.g., play a game, go to a sporting event, go swimming), (3) does something religious with the family (e.g., go to church, pray, read the scriptures).

Peer Association & Behavioral Autonomy

Antisocial Peer Affiliation	Sum of the number of statements in which youth agrees that about half or more of his/her peers at school: (1) smoke cigarettes, (2) get drunk at least once a month, (3)
-----------------------------	--

	belong to a gang that does illegal activities, (4) use marijuana, inhalants, or other drugs, (5) cut classes or skip school.
Friends in a Gang	=1 if youth has any brothers, sisters, cousins or friends that belong to a gang.
Unsupervised Dating	=1 if youth has ever gone on a date or unsupervised social outing with a girl/boyfriend.
Earned an Allowance	=1 if youth received an allowance from family in the previous year.
Worked in an Informal Job	=1 if youth has ever worked in an informal job.
Early Problem Behavior	
Ran Away from Home	=1 if youth has ever run away from home (i.e., left home and stayed away at least overnight without parents' prior knowledge or permission).
Lifetime Delinquency Variety	Sum of the number of delinquent/criminal behavior in which youth has ever engaged: (1) vandalism, (2) petty theft (under 50 dollars), (3) petty shoplifting, (4) petty larceny, (5) petty burglary, (6) petty robbery, (7) major theft (over 50 dollars), (8) major shoplifting, (9) major larceny, (10) major burglary, (11) major robbery, (12) vehicle theft, (13) "other" property crime (e.g., receiving, possessing, selling stolen property), (14) income from stolen property, (15) income from "other" property crime, (16) aggravated assault (i.e., assault with the intention of inflicting serious harm), (17) selling drugs, (18) selling marijuana, (19) selling hard drugs (heroin, cocaine, LSD, other drugs), (20) income from selling drugs, (21) carrying a handgun, (22) arrest for a delinquent or criminal offense (not including arrest for minor traffic violations).
Involved in Minor Property Crime	=1 if youth engaged in offenses (1), (2), (3), (4), (7), (8), or (9).
Involved in Serious Property Crime	=1 if youth engaged in offenses (5), (6), (10), (11), or (12)
Involved in Income-Generating Crime	=1 if youth engaged in offenses (13), (14), (15), (17), (18), (19), or (20).
Committed Aggravated Assault	=1 if youth engaged in offense (16).
Carried a Handgun	=1 if youth engaged in offense (21).
Arrested for a Delinquent Offense	=1 if youth engaged in offense (22).
Lifetime Substance Use Variety	Sum of the number of substance use behavior in which youth has ever engaged: (1) smoked cigarettes, (2) had one or more drinks of alcohol (e.g., can or bottle of beer, glass of wine, mixed drink, shot of liquor), (3) smoked marijuana.
Smoked Cigarettes	=1 if youth engaged in offense (1).
Drank Alcohol	=1 if youth engaged in offense (2).
Used Marijuana	=1 if youth engaged in offense (3).
Recent Substance Use Variety	Sum of the number of substance use behavior in which youth has engaged in the past 30 days: (1) smoked cigarettes, (2) had one or more drinks of alcohol (e.g., can or bottle of beer, glass of wine, mixed drink, shot of liquor), (3) smoked marijuana, (4) drank five or more drinks in a single setting.
Smoked Cigarettes	=1 if youth engaged in offense (1).
Drank Alcohol	=1 if youth engaged in offense (2).
Binge Drank	=1 if youth engaged in offense (3).
Used Marijuana	=1 if youth engaged in offense (4).

REFERENCES

- Abe, Yasuyo. (1999). Adolescent antisocial behavior and work experience: NLSY79 vs. NLSY97. Paper presented at the NLSY97 Early Results Conference, Washington, DC, November 18-19.
- Abe, Yasuyo. (2001). Changes in gender and racial gaps in adolescent antisocial behavior: The NLSY97 versus the NLSY79. In Robert T. Michael (Ed.), Social Awakening: Adolescent Behavior as Adulthood Approaches (pp. 339-378). New York: Russell Sage Foundation.
- Agnew, Robert. (1986). Work and delinquency among juveniles attending school. Journal of Criminal Justice, 9: 19-41.
- Agnew, Robert. (1990). Adolescent resources and delinquency. Criminology, 28: 535-566.
- Agnew, Robert. (1992). Foundation for a general strain theory of crime and delinquency. Criminology, 30: 47-87.
- Agnew, Robert. (1994). Delinquency and the desire for money. Justice Quarterly, 11: 411-427.
- Akers, Ronald L., Marvin D. Krohn, Lonn Lanza-Kaduce, and Marcia Radosevich. (1979). Social learning and deviant behavior: A specific test of a general theory. American Sociological Review, 44: 636-655.
- Anderson, Elijah. (1999). Code of the Street: Decency, Violence, and the Moral Life of the Inner City. New York: W.W. Norton.
- Angrist, Joshua D. (2004). Treatment effect heterogeneity in theory and practice. The Economic Journal, 114: C52-C83.
- Angrist, Joshua D. and Guido W. Imbens. (1995). Two-stage least squares estimation of average causal effects in models with variable treatment intensity. Journal of the American Statistical Association, 90: 431-442.
- Angrist, Joshua D. and Alan B. Krueger. (2001). Instrumental variables and the search for identification: From supply and demand to natural experiments. Journal of Economic Perspectives, 15: 69-85.
- Angrist, Joshua D., Guido W. Imbens, and Donald B. Rubin. (1996). Identification of causal effect using instrumental variables. Journal of the American Statistical Association, 91: 444-455.

- Apel, Robert, Raymond Paternoster, Shawn Bushway, and Robert Brame. (2003). Gender and race differences in the impact of youth employment on problem behavior. Unpublished manuscript, University of Maryland, College Park.
- Ayres, Ian and Steven D. Levitt. (1998). Measuring positive externalities from unobservable victim precaution: An empirical analysis of Lojack. Quarterly Journal of Economics, 113: 43-77.
- Bachman, Jerald G. (1983). Premature affluence: Do high school students earn too much? Economic Outlook USA, 10: 64-67.
- Bachman, Jerald G., Dawn E. Bare, and Eric I. Frankie. (1986). Correlates of employment among high school seniors. Monitoring the Future Occasional Paper 20. Institute for Social Research, University of Michigan, Ann Arbor.
- Bachman, Jerald G., Lloyd D. Johnston, and Patrick M. O'Malley. (1981). Smoking, drinking, and drug use among American high school students: Correlates and trends, 1975-1979. American Journal of Public Health, 71: 59-69.
- Bachman, Jerald G., Deborah J. Safron, Susan Rogala Sy, and John E. Schulenberg. (2003). Wishing to work: New perspectives on how adolescents' part-time work intensity is linked to educational disengagement, substance use, and other problem behaviours. International Journal of Behavioral Development, 27: 301-315.
- Bachman, Jerald G. and John Schulenberg. (1992). Part-time work by high school seniors: Sorting out correlates and possible consequences. Monitoring the Future Occasional Paper 32. Institute for Social Research, University of Michigan, Ann Arbor.
- Bachman, Jerald G. and John Schulenberg. (1993). How part-time work intensity relates to drug use, problem behavior, time use, and satisfaction among high school seniors: Are these consequences or merely correlates? Developmental Psychology, 29: 220-235.
- Barling, Julian, Kimberley-Ann Rogers, and E. Kevin Kelloway. (1995). Some effects of teenagers' part-time employment: The quantity and quality of work make the difference. Journal of Organizational Behavior, 16: 143-154.
- Bartko, W. Todd and Jacquelynne S. Eccles. (2003). Adolescent participation in structured and unstructured activities: A person-oriented analysis. Journal of Youth and Adolescence, 32: 233-241.
- Baum, Christopher F., Mark E. Schaffer, and Steven Stillman. (2003). Instrumental variables and GMM: Estimation and testing. The Stata Journal, 3: 1-31.

- Baumrind, Diana. (1978). Parental disciplinary patterns and social competence in children. Youth and Society, 9: 239-276.
- Beenstock, Michael and Giora Rahav. (2002). Testing gateway theory: Do cigarette prices affect illicit drug use? Journal of Health Economics, 21: 679-698.
- Bhargava, A., L. Franzini, and W. Narendranathan. (1982). Serial correlation and the fixed effects model. Review of Economic Studies, 49: 533-549.
- Blumstein, Alfred, Jacqueline Cohen, Jeffrey A. Roth, and Christy A. Visher (Eds.). (1986). Criminal Careers and "Career Criminals" (Vol. 1). Washington, DC: National Academy Press.
- Bound, John, David A. Jaeger, and Regina M. Baker. (1995). Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variable is weak. Journal of the American Statistical Association, 90: 443-450.
- Brame, Robert, Shawn Bushway, Raymond Paternoster, and Robert Apel. (forthcoming). Assessing the effect of adolescent employment on involvement in criminal activity. Journal of Contemporary Criminal Justice.
- Brezina, Timothy. (1996). Adapting to strain: An examination of delinquent coping responses. Criminology, 34: 39-60.
- Briar, Scott and Irving Piliavin. (1965). Delinquency, situational inducements, and commitment to conformity. Social Problems, 13: 35-45.
- Bushway, Shawn, Raymond Paternoster, Robert Brame, and Robert Apel. (2003). School to work trajectories from age 14 to age 18 using the NLSY97: Are patterns of work during adolescence associated with offending and other salient life outcomes?. Paper presented at the 55th Annual Meeting of the American Society of Criminology, Denver, CO, November 19-22.
- Campbell, Ernest Q. (1969). Adolescent socialization. In David A. Goslin (Ed.), Handbook of Socialization Theory and Research (pp. 821-859). Chicago: Rand McNally.
- Carnegie Council on Policy Studies in Higher Education. (1979). Giving Youth a Better Chance: Options for Education, Work, and Service. San Francisco: Jossey-Bass.
- Carr, Rhoda V., James D. Wright, and Charles J. Brody. (1996). Effects of high school work experience a decade later: Evidence from the National Longitudinal Survey. Sociology of Education, 69: 66-81.

- Center for Human Resource Research. (2002). NLSY97 User's Guide. Columbus, OH: Center for Human Resource Research.
- Chatterji, Pinka. (2003). Illegal drug use and educational attainment. NBER Working Paper 10045. Cambridge, MA: National Bureau of Economic Research.
- Chatterji, Pinka and Sara Markowitz. (2000). The impact of maternal alcohol and illicit drug use on children's behavior problems: Evidence from the Children of the National Longitudinal Survey of Youth. NBER Working Paper 7692. Cambridge, MA: National Bureau of Economic Research.
- Cook, Philip J. and George Tauchen. (1982). The effect of liquor taxes on heavy drinking. Bell Journal of Economics, 13: 379-390.
- Crowley, Joan E. (1984). Delinquency and employment: Substitutions or spurious associations. In Michael E. Borus (Ed.), Youth and the Labor Market: Analyses of the National Longitudinal Survey (pp. 239-295). New York: W.E. Upjohn Institute for Employment Research.
- Csikszentmihalyi, Mihaly and Reed Larson. (1984). Being Adolescent: Conflict and Growth in the Teenage Years. New York: Basic Books.
- Cullen, Francis T., Martha Todd Larson, and Richard A. Mathers. (1985). Having money and delinquent involvement: The neglect of power in delinquency theory. Criminal Justice and Behavior, 12: 171-192.
- Cullen, Francis T., Nicolas Williams, and John Paul Wright. (1997). Work conditions and juvenile delinquency: Is youth employment criminogenic? Criminal Justice Policy Review, 8: 119-143.
- D'Amico, Ronald. (1984a). Does employment during high school impair academic progress? Sociology of Education, 57: 152-164.
- D'Amico, Ronald. (1984b). The time-use behavior of young adults. In Michael E. Borus (Ed.), Youth and the Labor Market: Analyses of the National Longitudinal Survey (pp. 193-238). New York: W.E. Upjohn Institute for Employment Research.
- Dave, Dhaval and Robert Kaestner. (2002). Alcohol taxes and labor market outcomes. Journal of Health Economics, 21: 357-371.
- Davidson, Russell and James G. MacKinnon. (1993). Estimation and Inference in Econometrics. New York: Oxford University Press.
- Dee, Thomas S. (2001). Does setting limits save lives? The case of 0.08 BAC laws. Journal of Policy Analysis and Management, 20: 111-118.

- Dee, Thomas S. and William N. Evans. (2003). Teen drinking and educational attainment: Evidence from two-sample instrumental variables estimates. Journal of Labor Economics, 21: 178-209.
- Deseran, Forrest A. and Diane Keithly. (1994). Teenagers in the U.S. labor force: Local labor markets, race, and family. Rural Sociology, 59: 668-692.
- DeSimone, Jeff. (2002). Illegal drug use and employment. Journal of Labor Economics, 20: 952-977.
- Donovan, John E. and Richard Jessor. (1985). Structure of problem behavior in adolescence and young adulthood. Journal of Consulting and Clinical Psychology, 53: 890-904.
- Donovan, John E., Richard Jessor, and Frances M. Costa. (1988). Syndrome of problem behavior in adolescence: A replication. Journal of Consulting and Clinical Psychology, 56: 762-765.
- Dugan, Laura and Robert Apel. (2003). An exploratory study of the violent victimization of women: Race/ethnicity and situational context. Criminology, 41: 959-979.
- Eccles, Jacquelynne S., Christy Miller Buchanan, Constance Flanagan, Andrew Fuligni, Carol Midgley, and Doris Yee. (1991). Control versus autonomy during early adolescence. Journal of Social Issues, 47: 53-68.
- Elder, Glen H. (1998). The life course as developmental theory. Child Development, 69: 1-12.
- Elliott, James R. and Mario Sims. (2001). Ghettos and barrios: The impact of neighborhood poverty and race on job matching among blacks and Latinos. Social Problems, 48: 341-361.
- Entwisle, Doris R., Karl L. Alexander, and Linda Steffel Olson. (2000). Early work histories of urban youth. American Sociological Review, 65: 279-297.
- Entwisle, Doris R., Karl L. Alexander, and Linda Steffel Olson. (2004). Temporary as compared to permanent high school dropout. Social Forces, 82: 1181-1205.
- Entwisle, Doris R., Karl L. Alexander, Linda Steffel Olson, and Karen Ross. (1999). Paid work in early adolescence: Developmental and ethnic patterns. Journal of Early Adolescence, 19: 363-388.
- Evans, William N. and Jeanne S. Ringel. (1999). Can higher cigarette taxes improve birth outcomes? Journal of Public Economics, 72: 135-154.

- Fagan, Jeffrey and Richard B. Freeman. (1999). Crime and work. In Michael Tonry (Ed.), Crime and Justice: A Review of Research, Vol. 25 (pp. 225-290). Chicago: University of Chicago Press.
- Farrell, Albert D., Steven J. Danish, and Catherine W. Howard. (1992). Relationship between drug use and other problem behaviors in urban adolescents. Journal of Consulting and Clinical Psychology, 60: 705-712.
- Farrell, Susan, Willard G. Manning, and Michael D. Finch. (2003). Alcohol dependence and the price of alcoholic beverages. Journal of Health Economics, 22: 117-147.
- Farrington, David F., Bernard Gallagher, Linda Morley, Raymond St. Leger, and Donald J. West. (1986). Unemployment, school leaving, and crime. British Journal of Criminology, 26: 335-356.
- Felson, Marcus. (1998). Crime and Everyday Life (2nd ed.). Thousand Oaks, CA: Pine Forge Press.
- Freeman, Richard B. and Harry J. Holzer (Eds.). (1986). The Black Youth Employment Crisis. Chicago: University of Chicago Press.
- Freeman, Richard B. and David A. Wise (Eds.). (1982). The Youth Labor Market Problem: Its Nature, Causes, and Consequences. Chicago: University of Chicago Press.
- Gardecki, Rosella M. (2001, August). Racial differences in youth employment. Monthly Labor Review, 124: 51-67.
- Glueck, Sheldon and Eleanor Glueck. (1950). Unraveling Juvenile Delinquency. Cambridge, MA: The Commonwealth Fund.
- Good, David H., Maureen A. Pirog-Good, and Robin C. Sickles. (1986). An analysis of youth crime and employment patterns. Journal of Quantitative Criminology, 2: 219-236.
- Gottfredson, Denise C. (1985). Youth employment, crime, and schooling: A longitudinal study of a national sample. Developmental Psychology, 21: 419-432.
- Gottfredson, Michael R. and Travis Hirschi. (1990). A General Theory of Crime. Stanford, CA: Stanford University Press.
- Granovetter, Mark S. (1973). The strength of weak ties. American Journal of Sociology, 78: 1360-1380.
- Green, Gary and Sur Norvill Jaquess. (1987). The effect of part-time employment on academic achievement. Journal of Educational Research, 80: 325-329.

- Greenberger, Ellen. (1983). A research in the policy arena. American Psychologist, 38: 104-111.
- Greenberger, Ellen and Laurence D. Steinberg. (1981). The workplace as a context for the socialization of youth. Journal of Youth and Adolescence, 10: 185-210.
- Greenberger, Ellen and Laurence D. Steinberg. (1983). Sex differences in early labor force experience: Harbinger of things to come. Social Forces, 62: 467-486.
- Greenberger, Ellen and Laurence Steinberg. (1986). When Teenagers Work: Psychological and Social Costs Adolescent Employment. New York: Basic Books.
- Greenberger, Ellen, Laurence D. Steinberg, and Mary Ruggiero. (1982). A job is a job is a job...or is it? Work and Occupations, 9: 79-96.
- Greenberger, Ellen, Laurence D. Steinberg, and Alan Vaux. (1981). Adolescents who work: Health and behavioral consequences of job stress. Developmental Psychology, 17: 691-703.
- Greenberger, Ellen, Laurence D. Steinberg, Alan Vaux, and Sharon McAuliffe. (1980). Adolescence who work: Effects of part-time employment on family and peer relations. Journal of Youth and Adolescence, 9: 189-202.
- Greene, William H. (2003). Econometric Analysis (5th ed.). Upper Saddle River, NJ: Prentice Hall.
- Hagan, John. (1991). Destiny and drift: Subcultural preferences, status attainments, and the risks and rewards of youth. American Sociological Review, 56: 567-582.
- Hagan, John and Bill McCarthy. (1997). Mean Streets: Youth Crime and Homelessness. New York: Cambridge University Press.
- Hall, Alistair R., Glenn D. Rudebusch, and David W. Wilcox. (1996). Judging instrument relevance in instrumental variables estimation. International Economic Review, 37: 283-298.
- Hall, G. Stanley. (1904). Adolescence: Its Psychology and Its Relations to Physiology, Anthropology, Sociology, Sex, Crime, Religion, and Education (Vol. 1). New York: D. Appleton and Company.
- Hansen, David M. and Patricia A. Jarvis. (2000). Adolescent employment and psychosocial outcomes: A comparison of two employment contexts. Youth and Society, 31: 417-436.

- Hausman, Jerry. (1978). Specification tests in econometrics. Econometrica, 46: 1251-1271.
- Heckman, James J. (1979). Sample selection bias as a specification error. Econometrica, 47: 153-161.
- Heckman, James. (1997). Instrumental variables: A study of explicit behavioral assumptions used in making program evaluations. Journal of Human Resources, 32: 441-462.
- Heckman, James J. and V. Joseph Hotz. (1989). Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training. Journal of the American Statistical Association, 84: 862-874.
- Heimer, Karen. (1995). Gender, race, and the pathways to delinquency. In John Hagan and Ruth D. Peterson (Eds.), Crime and Inequality (pp. 140-173). Stanford, CA: Stanford University Press.
- Heimer, Karen and Ross L. Matsueda. (1994). Role-taking, role commitment, and delinquency: A theory of differential social control. American Sociological Review, 59: 365-390.
- Hirschi, Travis. (1969). Causes of Delinquency. Berkeley, CA: University of California Press.
- Hirschi, Travis. (1983). Crime and the family. In James Q. Wilson (Ed.), Crime and Public Policy (pp. 53-68). San Francisco: ICS Press.
- Hirschi, Travis and Michael J. Hindelang. (1977). Intelligence and delinquency: A revisionist review. American Sociological Review, 42: 571-587.
- Hotchkiss, Lawrence. (1986). Work and schools—Complements or competitors? The effect of adolescent employment on dropping out. In Kathryn M. Borman and Jane Reisman (Eds.), Becoming a Worker (pp. 90-115). Norwood, NJ: Ablex.
- Hotz, V. Joseph, Lixin Colin Xu, Marta Tienda, and Avner Ahituv. (2002). Are there returns to the wages of young men from working while in school? Review of Economics and Statistics, 84: 221-236.
- Huang, Lynn, Michael Pergamit, and Jamie Shkolnik. (2001, August). Youth initiation into the labor market. Monthly Labor Review, 124: 18-24.
- Ilg, Randy E. and Steven E. Haugen. (2000). Earnings and employment trends in the 1990s. Monthly Labor Review, 123: 21-33.

- Imbens, Guido W. and Joshua D. Angrist. (1994). Identification and estimation of local average treatment effects. Econometrica, 62: 467-475.
- Jessor, Richard, John E. Donovan, and Frances M. Costa. (1991). Beyond Adolescence: Problem Behavior and Young Adult Development. New York: Cambridge University Press.
- Jessor, Richard and Shirley L. Jessor. (1977). Problem Behavior and Psychosocial Development: A Longitudinal Study of Youth. New York: Academic Press.
- Johnson, David S. and Mark Lino. (2000, September). Teenagers: Employment and contributions to family spending. Monthly Labor Review, 123: 15-25.
- Johnson, Monica Kirkpatrick. (forthcoming). Further evidence on adolescent employment and substance use. Journal of Health and Social Behavior.
- Katz, Jack. (1988). Seductions of Crime: Moral and Sensual Attractions in Doing Evil. New York: Basic Books.
- Kelejian, Harry H. (1971). Two-stage least squares and econometric systems linear in parameters but nonlinear in the endogenous variable. Journal of the American Statistical Association, 66: 373-374.
- Kelejian, Harry H. and Wallace E. Oates. (1989). Introduction to Econometrics: Principles and Applications (3rd ed.). New York: Harper and Row.
- Kett, Joseph F. (1978). Curing the disease of precocity. American Journal of Sociology, 84, Supplement: S183-S211.
- Ku, Leighton, Freya L. Sonenstein, and Joseph H. Pleck. (1993). Neighborhood, family, and work: Influences on the premarital behaviors of adolescent males. Social Forces, 72: 479-503.
- Larson, Tom and Madhu Mohanty. (1999). Minority youth employment, residential location, and neighborhood jobs: A study of Los Angeles County. Review of Black Political Economy, 28: 33-62.
- Laub, John H. and Robert J. Sampson. (2003). Shared Beginnings, Divergent Lives: Delinquent Boys to Age 70. Cambridge, MA: Harvard University Press.
- Leventhal, Tama, Julia A. Graber, and Jeanne Brooks-Gunn. (2001). Adolescent transitions to young adulthood: Antecedents, correlates, and consequences of adolescent employment. Journal of Research on Adolescence, 11: 297-323.
- Levitt, Steven D. (1996). Using electoral cycles in police hiring to estimate the effect of police on crime. American Economic Review, 87: 270-290.

- Levitt, Steven D. (1998). Juvenile crime and punishment. Journal of Political Economy, 106: 1156-1185.
- Lewin-Epstein, Noah. (1986). Effects of residential segregation and neighborhood opportunity structure on the employment of black and white youth. Sociological Quarterly, 27: 559-570.
- Liang, Lan and Frank J. Chaloupka. (2002). Differential effects of cigarette price on youth smoking intensity. Nicotine and Tobacco Research, 4: 109-114.
- Light, Audrey. (1994). Transitions from School to Work: A Survey of Research Using the National Longitudinal Surveys. NLS discussion paper, NLS 94-18.
- Lillydahl, Jane H. (1990). Academic achievement and part-time employment of high school students. Journal of Economic Education, 21: 307-316.
- Lorence, Jon and Jeylan T. Mortimer. (1985). Job involvement through the life course: A panel study of three age groups. American Sociological Review, 50: 618-638.
- Manning, Wendy D. (1990). Parenting employed teenagers. Youth and Society, 22: 184-200.
- Manski, Charles F. (1995). Identification Problems in the Social Sciences. Cambridge, MA: Harvard University Press.
- Marsh, Herbert W. (1991). Employment during high school: Character building or a subversion of academic goals? Sociology of Education, 64: 172-189.
- Matsueda, Ross L. (1992). Reflected appraisals, parental labeling, and delinquency: Specifying a symbolic interactionist theory. American Journal of Sociology, 97: 1577-1611.
- Matza, David. (1964/1990). Delinquency and Drift. New Brunswick, NJ: Transaction.
- McGee, Linda and Michael D. Newcomb. (1992). General deviance syndrome: Expanded hierarchical evaluations at four ages from early adolescence to adulthood. Journal of Consulting and Clinical Psychology, 60: 766-776.
- McKinnish, Terra G. (2000). Model sensitivity in panel data analysis: Some caveats about the interpretation of fixed effects and differences estimators. Unpublished manuscript, University of Colorado, Boulder.
- McMorris, Barbara J. and Christopher Uggen. (2000). Alcohol and employment in the transition to adulthood. Journal of Health and Social Behavior, 41: 276-294.

- McNeal, Ralph B., Jr. (1997). Are students being pulled out of high school? The effects of adolescent employment on dropping out. Sociology of Education, 70: 206-220.
- Meyer, Robert H. and David A. Wise. (1982). High school preparation and early labor force experience. In Richard B. Freeman and David A. Wise (Eds.), The Youth Labor Market Problem: Its Nature, Causes, and Consequences (pp. 277-339). Chicago: University of Chicago Press.
- Michael, Robert T. and Nancy Brandon Tuma. (1984). Youth employment: Does life begin at 16? Journal of Labor Economics, 2: 464-476.
- Mihalic, Sharon Wofford and Delbert Elliott. (1997). Short- and long-term consequences of adolescent employment. Youth and Society, 28: 464-498.
- Moehling, Carolyn. (1999). State child labor laws and the decline of child labor. Explorations in Economic History, 36: 72-106.
- Moffitt, Terrie E. (1993). Adolescence-limited and life-course persistent antisocial behavior: A developmental taxonomy. Psychological Review, 100: 674-701.
- Moffitt, Terrie E., Avshalom Caspi, Michael Rutter, and Phil A. Silva. (2001). Sex Differences in Antisocial Behaviour: Conduct Disorder, Delinquency, and Violence in the Dunedin Longitudinal Study. New York: Cambridge University Press.
- Mortimer, Jeylan T. (2003). Working and Growing Up in America. Cambridge, MA: Harvard University Press.
- Mortimer, Jeylan T. and Michael D. Finch. (1986). The effects of part-time work on adolescent self-concept and achievement. In Kathryn M. Borman and Jane Reisman (Eds.), Becoming a Worker (pp. 66-89). Norwood, NJ: Ablex.
- Mortimer, Jeylan T., Michael D. Finch, Timothy J. Owens, and Michael J. Shanahan. (1990). Gender and work in adolescence. Youth and Society, 22: 201-224.
- Mortimer, Jeylan T., Michael D. Finch, Seongryeol Ryu, Michael J. Shanahan, and Kathleen T. Call. (1996). The effects of work intensity on adolescent mental health, achievement, and behavioral adjustment: New evidence from a prospective study. Child Development, 67: 1243-1261.
- Mortimer, Jeylan T. and Monica Kirkpatrick Johnson. (1998). New perspectives on adolescent work and the transition to adulthood. In Richard Jessor (Ed.), New Perspectives on Adolescent Risk Behavior (pp. 425-496). New York: Cambridge University Press.

- Mortimer, Jeylan T. and Michael J. Shanahan. (1994). Adolescent work experience and family relationships. Work and Occupations, 21: 369-384.
- Mullahy, John and Paul R. Portney. (1990). Air pollution, cigarette smoking, and the production of respiratory health. Journal of Health Economics, 9: 193-205.
- Mullahy, John and Jody Sindelar. (1996). Employment, unemployment, and problem drinking. Journal of Health Economics, 15: 409-434.
- Nagin, Daniel S. and Kenneth C. Land. (1993). Age, criminal careers, and population heterogeneity: Specification and estimation of a nonparametric, mixed Poisson model. Criminology, 31: 327-362.
- Nagin, Daniel S., David P. Farrington, and Terrie E. Moffitt. (1995). Life-course trajectories of different types of offenders. Criminology, 33: 111-139.
- Nagin, Daniel S. and Raymond Paternoster. (1991). On the relationship of past and future participation in delinquency. Criminology, 29: 163-190.
- Nagin, Daniel S. and Raymond Paternoster. (2000). Population heterogeneity and state dependence: State of the evidence and directions for future research. Journal of Quantitative Criminology, 16: 117-144.
- National Commission on the Reform of Secondary Education. (1973). The Reform of Secondary Education. New York: McGraw-Hill.
- National Commission on Youth. (1980). The Transition of Youth to Adulthood: A Bridge Too Long. Boulder, CO: Westview Press.
- National Panel on High School and Adolescent Education. (1976). The Education of Adolescents. Washington, DC: Office of Education, U.S. Department of Health, Education, and Welfare.
- National Research Council. (1998). Protecting Youth at Work: Health, Safety, and Development of Working Children and Adolescents in the United States. Washington, DC: National Academy Press.
- Nelson, Richard R. (1997, January). State labor legislation enacted in 1996. Monthly Labor Review, 120: 29-41.
- Nelson, Richard R. (1998, January). State labor legislation enacted in 1997. Monthly Labor Review, 121: 3-22.
- Nelson, Richard R. (1999, January). State labor legislation enacted in 1998. Monthly Labor Review, 122: 3-15.

- Nelson, Richard R. (2000, January). State labor legislation enacted in 1999. Monthly Labor Review, 123: 3-19.
- Nelson, Richard R. (2001, January). State labor legislation enacted in 2000. Monthly Labor Review, 124: 12-24.
- Nelson, Richard R. (2002, January). State labor legislation enacted in 2001. Monthly Labor Review, 125: 12-30.
- Nelson, Richard R. (2003, January). State labor legislation enacted in 2002. Monthly Labor Review, 126: 3-24.
- Newcomb, Michael D. and Peter M. Bentler. (1988). Consequences of Adolescent Drug Use: Impact on the Lives of Young Adults. Newbury Park, CA: Sage.
- Newman, Katherine S. (1999). No Shame in My Game: The Working Poor in the Inner City. New York: Russell Sage Foundation.
- Nye, F. Ivan. (1958). Family Relationships and Delinquent Behavior. New York: John Wiley and Sons.
- Oettinger, Gerald S. (2000, April). Seasonal and sectoral patterns in youth employment. Monthly Labor Review, 123: 6-12.
- Ohsfeldt, Robert L. and Michael A. Morrissey. (1997). Beer taxes, workers' compensation, and industrial injury. Review of Economics and Statistics, 79: 155-160.
- O'Regan, Kathryn M. and John M. Quigley. (1996). Teenage employment and the spatial isolation of minority and poverty households. Journal of Human Resources, 31: 692-702.
- Osgood, D. Wayne. (1999). Having the time of their lives: All work and no play? In Alan Booth, Ann C. Crouter, and Michael J. Shanahan (Eds.), Transitions to Adulthood in a Changing Economy: No Work, No Family, No Future? (pp. 176-186). Westport, CT: Praeger.
- Osgood, D. Wayne, Lloyd D. Johnston, Patrick M. O'Malley, and Jerald G. Bachman. (1988). The generality of deviance in late adolescence and early adulthood. American Sociological Review, 53: 81-93.
- Osgood, D. Wayne, Janet K. Wilson, Patrick M. O'Malley, Jerald G. Bachman, and Lloyd D. Johnston. (1996). Routine activities and individual deviant behavior. American Sociological Review, 61: 635-655.

- Panel on Youth of the President's Science Advisory Committee. (1974). Youth: Transition to Adulthood. Chicago: University of Chicago Press.
- Paternoster, Raymond, Shawn Bushway, Robert Brame, and Robert Apel. (2003). The effect of teenage employment on delinquency and problem behaviors. Social Forces, 82: 297-335.
- Phillips, Sarah and Kent L. Sandstrom. (1990). Parental attitudes toward youth work. Youth and Society, 22: 160-183.
- Pickering, Lloyd E. and Alexander T. Vazsonyi. (2002). The impact of adolescent employment on family relationships. Journal of Adolescent Research, 17: 196-218.
- Ploeger, Matthew. (1997). Youth employment and delinquency: Reconsidering a problematic relationship. Criminology, 35: 659-675.
- Resnick, Michael D., Peter S. Bearman, Robert Wm. Blum, Karl E. Bauman, Kathleen M. Harris, Jo Jones, Joyce Tabor, Trish Beuhring, Renee E. Sieving, Marcia Shew, Marjorie Ireland, Linda H. Bearinger, and J. Richard Udry. (1997). Protecting adolescents from harm: Findings from the National Longitudinal Study of Adolescent Health. Journal of the American Medical Association, 278: 823-832.
- Rothstein, Donna S. (2001, August). Youth employment in the United States. Monthly Labor Review, 124: 6-17.
- Ruggiero, Mary Grace. (1984). Work as an Impetus to Delinquency: An Examination of Theoretical and Empirical Connections. Unpublished Ph.D. dissertation, University of California, Irvine.
- Ruggiero, Mary, Ellen Greenberger, and Laurence D. Steinberg. (1982). Occupational deviance among adolescent workers. Youth and Society, 13: 423-448.
- Ruhm, Christopher J. (1995). The extent and consequences of high school employment. Journal of Labor Research, 16: 293-303.
- Ruhm, Christopher J. (1996). Alcohol policies and highway vehicle fatalities. Journal of Health Economics, 15: 435-454.
- Ruhm, Christopher J. (1997). Is high school employment consumption or investment? Journal of Labor Economics, 15: 735-776.
- Saffer, Howard and Dhaval Dave. (2003). Alcohol advertising and alcohol consumption by adolescents. NBER Working Paper 9676. Cambridge, MA: National Bureau of Economic Research.

- Safron, Deborah J., John E. Schulenberg, and Jerald G. Bachman. (2001). Part-time work and hurried adolescence: The links among work intensity, social activities, health behaviors, and substance use. Journal of Health and Social Behavior, 42: 425-449.
- Sampson, Robert J. and John H. Laub. (1993). Crime in the Making: Pathways and Turning Points through Life. Cambridge, MA: Harvard University Press.
- Schill, William J., Rosemarie McCartin, and Katrina Meyer. (1985). Youth employment: Its relationship to academic and family variables. Journal of Vocational Behavior, 26: 155-163.
- Schoenhals, Mark, Marta Tienda, and Barbara Schneider. (1998). The educational and personal consequences of adolescent employment. Social Forces, 77: 723-762.
- Shanahan, Michael J., Glen H. Elder, Jr., Margaret Burchinal, and Rand D. Conger. (1996). Adolescent paid labor and relationships with parents: Early work-family linkages. Child Development, 67: 2183-2200.
- Shanahan, Michael J., Michael Finch, Jeylan T. Mortimer, and Seongryeol Ryu. (1991). Adolescent work experience and depressive affect. Social Psychology Quarterly, 54: 299-317.
- Shannon, Lyle W. (1982). Assessing the Relationship of Adult Criminal Careers to Juvenile Careers: A Summary. Washington, DC: Office of Juvenile Justice and Delinquency Prevention, U.S. Department of Justice.
- Shannon, Lyle W. (1988). Criminal Career Continuity: Its Social Context. New York: Human Sciences Press.
- Shea, John. (1997). Instrument relevance in multivariate linear models: A simple measure. Review of Economics and Statistics, 79: 348-352.
- Singh, Kusum. (1998). Part-time employment in high school and its effect on academic achievement. Journal of Educational Research, 91: 131-139.
- Singh, Kusum and Mehmet Ozturk. (2000). Effect of part-time work on high school mathematics and science course taking. Journal of Educational Research, 94: 67-74.
- Staff, Jeremy and Christopher Uggen. (2003). The fruits of good work: Early work experiences and adolescent deviance. Journal of Research in Crime and Delinquency, 40: 263-290.
- Steel, Lauri. (1991). Early work experience among white and non-white youths: Implications for subsequent enrollment and employment. Youth and Society, 22: 419-447.

- Steinberg, Laurence. (1996). Beyond the Classroom: Why School Reform Has Failed and What Parents Need to Do. New York: Touchstone.
- Steinberg, Laurence D. (1982). Jumping off the work experience bandwagon. Journal of Youth and Adolescence, 11: 183-205.
- Steinberg, Laurence and Shelli Avenevoli. (1998). Disengagement from school and problem behavior in adolescence: A developmental-contextual analysis of the influence of family and part-time work. In Richard Jessor (Ed.), New Perspectives on Adolescent Risk Behavior (pp. 392-424). New York: Cambridge University Press.
- Steinberg, Laurence and Elizabeth Cauffman. (1995). The impact of employment on adolescent development. Annals of Child Development, 11: 131-166.
- Steinberg, Laurence and Sanford M. Dornbusch. (1991). Negative correlates of part-time employment during adolescence: Replication and elaboration. Developmental Psychology, 27: 304-313.
- Steinberg, Laurence, Suzanne Fegley, and Sanford M. Dornbusch. (1993). Negative impact of part-time work on adolescent adjustment: Evidence from a longitudinal study. Developmental Psychology, 29: 171-180.
- Steinberg, Laurence D., Ellen Greenberger, Laurie Garduque, Mary Ruggiero, and Alan Vaux. (1982). Effects of working on adolescent development. Developmental Psychology, 18: 385-395.
- Steinberg, Laurence D., Ellen Greenberger, Alan Vaux, and Mary Ruggiero. (1981). Early work experience: Effects on adolescent occupational socialization. Youth and Society, 12: 403-422.
- Stephenson, Stanley P., Jr. (1981a). In-school labour force status and post-school wage rates of young men. Applied Economics, 13: 279-302.
- Stephenson, Stanley P., Jr. (1981b). Young women and labor: In-school labor force status and early post school labor market outcomes.
- Stern, David and Yoshi-fumi Nakata. (1989). Characteristics of high school students' paid jobs, and employment experiences after graduation. In David Stern and Dorothy Eichorn (Eds.), Adolescence and Work: Influences of Social Structure, Labor Markets, and Culture (pp. 189-233). Hillsdale, NJ: Lawrence Erlbaum Associates.
- Stevenson, Wayne. (1978). The relationship between early work experience and future employability. In Arvil V. Adams and Garth L. Magnum, The Lingering Crisis of

- Youth Unemployment (pp. 93-124). Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.
- Stoll, Michael A. (1998). Spatial mismatch, discrimination, and male youth employment in the Washington, DC area: Implications for residential mobility policies. Journal of Policy Analysis and Management, 18: 77-98.
- Sullivan, Mercer L. (1989). "Getting Paid": Youth Crime and Work in the Inner City. Ithaca, NY: Cornell University Press.
- Sutherland, Edwin H. (1947). Principles of Criminology (4th ed.). Chicago: J.B. Lippincott.
- Tanner, Julian and Harvey Krahn. (1991). Part-time work and deviance among high-school seniors. Canadian Journal of Sociology, 16: 281-302.
- Tauras, John A. and Lan Liang. (2003). The impact of physician intervention and tobacco control policies on average daily cigarette consumption among adult smokers. NBER Working Paper 9790. Cambridge, MA: National Bureau of Economic Research.
- Thornberry, Terence P. and R.L. Christenson. (1984). Unemployment and criminal involvement: An investigation of reciprocal causal structures. American Sociological Review, 56: 609-627.
- Topel, Robert H. and Michael P. Ward. (1992). Job mobility and the careers of young men. Quarterly Journal of Economics, 107: 439-479.
- Triplett, Ruth. (1993). The conflict perspective, symbolic interactionism, and the status characteristics hypothesis. Justice Quarterly, 10: 541-558.
- Tyler, John H. (2003). Using state child labor laws to identify the effect of school-year work in high school achievement. Journal of Labor Economics, 21: 381-408.
- Uggen, Christopher. (2000a). Class, gender, and arrest: An intergenerational analysis of workplace power and control. Criminology, 38: 835-862.
- Uggen, Christopher. (2000b). Work as a turning point in the life course of criminals: A duration model of age, employment, and recidivism. American Sociological Review, 65: 529-546.
- U.S. Department of Labor. (2000). Report on the Youth Labor Force. Washington, DC: Bureau of Labor Statistics, U.S. Department of Labor.
- Ward, David A. and Charles R. Tittle. (1994). IQ and delinquency: A test of two competing explanations. Journal of Quantitative Criminology, 10: 189-212.

- Warr, Mark. (2002). Companions in Crime: The Social Aspects of Criminal Conduct. New York: Cambridge University Press.
- Warren, John Robert. (2002). Reconsidering the relationship between student employment and academic outcomes: A new theory and better data. Youth and Society, 33: 366-393.
- Warren, John Robert, Emily Forrest Cataldi, and Jennifer C. Lee. (2001). Adolescent employment and high school dropout. Unpublished manuscript, University of Washington.
- Warren, John Robert and Jennifer C. Lee. (2003). The impact of adolescent employment on high school dropout: Differences by individual and labor-market characteristics. Social Science Research, 32: 98-128.
- Warren, John Robert, Paul C. LePore, and Robert D. Mare. (2000). Employment during high school: Consequences for students' grades in academic courses. American Educational Research Journal, 37: 943-969.
- White, Lynn K. and David B. Brinkerhoff. (1981). The sexual division of labor: Evidence from childhood. Social Forces, 60: 170-181.
- Wilson, James Q. and Richard J. Herrnstein. (1985). Crime and Human Nature. New York: Simon and Schuster.
- Winship, Christopher and Stephen L. Morgan. (1999). The estimation of causal effects from observational data. Annual Review of Sociology, 25: 659-706.
- Witte, Anne Dryden and Helen Tauchen. (1994). Work and crime: An exploration using panel data. Public Finance, 49: 155-167.
- Wood, Peter B., Walter R. Gove, James A. Wilson, and John K. Cochran. (1997). Nonsocial reinforcement and habitual criminal conduct: An extension of learning theory. Criminology, 35: 335-366.
- Wooldridge, Jeffrey M. (2002). Econometric Analysis of Cross Section and Panel Data. Cambridge, MA: MIT Press.
- Wright, John Paul and Francis T. Cullen (2000). Juvenile involvement in occupational delinquency. Criminology, 38: 863-896.
- Wright, John Paul, Francis T. Cullen, Robert S. Agnew, and Timothy Brezina. (2001). "The root of all evil?" An exploratory study of money and delinquent involvement. Justice Quarterly, 18: 239-268.

Wright, John Paul, Francis T. Cullen, and Nicolas Williams. (1997). Working while in high school and delinquent involvement: Implications for social policy. Crime and Delinquency, 43: 203-221.

Wright, John Paul, Francis T. Cullen, and Nicolas Williams. (2002). The embeddedness of adolescent employment and participation in delinquency: A life course perspective. Western Criminology Review, 4: 1-19.