

Revisiting the Impact of Part-Time Work on Adolescent Adjustment: Distinguishing Between Selection and Socialization Using Propensity Score Matching

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The impact of part-time employment on adolescent functioning remains unclear because most studies fail to adequately control for differential selection into the workplace. The present study reanalyzes data from Steinberg, L., Fegley, S., and Dornbusch, S. M. (1993) using multiple imputation, which minimizes bias in effect size estimation, and two types of propensity score matching, to account for selection effects. In this sample ($N = 1,792$; grades 10–11, $M = 16.26$), youth who begin working more than 20 hr weekly evince declines in school engagement and increases in substance use and delinquency compared with youth who remain unemployed. Conversely, working 20 hr or less has negligible effects, positive or negative, on academic, psychological, or behavioral outcomes.

Working part-time during the school year has been a fixture of American adolescence for more than 30 years. Today, as has been the case since 1980, a substantial proportion of American high school students hold part-time jobs during the school year, and a large number of these teenagers work in excess of 20 hr each week (Staff, Messersmith, & Schulenberg, 2009). During the past two decades, numerous studies of the impact of working on adolescent development have been published, but the literature on the effects of part-time employment is riddled with inconsistent findings and a lack of consensus on whether intensive school-year employment (typically defined as working more than 20 hr a week) is detrimental to adolescents' academic, behavioral, and psychological functioning.

Policy-makers, educators, and parents often seek guidance about raising healthy children, but in many instances the research on best practices can

only be studied using nonexperimental data, which limit the strength of the conclusions—and the advice—developmental scientists can offer. Despite these limitations, researchers are still responsible for translating their work so that the general public can make informed decisions that promote positive development and minimize negative outcomes for youth. Coupled with the obligation for translational research is the need for developmental scientists to employ analytic techniques that minimize bias in nonexperimental research to give the best advice that science can allow at that point in time.

One of the greatest limitations prohibiting researchers from drawing firm causal conclusions from nonexperimental data is the problem of selection effects. In the real world, children and youth are not randomly assigned to day care, to go online, or to participate in community service. Consequently, it is difficult to say with certainty how children and youth are affected by child care, the Internet, or volunteer activities. Without experimental designs using random assignment, it is difficult to conclude whether observed differences between individuals who do or do not have an experience are because of (a) preexisting characteristics

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1 that impact one's propensity to have that experi-
2 ence or (b) the experience itself. This uncertainty
3 has long characterized the study of adolescent
4 employment.

5 Research by Greenberger and Steinberg in the
6 1980s challenged the assumption that employment
7 promoted responsible behavior and deterred delin-
8 quency and found instead that intensive employ-
9 ment predicted lower achievement and more
10 problem behavior (Greenberger & Steinberg, 1986).
11 Since then, experts have debated whether or not
12 intensive school-year employment is detrimental to
13 adolescents' academic, behavioral, and psychologi-
14 cal functioning, with the consensus changing with
15 each new wave of research. In the present study,
16 we reanalyze data used by Steinberg, Fegley, and
17 Dornbusch (1993), employing a more rigorous sta-
18 tistical method for modeling selection effects. The
19 data set is particularly well suited for this purpose
20 because it includes measures of many factors that
21 potentially affect selection into the labor force, and
22 not just the demographic variables often used to
23 take preexisting differences into account. Moreover,
24 the longitudinal design of the study by Steinberg
25 et al. permits assessment of *changes* in employment
26 status, including increases and decreases in work
27 hours; thus, we examine how changes in employ-
28 ment affect adolescent outcomes.

31 *The Impact of Employment on Adolescent Well-Being*

32 Steinberg et al. (1993) compared the effects of
33 moderate (under 20 hr/week) and intensive
34 employment (more than 20 hr/week) on changes in
35 academic, behavioral, and psychosocial outcomes
36 1 year later, controlling for demographics as well as
37 initial scores on the academic, behavioral, and psy-
38 chosocial measures. They found that working long
39 hours during the school year predicted negative
40 outcomes in multiple domains (e.g., school engage-
41 ment, delinquency and substance use, autonomy
42 from parents, and self-reliance), whereas leaving
43 the workforce was related to improvements in aca-
44 demic outcomes.

45 The results of the Steinberg et al. (1993) study are
46 fairly typical of nonexperimental research on
47 adolescent employment, which often show that
48 employment during the school year, particularly
49 intensive employment, is associated with negative
50 outcomes, including antisocial behavior (Greenber-
51 ger & Steinberg, 1986; McMorris & Uggen, 2000;
52 Steinberg & Dornbusch, 1991; Steinberg et al., 1993),
53 lower school achievement (Carr, Wright, & Brody,
54 1996; D'Amico, 1984; Greenberger & Steinberg,

1986; Mortimer & Johnson, 1998; Steinberg, Green-
berger, Garduque, Ruggiero, & Vaux, 1982), and
higher rates of school absence and dropout (Lee &
Staff, 2007; Schoenhals, Tienda, & Schneider, 1998).
Some researchers suggest that working promotes
"precocious maturity," as youth extend their
involvement in adult behavior beyond employment
to include engagement in behaviors such as smok-
ing, drinking, and sexual activity (Bachman, Safron,
Sy, & Schulenberg, 2003; Bachman & Schulenberg,
1993; Bozick, 2006). Others cite the time commit-
ment as the primary issue, in that working long
hours detracts from involvement in extracurricular
activities and other school-related endeavors (e.g.,
studying, getting extra help, etc.; Carr et al., 1996;
D'Amico, 1984; Greenberger & Steinberg, 1986).
Finally, some writers report that the typical work
setting for teens often lacks adult supervision and
mentorship, thus providing more opportunities for
youth to engage in deviant behavior (Greenberger,
1988; Wright & Cullen, 2000) and diminishing expo-
sure to adults at home or in school.

Although many have reported that working dur-
ing the school year is related to negative behavioral
and academic outcomes, several studies suggest
otherwise. Mortimer (2003) followed a large group
of adolescents through high school into early adult-
hood and concluded that having a job during high
school can promote positive psychosocial develop-
ment and responsible behavior; furthermore, as
adults, these students held an advantage in the
workforce over students who were not employed
during high school. In addition, there is some evi-
dence that working fewer than 20 hr per week,
compared with not working, is associated with
lower school dropout rates (D'Amico, 1984),
increased involvement in school activities (Mihalic
& Elliot, 1997), and better grades (Mortimer & **4**
Johnson, 1998).

The effects of working cannot be adequately
studied without taking into account differential
selection into the workforce. For example, because
an adolescent's orientation toward academics or
substance use might influence the decision to work,
or to work long hours, it is entirely plausible that
observed differences between workers and non-
workers, or among workers who work different
amounts, are because of preexisting differences and
not because of consequences of employment. This
is possible both with respect to the putative nega-
tive consequences of working (e.g., some students
choose to work long hours because they are less
concerned about how they do in school) and posi-
tive ones (e.g., some students choose to limit their

1 employment hours so as not to interfere with other
2 commitments).

3 4 *Accounting for Selection Effects*

5
6 In a much previous work on adolescent employ-
7 ment, researchers attempted to account for selection
8 effects using approaches (e.g., controlling for con-
9 current or time-lagged covariates) that do not take
10 unobserved heterogeneity into account (see Heck-
11 man & Hotz, 1989 or Winship & Mare, 1992 for a
12 discussion of limitations of these approaches).
13 When statistical methods specifically designed to
14 account for unobserved sample heterogeneity are
15 employed, many of the putative effects of school-
16 year employment disappear. For example, one team
17 of researchers used group-based trajectory model-
18 ing to address the issue of unobserved heterogene-
19 ity and found that the transition to employment
20 was not associated with greater risk of delinquent
21 behavior (Apel et al., 2007). In another study,
22 researchers estimated fixed-effect logit and ran-
23 dom-effect probit models to account for unobserved
24 heterogeneity and similarly found no significant
25 within-individual variation in delinquent behaviors
26 as a result of employment status (Paternoster,
27 Bushway, Brame, & Apel, 2003). Finally, Warren,
28 LePore, and Mare (2000) estimated a simultaneous
29 equations model looking at both short- and long-
30 term effects of employment, as well as the poten-
31 tially reciprocal relations between employment and
32 academic achievement, and found no negative
33 effects of employment on school grades.

34
35 Recently, Lee and Staff (2007) used a propensity
36 score matching technique to assess variation in stu-
37 dents' propensities toward working at different lev-
38 els of intensity; once students were matched on
39 these propensities, high school dropout was exam-
40 ined as an outcome. They found that overall, inten-
41 sive employment increased the odds of high school
42 dropout 1.5 times compared with moderate
43 employment. However, the researchers went a step
44 further and, based on the value derived from the
45 propensity score, divided the sample into students
46 who had a low, moderate, or high likelihood (pro-
47 pensity) to work more than 20 hr per week. The
48 results of this analysis suggested that only among
49 individuals with low or moderate propensity to
50 work was there a link between high-intensity
51 employment and school dropout. In contrast,
52 among students with a high propensity to work
53 more than 20 hr weekly, there was no association
54 between working long hours and high school drop-
55 out. Individuals with a high propensity to work

tended to report greater economic disadvantage
and lower school performance, suggesting that the
relation between high-intensity employment and
school dropout may vary as a function of socio-
economic status and prior academic success.

Although studies of school-year employment
that account for unobserved heterogeneity have
called into question the results of previous research,
several limitations in the extant literature still exist.
First, both the Paternoster et al. (2003) and the Apel
et al. (2007) studies focused exclusively on
delinquent behavior and substance use, and it is
plausible that intensive employment during the
school year might have a negative impact on
other domains, including academic and psychoso-
cial functioning. And although Warren et al.
(2000) examined how employment affects grade-
point-averages (GPAs), and Lee and Staff (2007)
examined high school dropout, neither study tested
how employment might impact school engagement
more broadly (e.g., paying attention in class, spend-
ing time on homework), which may be important
to assess if working students are able to protect
their GPAs by taking easier classes or by cheating,
as some studies have reported (Steinberg et al.,
1993). Another limitation of these newer studies is
that they do not examine the effects that *leaving* a
job might have on psychological and behavioral
outcomes. This is a potentially important oversight,
especially given the Steinberg et al.'s (1993) finding
that leaving the workforce had positive effects on
school performance.

In the present study, we reanalyze the data from
Steinberg et al. (1993) using propensity score
matching, an analytic approach that is more suit-
able for modeling selection effects than that origi-
nally utilized by these researchers (i.e., time-lagged
controls). These data are ideally suited for this pur-
pose because they come from a large, ethnically,
and socioeconomically diverse community sample
on which assessments of a wide range of outcomes,
as well as information about employment, were col-
lected longitudinally. The longitudinal design
allows us to examine the dynamic effects of *change*
in work status or hours over time. Although these
data are 20 years old, there is little reason to believe
that they are not generalizable to today's adoles-
cents because the nature of the jobs held by stu-
dents has not changed during the past two decades
(Staff et al., 2009).

Propensity score matching (Rosenbaum & Rubin,
1983) creates balance between "treatment" and
"control" groups (e.g., youth who change work
status and youth who do not) by matching

participants on a number of covariates that may affect group membership. The assumption behind this approach is that individuals have an underlying propensity to be in one group or another, and matching them based on preexisting characteristics provides a way of statistically controlling the variation in these characteristics, thereby minimizing selection effects. Because missing data can impact the accuracy of propensity score matching, it is best to utilize full data on all matched variables. Instead of excluding subjects with missing data, we employ multiple imputation (MI) in the present study to limit bias in our analyses. This approach accommodates many patterns of missing data and reflects uncertainty when inputting the missing values (Little & Rubin, 2002; Rubin, 1987), which ultimately allows for the inclusion of a larger and more representative sample in the analyses.

Utilizing these statistical modifications, we investigate whether there are differences in school performance and engagement, problem behavior, depressive symptoms, autonomy from parents, and psychosocial functioning (i.e., the outcomes examined by Steinberg et al.) among groups of youth who enter the workplace at moderate (20 hr/week or less) or intensive (more than 20 hr/week) levels of employment, transition to a higher or lower level of employment intensity, leave the workforce from either moderate or intensive employment, or remain nonemployed during the school year. We ask whether intensive employment during the school year does in fact have a negative effect on academic, behavioral, and psychosocial outcomes even after taking unobserved sample heterogeneity into account.

Method

Participants

The present study uses a sample of youths from nine high schools in Wisconsin and northern California. Schools were selected for diversity in terms of socioeconomic status, ethnicity, and urbanicity. Data were collected over the course of two school years, 1987–1988 (Time 1) and 1988–1989 (Time 2) and were provided by the original researchers.

Analyses are limited to youths who provided data on employment status at both time points. In addition, because we were interested in changes in work status, we only included youths who were enrolled in Grade 10 or 11 at the first time point. Ninth graders were excluded because a very small proportion were employed, or became employed,

over the next year. Twelfth graders were excluded because they graduated and did not provide data at the second time point.

Thus, the final analytic sample consisted of 1,792 youths who were enrolled in tenth or eleventh grade at Time 1 (mean age of 16.26, $SD = 0.84$; 960 males, 832 females). The sample was primarily White (65%), followed by Asian (16%), Hispanic (8%), African American (8%), and youth of other ethnicities (3%). Sixty-eight percent of youth lived with both biological mother parents, 16% lived with a biological parent and a step-parent, 8% were from single parent homes, and the remaining 8% lived in other familial contexts. Fewer than 10% of youth had parents with less than a high school education, 13% of parents were high school graduates, 31% of parents had completed some college, 12% had college diplomas, and the remaining 34% had post-college education or a professional degree.

Compared with the full sample, youth in our analytic sample came from families with higher parental education [$t(9070) = -7.852$; $p < 0.01$] and were more likely to be White ($\chi^2(3) = 85.13$; $p < 0.01$). Furthermore, when we compared individuals in the analytic sample with those who met age requirements for inclusion but did not provide employment data at both time points, we found the same pattern of differences: Our sample reported higher parental education [$t(4481) = -8.09$; $p < 0.01$] and were more likely to be Caucasian [$\chi^2(3) = 91.95$; $p < 0.01$]; additionally, they were slightly but significantly older than youth with missing job data [$t(4659) = 2.76$; $p < 0.01$].

Procedures

Because “active parental consent” may result in sampling biases that overrepresent well-functioning adolescents (e.g., Weinberger, Tublin, Ford, & Feldman, 1990), with university IRB, funding agency, and school district support, the study employed an “active” informed assent procedure with the adolescents, but “passive” informed consent with their parents. Parents were asked to call or write to their child’s school or research office if they did *not* want their child to participate in the research (this is no longer allowed by most university IRBs, but it was common at the time of data collection). Approximately 80% of eligible students completed the survey. Reasons for not completing the survey were because of parents withholding consent (<1%), adolescents withholding assent (<5%), and absence from school on study administration days (approximately 15%, a figure consistent with national school

attendance rates at the time of the study). The study was conducted on two separate days during the fall of each school year. Surveys were administered in classrooms under the supervision of a teacher and a representative from the research staff.

Measures

Of interest in the present analyses are demographic measures, measures of employment, and measures of our outcomes of interest: school performance and engagement, problem behavior, depressive symptomatology, autonomy from parents, and psychosocial development. Time 1 scores on each of these variables and demographic measures were utilized to create propensity scores; at Time 2, these variables were outcomes of interest. The analyses (a) assess the propensity to belong to each of the work transition groups as a function of demographic factors, and academic, behavioral, and psychosocial status at Time 1; (b) match individuals across work groups based on their propensity score; and (c) examine changes in the outcomes of interest between Times 1 and 2 as a function of changes in employment status.

Employment. Each year, students reported (a) if they had a "regular paying part-time job" and (b), if so, how many hours they typically worked in a given week. Based on these variables, individuals were classified using the categories from Steinberg et al. (1993) as *non-employed*, *employed at moderate intensity* (20 hr/week or less), or *employed at high intensity* (more than 20 hr/week) at each time point. Based on responses at both time points, we identified nine groups of youths: (1) youths who were nonemployed at both time points ($n = 655$), (2) youths who were nonemployed and became employed at moderate intensity ($n = 356$), (3) youths who were nonemployed and became employed at high intensity ($n = 107$), (4) youths who were employed at moderate intensity and became nonemployed ($n = 140$), (5) youths who were employed at moderate intensity and remained so ($n = 286$), (6) youths who were employed at moderate intensity and increased to high-intensity employment ($n = 99$), (7) youths who were employed at high intensity and became nonemployed ($n = 38$), (8) youths who were employed at high intensity and decreased to moderate-intensity employment ($n = 49$), and (9) youths who were employed at high intensity across both time points ($n = 62$). In six of the nine groups, youth's employment status changed between Times 1 and 2; these six groups represent the "treatment" groups and

were each compared with one of the relevant three "control" groups, whose employment status remained the same at both time points (e.g., Groups 1, 5, and 9).

Demographics. Five demographic variables, including age and gender, were included and all were based on youth self-report. *Ethnicity* was reported in one of seven categories: Caucasian, Asian, Hispanic, African American, American Indian, Pacific Islander, or Middle Eastern; because of the small prevalence of certain ethnic groups, data were recoded into five groups: Caucasian, Asian, Hispanic, African American, and Other. The highest level of parental education for either mother or father was coded on an 8-point scale, ranging from "less than high school education" to "professional/advanced degree." Finally, a variable was constructed to indicate family structure: Living with both biological parents, living with mother-only, living with father-only, living with mother and step-parent, living with father and step-parent, or living in some other arrangement. Categorical demographic variables were dummy-coded with male, Caucasian, high parental education, and two biological parent homes as reference groups.

School performance and engagement. Nine variables were used to assess school performance and engagement. GPA was determined by adolescents' response to the question "What best describes your grades?" on an 11-point scale (e.g., "Mostly A's," "Half A's," "Half B's," . . . , "Half D's," "Mostly D's," and "Mostly Below D's"). Higher scores indicate a higher GPA.

Time spent on homework was calculated as the average amount of time spent studying each major subject in school (math, social studies, science, and English; e.g., "How much time do you currently put into homework each week, including reading assignments?"). Responses ranged from 1 = "none," 2 = "15 min," 3 = "30 min," 4 = "an hour," 5 = "2-3 hr," 6 = "4 or more hours." Higher scores indicate more time spent on homework.

Cutting class was assessed by adolescent self-report of unexcused absences in each major subject area (e.g., "How often do you cut [an unexcused absence] math class?"). Individuals responded on a 5-point scale: 1 = "almost every day," 2 = "once or twice a week," 3 = "a few times a month," 4 = "a few times a year," and 5 = "never". Items were reverse coded and averaged across the four major subjects, with higher scores indicating more class-cutting.

Paying attention in class was assessed by averaging adolescent responses to statements about

1 paying attention in each major subject (e.g., "How
2 often to you really pay attention to the classwork
3 during math?") Youth responded on a 5-point scale
4 from 1 ("never") to 5 ("always"). Items were aver-
5 aged across subjects, with higher scores indicating
6 greater attention.

7 *Mind wandering* was measured by adolescent
8 report of how much their mind wanders in class
9 (e.g., "How often does your mind wander in math
10 class?"). Adolescents responded on a 5-point scale
11 from 1 ("never") to 5 ("always"). Items were
12 averaged across the four subjects, with higher
13 scores indicating greater mind wandering during
14 class.

15 *Concentration in class* was measured by self-
16 report of how often an adolescent concentrates in
17 class (e.g., "How often do you concentrate so hard
18 that time passes quickly during math?"). Responses
19 ranged from 1 ("almost every day") to 5 ("never").
20 Responses were reverse coded and averaged across
21 subjects, with higher values indicating greater con-
22 centration.

23 *Effort in school* included items such as "How
24 often do you try as hard as you can in math class?"
25 Responses ranged from 1 ("almost every day") to 5
26 ("never"). Responses were reverse coded and aver-
27 aged across the four subjects, with higher scores
28 indicating greater effort.

29 *Engagement in school* (Wehlege, Rutter, Smith,
30 Lesko, & Fernandez, 1989) was assessed by six
31 items (e.g., "The best way to get through most days
32 at school is to goof off with my friends," "I'm los-
33 ing interesting in school because my teachers keep
34 going over the same things"). Adolescents
35 responded on a 4-point scale from 1 ("agree
36 strongly") to 4 ("disagree strongly"). Responses
37 were averaged, with higher scores indicating
38 greater engagement in school; the scale was shown
39 to have good reliability in the sample ($\alpha = 0.69$).

40 Finally, to assess *educational expectations*, youth
41 were asked, "How far do you expect to go in
42 school?" Options on the 6-point scale were "Leave
43 school as soon as possible," "Finish high school,"
44 "Get some vocational or college training," "Finish a
45 two-year community college degree," "Finish col-
46 lege with a four-year college degree," and "Finish
47 college and take further training". Higher scores
48 indicate higher educational expectations.

49 *Problem behavior.* Responses to three questions
50 about frequency of misconduct in school (e.g.,
51 cheating, copying homework, getting into fights at
52 school) were used to assess *school deviance* ($\alpha = 0.68$;
53 Ruggiero, 1984). Seven items assessed frequency of
54 *nonschool deviance*, such as theft, carrying a weapon,

and vandalism ($\alpha = 0.82$; Gold, 1970). Finally, *sub-
stance use* was assessed as the frequency of use of
different substances, including cigarettes, alcohol,
marijuana, and other illicit drugs (a total of five
items; $\alpha = 0.86$; Greenberger, Steinberg, & Vaux,
1981). For each problem behavior, adolescents
reported if they engaged in the behavior "never,"
"once or twice," "several times," or "often."
Higher scores on each of the variables indicate
greater involvement in problem behavior.

55 *Psychological and somatic symptomatology.* The
Center for Epidemiological Studies–Depressive
Symptomatology Scale (CES–D) was used to assess
internalizing symptomatology. Students were
instructed to rate each of the 13 items with respect
to their experiences in the past month on a 4-point
scale from 1 ("never") to 4 ("three or more times").
Two subscales are calculated: *psychological symptoms*
uses eight items (e.g., "In the past month, how
often have you felt nervous?" "In the past month,
how often have you felt depressed?" $\alpha = 0.88$) and
somatic symptoms uses five items (e.g., "In the past
month, how often have you had headaches?" "In
the past month, how often have you had stomach
aches?" $\alpha = 0.68$). For both subscales, responses
were summed and divided by the number of items;
higher scores indicate greater distress.

56 *Psychosocial development.* First, *self-reliance* was
assessed by the self-reliance subscale of the Psycho-
social Maturity Inventory, which consists of 10
items (e.g., "In a group I let others decide") on a
4-point scale from "strongly disagree" to "strongly
agree" (Form D; Greenberger & Bond, 1976;
 $\alpha = 0.81$). Items are averaged and higher scores
index greater self-reliance. Second, a measure of
self-esteem was adapted for the present study
(Rosenberg, 1965; $\alpha = 0.87$). Youths were presented
with 10 statements (e.g., "I am satisfied," "I am no
good") and asked to rate their agreement with the
statement on a 4-point scale from "strongly agree"
to "strongly disagree." Items are averaged and
higher scores indicate greater self-esteem.

Autonomy from parents. The first measure,
parental monitoring, is a 5-item scale adapted from
Patterson and Stouthamer-Loeber (1984), which
assesses the extent to which an adolescent's parents
are aware of his or her activities (e.g., "How much
do your parents try to know where you go after
school?" $\alpha = 0.79$). Youths respond from 1 ("do not
try") to 3 ("try a lot"), and responses were aver-
aged across items with higher scores indicating
greater parental monitoring of adolescent activities.
Autonomous decision making was assessed with a
13-item measure of the extent to which adolescents,

1 as opposed to parents, make decisions about cur-
2 few, dating, leisure activities, and money manage-
3 ment. Responses ranged on a 3-point scale from "I
4 make decisions without parents" to "parents make
5 decisions without me" ($\alpha = 0.82$; see Dornbusch
6 et al., 1985 and Steinberg, 1987 for examples of sim-
7 ilar measures). The number of times a youth
8 endorsed making the decision without parents was
9 counted, with higher scores indicating greater
10 autonomy in decision making.

13 *Plan of Analysis*

14 Analysis of variance (ANOVA) were used to test
15 for overall differences across our employment
16 groups on the covariates of interest. Notably, at
17 Wave 1, prior to matching across groups with pro-
18 pensity scores, and consistent with the notion that
19 adolescents differentially select into the workplace
20 (e.g., Lee & Staff, 2007), there were a number of
21 differences across groups. With the exception of
22 self-esteem, self-reliance, and school deviance, there
23 were significant differences on every other covari-
24 ate as a function of employment status. Because the
25 groups were already different at Time 1, it was
26 imperative that we account for differential selection
27 into employment, unemployment, or change in
28 employment before examining the effects of
29 working.

30 *Multiple imputation.* In the current sample, the
31 amount of missing data varied across each con-
32 struct from 0.2% to 26%. Traditionally, researchers
33 have utilized a number of strategies to deal with
34 missing data, including listwise deletion, pairwise
35 deletion, dummy variables as indicators of missing
36 data, and nonresponse weighting. In recent years,
37 MI has become an increasingly popular approach
38 to deal with missing data in the social sciences. MI
39 is an analytic technique that replaces missing val-
40 ues with predicted values based on all other infor-
41 mation available in the study (Rubin, 1987; Schafer,
42 1997). In contrast to single imputation techniques,
43 which impute one value for missing data, MI
44 inputs several values for each missing value (with
45 variability because of both sampling error and
46 model uncertainty), creating several completed data
47 sets. Within each data set, a different possibility is
48 imputed for each missing value, reflecting the
49 range of possibilities for that variable (e.g., imputed
50 scores on the deviance variable are limited to a
51 range of 0–5). Subsequently, standard analyses are
52 performed on each completed data set, and effects
53 are aggregated to create one estimate of parameters
54 (Rubin, 1987).

The benefit of MI over other methods is that it
accommodates many different patterns of missing
data and reflects uncertainty about missing values.
Furthermore, in contrast to other techniques for
dealing with missing data, MI is based on more
plausible assumptions. For example, instead of
assuming that any two people are equally likely to
have missing data on a given variable, MI assumes
that two people have the same probability of having
missing data on a given variable *only if* they have
the same values for all other variables (the "missing
at random" assumption; Little & Rubin, 2002).
Given that dropping any data from analyses can
theoretically lead to bias, using MI as an analytic
technique allows for larger sample sizes and also
increases the generalizability of the analyses rela-
tive to other approaches of handling missing data
(e.g., listwise deletion). Thus, we employed MI
using SAS software (Proc MI; SAS Institute Inc.,
2004) to impute missing data both for covariates at
Time 1 and outcomes at Time 2.

Propensity scores. In the present study, 22 cova-
riates were used to create propensity scores: 5 demo-
graphic factors (age, gender, ethnicity, parental
education, and family structure) and the 17 differ-
ent outcome measures assessed at Time 1 (i.e., the
various indicators of academic, behavioral, and
psychosocial functioning). Using logistic regression,
the 22 covariates were used to predict the probabil-
ity of employment status (employed/nonemployed)
at Time 1 for each of the six employment groups
and three control groups; categorical variables such
as gender were specified as such in the model.
Because we use propensity scores in conjunction
with MI, we created propensity scores using each
of the five imputed data sets. Next, we used these
propensity scores to match individuals in the treat-
ment and control groups, again within each
imputed data set. Within the treatment groups (i.e.,
one of the six groups whose employment status
changed), each individual is matched to the indi-
vidual in the control group (i.e., one of the three
groups whose employment status did not change)
with the closest propensity score.

Although there are several different methods of
matching propensity scores, the present study
utilizes two of the most common approaches: *near-
est-neighbor matching with replacement* and *nearest-
neighbor matching without replacement*. Because the
choice of matching strategy can impact the quality
of matches, many researchers utilize more than one
strategy to match data, and examine effects that
are significant across both types of matching (see
Hill, Waldfoegel, Brooks-Gunn, & Han, 2005 for an

example). In matching without replacement, individuals in the control group are only matched to those in the treatment group one time. This technique utilizes as much data as possible from both treatment and control groups, but does so at the risk of not always providing the best overall matches. In contrast, matching with replacement allows for individuals in the control group to be matched more than once. That is, one control case could be matched to multiple treatment cases based on the closeness of their propensity scores. Thus, matching with replacement ensures the closest possible matches (Dehejia & Wahba, 2002), but does so at the cost of reducing sample size (some control cases may not be used at all). For each of our treatment groups, we match members to controls twice: the first time using matching without replacement and the second timing using matching with replacement. This results in treatment and control groups that are balanced with respect to their initial propensity (based on the 22 covariates) to be in either group.

Estimation of employment effects. After matching individuals in the treatment groups to counterparts in the appropriate control group, paired *t*-tests were conducted to examine how change in employment status (i.e., “treatment”) relative to no change in employment status affected school performance and engagement, problem behavior, depressive symptomatology, autonomy from parents, and psychosocial development over the 1-year period. These analyses were repeated using each of the five MI data sets, and effects were aggregated to obtain a final estimate of the impact of each treatment (i.e., each specific type of employment transition) on the outcome variables. We tested the effects separately using each matching strategy. To maximize confidence in our statistical analyses, only effects that were significant at $p < 0.05$ in both types of matching strategies were considered. To the extent

that results are significant across both types of matching, we can be more confident that the effects are robust.

Results

We first tested how well the propensity score matching worked. Recall that prior to MI and propensity score matching, there were a number of significant differences between groups in the covariates. Aggregating effects across our imputed data sets, once individuals had been matched to their nearest neighbor in the appropriate control group, paired *t*-tests found no significant differences across each of our six comparison groups on any of the propensity score covariates, indicating that we successfully balanced preexisting differences in the propensity to belong to a particular employment status transition treatment group (compared with the relevant control group), at least with respect to the 22 covariates used in the analysis. Accordingly, paired *t*-tests were conducted next to test for treatment effects on changes in all outcome variables at Time 2. Because our hypotheses had clear directional predictions, we compared the groups using one-tailed tests of significance if effects were observed using both matching strategies. Finally, we also calculated the effect size (Cohen’s *d*) of each significant effect, using the information from the results that employed matching without replacement.

Impact of Starting to Work During the School Year

We first examined the impact of transitioning to either moderate- or high-intensity employment from nonemployment by comparing outcomes for each of these groups with matched controls who remained nonemployed (see Table 2). Compared

Table 1
Total Number of Matched Controls to Treatment Groups with Matching with Replacement by Imputed Data Set

	NE Time 1/MI Time 2 (N = 356)	NE Time 1/HI Time 2 (N = 107)	MI Time 1/NE Time 2 (N = 140)	MI Time 1/HI Time 2 (N = 99)	HI Time 1/NE Time 2 (N = 38)	HI Time 1/MI Time 2 (N = 49)
Data set 1	280	95	112	84	28	35
Data set 2	281	91	106	81	27	38
Data set 3	279	90	103	84	29	40
Data set 4	285	95	113	83	31	37
Date set 5	270	88	110	82	32	35

Note. NE = nonemployed; MI = employed at moderate intensity; HI = employed at high intensity.

Table 2
Impact of Starting to Work During the School Year

Outcome	Group			
	Moderate intensity ^a , TE (SE)		High intensity ^b , TE (SE)	
	Matching with replacement	Matching without replacement	Matching with replacement	Matching without replacement
GPA	0.82 (0.91)	0.22 (0.94)	-0.95 (1.08)	-0.81 (1.12)
Homework time	-1.12 (1.38)	-1.50 (1.15)	-1.40 (1.79)	-1.06 (1.55)
Skipping school	0.69 (0.85)	1.43 (0.61)	0.86 (0.86)	0.81 (0.85)
Attention in class	-0.87 (0.80)	-0.62 (0.64)	-0.45 (0.98)	-0.49 (0.87)
Mind wander in class	1.37 (1.07)	1.42 (0.69)	0.78 (0.86)	1.04 (0.87)
Concentration in class	-1.44 (1.17)	-0.60 (0.71)	-0.98 (1.24)	-0.78 (0.97)
Effort in school	-0.62 (0.78)	0.27 (0.73)	0.82 (1.17)	0.81 (1.05)
School engagement	0.52 (0.99)	0.26 (0.47)	-1.82 (0.87)**	-1.81 (0.73)**
School expectations	-0.60 (0.87)	-1.34 (0.87)	-1.93 (1.18)**	-1.99 (1.26)**
School deviance	0.47 (1.09)	1.00 (0.68)	0.94 (0.97)	1.20 (0.79)
Deviance	0.05 (0.80)	0.15 (0.46)	2.15 (0.98)**	2.22 (0.71)**
Substance use	0.78 (1.17)	1.32 (0.75)	2.51 (1.06)**	2.32 (0.93)**
Psychological symptoms	-0.03 (0.67)	0.63 (0.78)	-0.24 (0.85)	-0.05 (0.91)
Somatic symptoms	-1.04 (0.66)	-0.60 (0.55)	0.03 (0.87)	0.09 (0.87)
Self-esteem	-0.52 (1.15)	-0.43 (0.45)	-1.34 (0.79)	-1.41 (1.03)
Self-reliance	0.13 (0.90)	0.38 (0.44)	-0.99 (1.00)	-1.02 (1.03)
Parental monitoring	0.71 (0.89)	0.37 (0.60)	0.26 (1.28)	-0.06 (1.08)
Autonomous decision making	0.46 (1.18)	1.14 (1.14)	1.81 (1.83)**	1.70 (1.61)**

Note. Table shows treatment effect (TE) estimates based on paired *t*-tests for the comparison of adolescents who transition to moderate intensity employment compared with youths who remain nonemployed and adolescents who transition to high-intensity employment compared with youths who remain nonemployed. GPA = grade-point-average.

^aIndividuals who became employed at moderate intensity after not working ($N = 356$).

^bIndividuals who became employed at high intensity after not working ($N = 107$).

[†] $p < .10$. * $p < .05$. ** $p < .01$.

with adolescents who remained nonemployed, at Time 2, individuals who were working at moderate intensity were no different in school performance and engagement, problem behavior, depressive symptomatology, autonomy from parents, or psychosocial functioning. In contrast, compared with individuals who remained nonemployed, those who were working at high intensity at Time 2 had lower expectations for educational attainment ($d = 0.30$), lower school engagement ($d = 0.28$), higher levels of substance use ($d = 0.34$), and higher levels of deviance ($d = 0.28$). At Time 2, high-intensity employed youths also reported greater decision-making autonomy than nonemployed youths ($d = 0.25$; see Figure 1). No other contrasts were statistically significant.

Impact of Leaving Work During the School Year

Next, we compared individuals who stopped working between Times 1 and 2 to the matched sample of those who remained employed at

the same level of intensity over this period (see Table 3). Few significant differences emerged. Compared with adolescents who remained employed at moderate intensity over the year, those who stopped working reported paying more attention in school ($d = 0.20$; see Figure 2). And, surprisingly, compared with youths who moved from high-intensity employment to nonemployment, those who remained employed at high intensity reported increases in GPA ($d = 0.42$; see Figure 3). We found no other differences in academic, behavioral, or psychosocial outcomes as a function of leaving the labor force.

Impact of Changing Work Intensity During the School Year

Finally, we examined the effects of changing work intensity during the year, either from moderate intensity to high intensity (increasing hours), or from high intensity to moderate intensity (decreasing hours). Compared with youth who remained

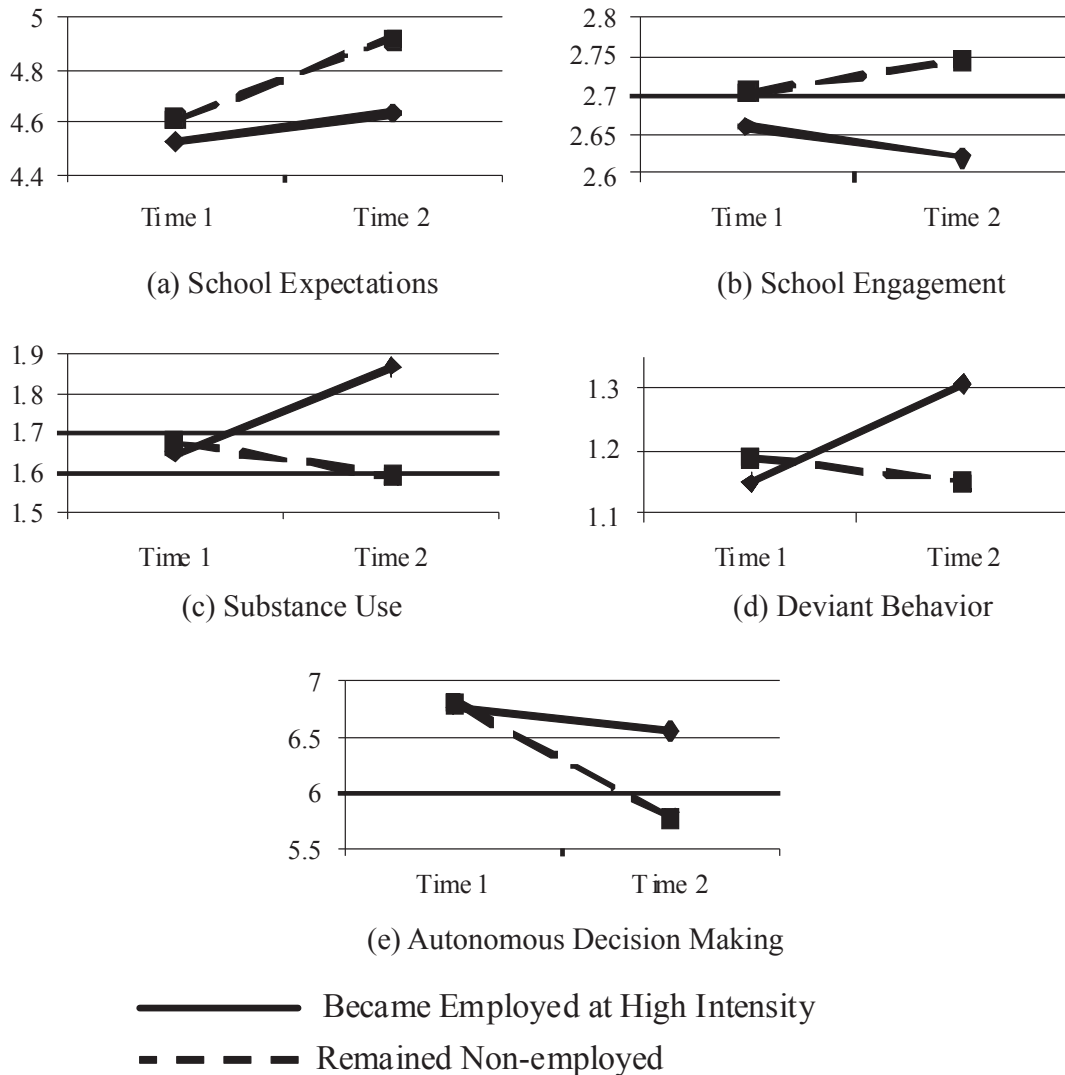


Figure 1. Impact of becoming employed at more than 20 hr per week.

employed at moderate intensity, those who increased their weekly work hours to more than 20 reported a decrease in time spent on homework ($d = 0.35$) and an increase in mind-wandering during class ($d = 0.25$; see Table 4 and Figure 4). Youths who cut back from intensive to moderate levels of employment were no different at Time 2 from youths who remained employed for more than 20 hr weekly.

Discussion

Although social scientists have questioned the impact of intensive employment on adolescent behavior and development, research to date has

yielded conflicting results, some finding negative effects of intensive employment and others finding no effects. Using recent advances in statistical methodology to account for differential selection into employment on 22 different demographic, academic, behavioral, and psychological factors, results of the present study indicate that becoming employed for more than 20 hr a week is associated with declines in school engagement and increases in problem behavior. These findings are particularly salient for nonemployed youth who begin high-intensity employment during the school year compared with youth who remain unemployed, but negative school effects are also apparent for youth who increase their weekly work hours to more than 20 compared with youth who remain

Table 3
Impact of Leaving Work During the School Year

Outcome	Group			
	Left moderate-intensity work ^a , TE (SE)		Left high-intensity work ^b , TE (SE)	
	Matching with replacement	Matching without replacement	Matching with replacement	Matching without replacement
GPA	0.93 (0.80)	0.32 (0.76)	-1.90 (1.39)**	-1.69 (1.13)**
Homework time	-0.62 (2.13)	-0.20 (1.45)	-1.18 (1.41)	-0.95 (1.40)
Skipping school	-1.30 (1.22)	-1.48 (0.87)	-0.10 (1.23)	-0.87 (0.98)
Attention in class	1.74 (1.54)**	2.12 (0.92)**	1.17 (1.36)	0.59 (1.08)
Mind wander in class	-0.53 (1.25)	-0.28 (1.16)	0.89 (1.20)	0.69 (0.98)
Concentration in class	1.01 (0.87)	0.63 (0.91)	0.25 (1.42)	-0.16 (1.12)
Effort in school	-0.08 (1.10)	-0.39 (0.94)	0.04 (1.39)	0.08 (1.16)
School engagement	-0.12 (0.87)	-0.12 (0.74)	>0.01 (1.11)	-0.07 (0.82)
School expectations	-0.96 (1.20)	-0.83 (0.99)	0.41 (1.71)	0.11 (1.28)
School deviance	0.25 (1.08)	0.38 (0.64)	0.01 (1.81)	0.12 (1.02)
Deviance	0.92 (0.77)	0.54 (0.54)	0.55 (1.23)	-0.07 (0.88)
Substance use	-0.63 (1.03)	-0.46 (0.72)	0.18 (1.39)	-0.59 (1.00)
Psychological symptoms	-1.19 (1.17)	-0.65 (0.83)	-1.43 (1.36)	-1.06 (1.22)
Somatic symptoms	-0.41 (1.37)	-0.22 (0.75)	0.78 (1.40)	0.54 (1.23)
Self-esteem	0.76 (0.64)	0.18 (0.71)	0.07 (1.07)	-0.84 (0.75)
Self-reliance	1.28 (1.08)	0.90 (0.88)	0.92 (0.98)	0.23 (0.83)
Parental monitoring	0.11 (1.05)	-0.09 (0.70)	1.26 (1.68)	0.83 (1.06)
Autonomous decision making	0.15 (2.11)	-0.27 (1.58)	-0.29 (2.00)	-0.11 (1.90)

Note. Table shows treatment effect (TE) estimates based on paired *t*-tests for the comparison of adolescents who transition to unemployment from moderate-intensity work compared with youths who remain employed at moderate intensity and adolescents who transition to unemployment from high-intensity work compared with youths who remain employed at high intensity. GPA = grade-point-average.

^aIndividuals who left work after working at moderate intensity ($N = 140$).

^bIndividuals who left work after working at high intensity ($N = 49$).

† $p < .10$. * $p < .05$. ** $p < .01$.

employed at fewer than 20 hr a week. Furthermore, among youth working long hours, there are no positive effects of decreasing work hours or leaving the workforce: Over the course of 1 year, the negative effects of high-intensity employment seen with respect to school engagement and problem behavior remain after an individual has stopped working or cut back work hours. In contrast, and consistent with much prior research, we find virtually no evidence that working 20 or fewer hours per week is associated with negative outcomes (although youths who leave moderate-intensity employment show improvement in school engagement). Thus, an important message for parents and practitioners is that while working more than 20 hr per week during the school year is probably not advisable, working less than this does not seem to affect an adolescents' academic, behavioral, or psychosocial well-being.

The one curious exception to this general pattern of findings is the modest *increase* in GPA (about

one-tenth of a letter grade) among the high-intensity workers who remained employed at more than 20 hr per week over the year. One possible explanation for this is that these workers may have moved from more challenging to easier classes during this time period, an account consistent with these adolescents' reports of a drop in time spent on homework. To test this, we examined responses to an item included in the survey at Time 2, where students were asked how much they agreed with the statement, "I try to take easier classes so that working doesn't hurt my grades." As suspected, individuals working more than 20 hr per week were significantly more likely to endorse this item than were nonworkers ($p < 0.05$, $d = 0.27$). Unfortunately, because this question was only asked at Time 2, we cannot examine whether responses changed over time. Nevertheless, it is reasonable to speculate that the "improvement" in GPA associated with high-intensity employment is because of taking easier classes.

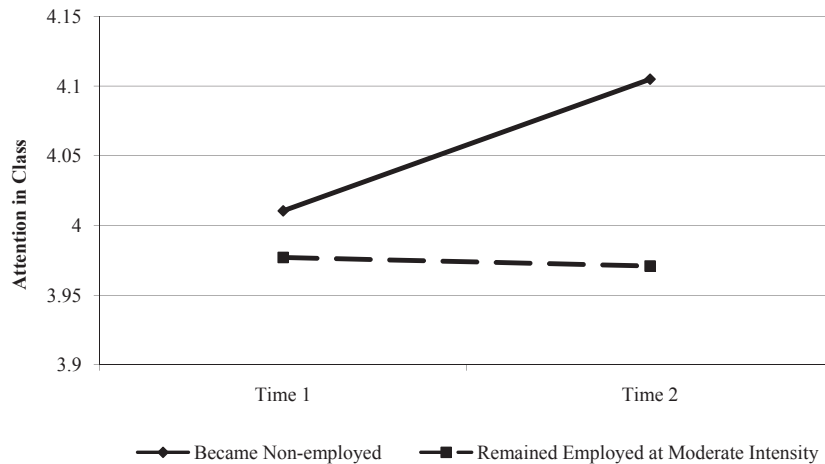


Figure 2. Impact of leaving employment of 20 or fewer hours per week.

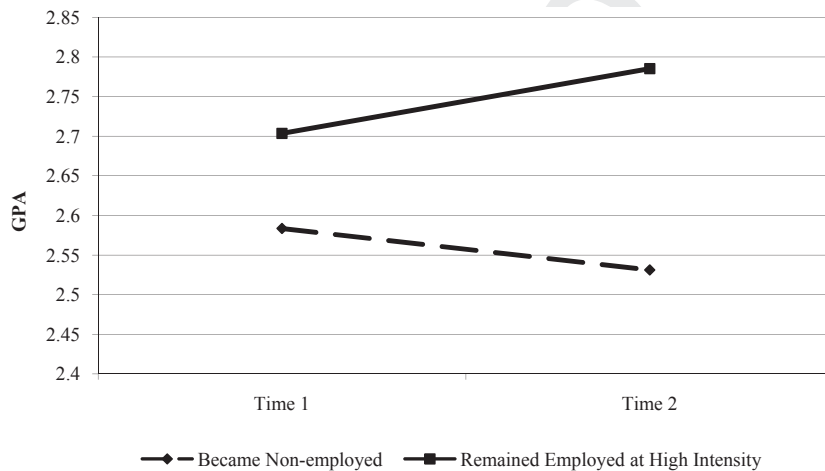


Figure 3. Impact of leaving employment of more than 20 hr per week.

Although the statistical analyses utilized here are arguably better at compensating for differential selection into the workforce, the results of the present study do not differ substantially from the findings originally reported by Steinberg et al. (1993). In keeping with the original study, we find that working long hours has a number of deleterious consequences. There are two primary differences between the findings of Steinberg et al. (1993) and ours, however. First, we find no evidence of diminished self-reliance among youths who become employed at high intensity. Second, Steinberg et al. did not find that working more than 20 hr weekly was associated with any increase in school grades. These two discrepancies may be the result of inadequately accounting for selection effects in the original analyses. Overall, though,

despite the use of very different analytic techniques, the similarity between the two studies is impressive.

Our findings contradict those of several recent studies that indicate no negative effects of high-intensity employment (Apel et al., 2007; Paternoster et al., 2003; Warren et al., 2000). Several differences between the present study and these other ones may account for this disparity, namely, differences in the treatment of missing data, differences in measurement, and differences in sample characteristics. With regard to the issue of missing data, whereas the present study used multiple imputation, these other studies used listwise (Warren et al., 2000) and pairwise (Paternoster et al., 2003) deletion, potentially leading to bias in their results (Apel et al., 2007 do not report how they addressed

Table 4
Impact of Changing Work Intensity During the School Year

Outcome	Group			
	From moderate to high intensity ^a , TE (SE)		From high to moderate intensity ^b , TE (SE)	
	Matching with replacement	Matching without replacement	Matching with replacement	Matching without replacement
GPA	-1.14 (1.20)	-1.22 (0.92)	-0.75 (0.95)	-0.51 (0.90)
Homework time	-2.50 (1.15)**	-2.07 (1.14)**	-0.59 (1.33)	-0.43 (1.41)
Skipping school	-0.08 (1.52)	0.25 (0.96)	-0.57 (1.08)	-0.90 (0.99)
Attention in class	-0.87 (1.10)	-1.01 (0.84)	-0.09 (1.15)	-0.22 (0.90)
Mind wander in class	1.76 (0.93)**	1.74 (0.87)**	0.94 (1.17)	1.06 (1.07)
Concentration in class	1.73 (1.68)**	0.85 (0.94)	1.08 (1.20)	0.49 (1.07)
Effort in school	0.20 (1.65)	-0.16 (1.07)	0.72 (1.06)	0.77 (1.07)
School engagement	0.12 (1.09)	-0.02 (0.80)	-0.36 (0.92)	-0.08 (0.71)
School expectations	-0.86 (1.06)	-1.02 (1.16)	0.39 (1.81)	1.09 (1.22)
School deviance	0.03 (0.98)	-0.08 (0.79)	0.72 (1.01)	-0.22 (0.88)
Deviance	0.10 (1.05)	0.62 (0.67)	1.12 (1.23)	0.11 (0.75)
Substance use	-0.45 (1.11)	-0.10 (1.11)	0.68 (1.11)	-0.32 (0.96)
Psychological symptoms	-0.98 (0.99)	-0.63 (1.06)	-1.48 (1.35)	-0.67 (0.93)
Somatic symptoms	-0.89 (1.08)	-0.41 (0.92)	-0.81 (1.21)	-0.78 (0.94)
Self-esteem	-0.23 (1.07)	-0.33 (0.75)	-0.96 (1.00)	-1.38 (0.74)
Self-reliance	0.03 (0.90)	-0.61 (0.80)	-0.33 (1.40)	-0.32 (0.71)
Parental monitoring	-1.29 (0.82)	-1.26 (0.74)	0.05 (1.09)	0.13 (0.87)
Autonomous decision making	1.31 (1.93)	1.18 (1.63)	-1.19 (1.99)	-0.32 (1.85)

Note. Table shows treatment effect (TE) estimates based on paired *t*-tests comparing adolescents who become employed at high intensity after being employed at moderate intensity compared with youths who remained employed at moderate intensity and adolescents who work at moderate intensity after high-intensity employment compared with youths who remain high-intensity employed. GPA = grade-point-average.

^aIndividuals who became employed at high intensity after working at moderate intensity ($N = 98$).

^bIndividuals who became employed at moderate intensity after working at high intensity ($N = 38$).

† $p < .10$. * $p < .05$. ** $p < .01$.

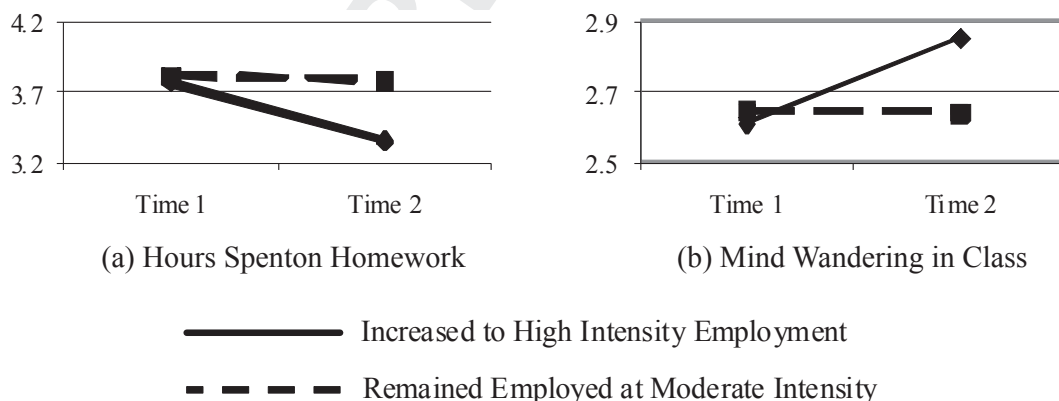


Figure 4. Impact of increasing work hours to more than 20 per week.

missing data). The use of listwise or casewise deletion likely results in a sample that disproportionately excludes a higher number of academically disengaged or delinquent adolescents who are more likely to be absent from school. A selection

bias of this sort reduces sample variability on many of the outcomes of interest, thus making it more difficult to detect effects.

Second, although Apel et al. (2007) and Paternoster et al. (2003) found no effects of high-intensity

1 employment on deviant behavior, both used a mea-
 2 sure of deviance from the National Longitudinal
 3 Study of Youth (NLSY), which includes more seri-
 4 ous types of offending than our index of antisocial
 5 behavior. In the NLSY, the deviant behavior index
 6 includes vandalism, theft (minor and major), car
 7 theft, property crime, aggravated assault, and sell-
 8 ing drugs; in the present study, deviant behavior
 9 was limited to owning a fake ID, taking something
 10 of value from someone, running away, getting into
 11 trouble with the police, bringing a weapon to
 12 school, getting into a fight, and damaging school
 13 property. Thus, it may be that high-intensity
 14 employment is associated with minor delinquency,
 15 but not with more serious crime.

16 Measurement issues may also explain why War-
 17 ren et al. (2000) found no impact of high-intensity
 18 employment on schooling. In that study, the only
 19 academic outcome of interest was GPA; we also
 20 found no effects of employment on GPA. But as
 21 others have argued, GPA is not the only academic
 22 outcome of interest in studies of the effects of work
 23 on schooling, and perhaps not even the most
 24 important one (Steinberg et al., 1993). Warren et al.
 25 did not study school engagement (which, in our
 26 study, declined when adolescents took on high-
 27 intensity jobs compared with youth who remained
 28 unemployed), time spent on homework (which
 29 declined when students increased their work hours
 30 compared with youth who remained employed at
 31 fewer than 20 hr), or mind-wandering during class
 32 (which also increased when work hours went up
 33 compared with youth who remained employed at
 34 fewer than 20 hr). Given the fact that most contem-
 35 porary American high school students report that it
 36 is easy to earn good grades without devoting much
 37 energy to school (Steinberg, 1996), and in view of
 38 the fact that students who work long hours admit
 39 to taking easier classes to protect their grades, limit-
 40 ing academic outcomes to GPA alone may paint a
 41 rosier picture than genuinely is the case.

42 A third possible reason for discrepancies
 43 between our findings and those of other studies
 44 that adequately account for selection effects con-
 45 cerns the samples studied. As noted earlier, Apel
 46 et al. (2007) and Paternoster et al. (2003) both used
 47 the NLSY, a study that oversampled minority
 48 youths; in contrast, the majority of youth in our
 49 sample are White. Given what we know about con-
 50 founds between race and socioeconomic status, in
 51 all likelihood, the socioeconomic background of the
 52 NLSY sample is on average lower than that of the
 53 present sample. Some research suggests that the
 54 impact of employment may be relatively more posi-

55 tive for low-income youth compared with middle-
 56 class youth (Lee & Staff, 2007), and given that our
 results are based on a relatively advantaged sam-
 ple, this possibility warrants further systematic
 study.

Importantly, a frequent assertion made by pro-
 ponents of adolescent employment—that working
 contributes in positive ways to psychosocial devel-
 opment—is not borne out in the present study once
 rigorous controls for selection effects are implemen-
 ted. We see no evidence that employment, either of
 moderate or high intensity, makes adolescents more
 self-reliant or increases their self-esteem. We do
 find that working leads to more permissive deci-
 sion making on the part of parents, but whether
 this increased autonomy in decision making is a
 good thing or a bad thing probably depends on
 whether one asks the adolescents or their parents.

Although the present study is strengthened by
 its use of statistical methodology that adjusts for
 selection effects, it is not without limitation. Indeed,
 one limitation is that propensity score matching is
 only as good as the variables that are included in
 creating the scores (Drake, 1993). We can only be
 certain that our groups are matched on certain
 demographics factors, initial levels of school perfor-
 mance and engagement, problem behavior, depres-
 sive symptomatology, autonomy from parents, and
 psychosocial development; we can make no
 assumptions about their comparability on other
 covariates. However, the fact that groups are
 matched at Time 1 on the very factors measured
 1 year later does give us added confidence in the
 results.

Second, although some of the most important
 findings concern the effects of high-intensity
 employment, fewer than 16% of individuals in the
 current sample were employed at high intensity at
 either time point. This limits our power to detect
 effects of beginning or terminating high-intensity
 employment. Similarly, we were not able to divide
 employment groups into subgroups by propensity
 score (e.g., low, moderate, or high propensity to
 belong to the group), which prevents us from
 drawing conclusions about whether a youths' pro-
 pensity to belong to a particular category further
 affected his or her outcomes 1 year later.

Third, we do not have information on changes in
 employment status that may have occurred
 between the two assessment points. Thus, for
 example, individuals who were unemployed at
 both assessment points may have actually held a
 job during the interim; likewise some youth work-
 ing at both time points may have experienced a

1 period of unemployment between them. Similarly,
 2 we cannot be sure if individuals who remained
 3 employed across time were employed at the same
 4 job at both time points. Indeed, some research sug-
 5 gests that the quality of the work environment can
 6 moderate the effects of long hours on behavioral
 7 outcomes (Mortimer, 2003; Staff et al., 2009); thus, it
 8 is possible that changing from one type of job to
 9 another may have important consequences not
 10 detected in the present research design.

11 Finally, although we can link change in employ-
 12 ment status during the study period with changes
 13 in functioning, we cannot determine with precision
 14 which of these changes occurred first or be certain
 15 that one change caused the other. Consider, for
 16 example, an adolescent who entered the labor force
 17 during the study interval and whose grades
 18 declined during that time. It is possible that the
 19 drop in grades may have been the continuation of a
 20 downward trajectory that was set in motion before
 21 the employment actually began (and before the
 22 adolescent was enrolled in our study). Although
 23 our ability to match this individual with another
 24 who was similarly situated at the beginning of the
 25 study but who did not start working helps rule out
 26 selection effects that can be attributed to initial
 27 characteristics, in the absence of a controlled experi-
 28 ment, we must interpret the present findings with
 29 appropriate caution.

34 Raising Healthy Children: Implications for Policy 35 and Practice

36 Although working during high school is unlikely to
 37 turn law-abiding teenagers into felons or cause stu-
 38 dents to flunk out of school, the magnitude of the
 39 adverse effects reported here is not trivial, and even
 40 a small decline in school engagement or increase in
 41 problem behavior may be of concern to parents
 42 who worry about their children's admission to
 43 highly selective postsecondary institutions or exper-
 44 imentation with deviant activity. Moreover, as Dmi-
 45 trieva, Steinberg, and Belsky (2007) pointed out in a
 46 recent paper on the impact of child care on young
 47 children's achievement, small effects can have big-
 48 ger consequences when they affect a large number
 49 of people at the same time. For example, having a
 50 large number of somewhat disengaged students in
 51 a classroom may be enough to adversely affect
 52 teaching quality, teacher morale, and classroom
 53 climate, a finding that has been reported previously
 54 (McNeil, 1984). In addition, the effects seen here are
 55 those observed over just a 1-year period. The accu-
 56

mulated impact of working at a high-intensity job
 continuously for more than 1 year may be even
 greater.

At the same time, it is important to emphasize
 that moderate-intensity employment, while evi-
 dently not the character-building activity that many
 think it is, is not associated with negative outcomes.
 Taken together, the present findings suggest that
 parents, educators, and policymakers should moni-
 tor and constrain the number of hours adolescents
 work each week during the school year. This was
 precisely the conclusion reached by the National
 Research Council, in its 1998 report, *Protecting
 Youth at Work*, and it is one that finds support in
 the present study. However, we recognize that
 employment may be especially crucial for some
 youth, such as those from families with difficult
 economic circumstances, and that many young peo-
 ple will undoubtedly find it necessary to work dur-
 ing high school. It is therefore important that future
 research explore mechanisms that can minimize the
 potential deleterious consequences of high-intensity
 work hours during the school year.

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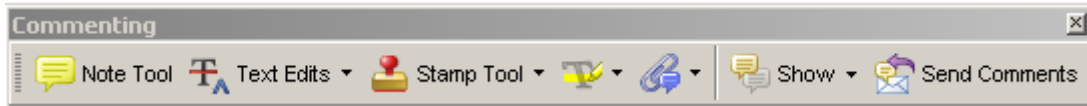
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USING E-ANNOTATION TOOLS FOR ELECTRONIC PROOF CORRECTION

Required Software

Adobe Acrobat Professional or Acrobat Reader (version 7.0 or above) is required to e-annotate PDFs. Acrobat 8 Reader is a free download: <http://www.adobe.com/products/acrobat/readstep2.html>

Once you have Acrobat Reader 8 on your PC and open the proof, you will see the Commenting Toolbar (if it does not appear automatically go to Tools>Commenting>Commenting Toolbar). The Commenting Toolbar looks like this:



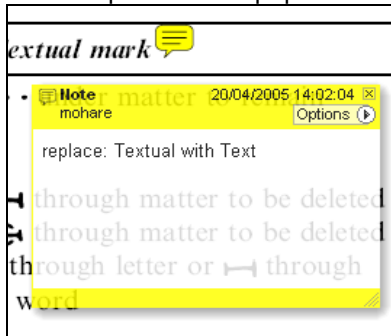
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Note Tool — For making notes at specific points in the text

Marks a point on the paper where a note or question needs to be addressed.

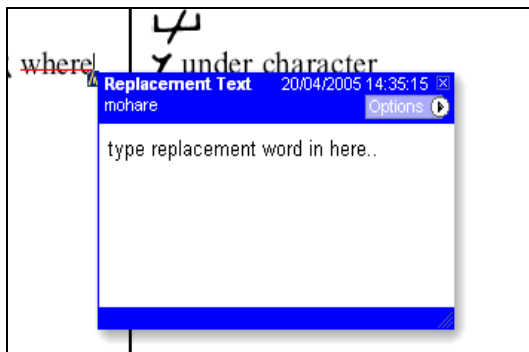


How to use it:

1. Right click into area of either inserted text or relevance to note
2. Select Add Note and a yellow speech bubble symbol and text box will appear
3. Type comment into the text box
4. Click the X in the top right hand corner of the note box to close.

Replacement text tool — For deleting one word/section of text and replacing it

Strikes red line through text and opens up a replacement text box.

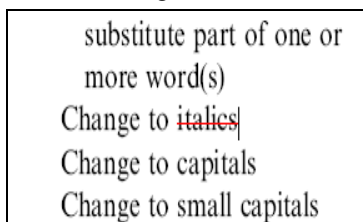


How to use it:

1. Select cursor from toolbar
2. Highlight word or sentence
3. Right click
4. Select Replace Text (Comment) option
5. Type replacement text in blue box
6. Click outside of the blue box to close

Cross out text tool — For deleting text when there is nothing to replace selection

Strikes through text in a red line.



How to use it:

1. Select cursor from toolbar
2. Highlight word or sentence
3. Right click
4. Select Cross Out Text

Approved tool — For approving a proof and that no corrections at all are required.

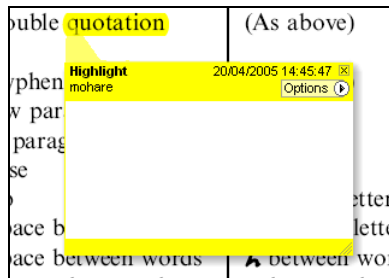


How to use it:

1. Click on the Stamp Tool in the toolbar
2. Select the Approved rubber stamp from the 'standard business' selection
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Highlight tool — For highlighting selection that should be changed to bold or italic.

Highlights text in yellow and opens up a text box.

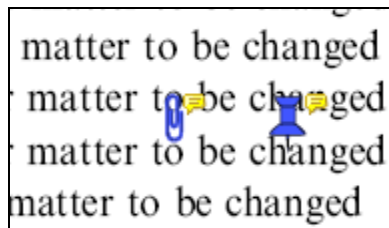


How to use it:

1. Select Highlighter Tool from the commenting toolbar
2. Highlight the desired text
3. Add a note detailing the required change

Attach File Tool — For inserting large amounts of text or replacement figures as a files.

Inserts symbol and speech bubble where a file has been inserted.

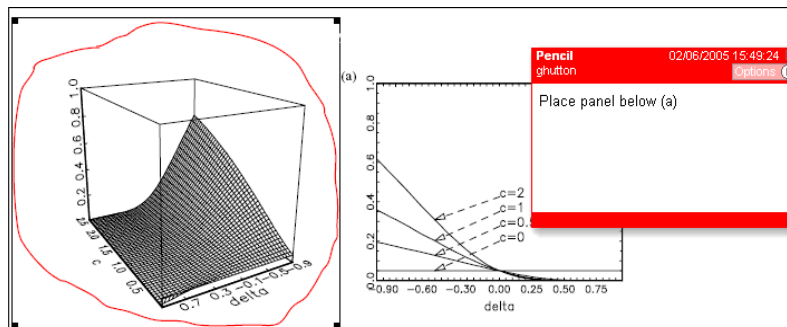


How to use it:

1. Click on paperclip icon in the commenting toolbar
2. Click where you want to insert the attachment
3. Select the saved file from your PC/network
4. Select appearance of icon (paperclip, graph, attachment or tag) and close

Pencil tool — For circling parts of figures or making freeform marks

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How to use it:

1. Select Tools > Drawing Markups > Pencil Tool
2. Draw with the cursor
3. Multiple pieces of pencil annotation can be grouped together
4. Once finished, move the cursor over the shape until an arrowhead appears and right click
5. Select Open Pop-Up Note and type in a details of required change
6. Click the X in the top right hand corner of the note box to close.

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