Essays on the Effects of Youth Labor Supply

by

Isabella Yerby

(Under the Direction of Meghan Skira)

Abstract

This dissertation examines how youth labor supply affects various adolescent outcomes. Using data from the National Longitudinal Study of Adolescent to Adult Health, I investigate the causal effects of adolescent work hours on mental health and educational outcomes. In Chapter 1, I estimate the causal effect of an additional hour worked per week during adolescence on depressive symptoms. Results show that increased hours worked leads to a statistically significant increase in depressive symptoms for females during both the school year and the summer, while there is little evidence of statistically significant effects for males in the aggregate. There is notable effect heterogeneity by parental education and race. The adverse mental health effects for females are larger for those with at least one college-educated parent. White adolescents experience declines in mental health, while Black adolescents see improvements in mental health from an increase in hours worked. Exploration of candidate mechanisms reveals that the effects on mental health may be driven by substitution away from active leisure and sleep along with changes in peer groups. In Chapter 2, I estimate the effects of increased youth labor supply on educational outcomes, such as high school dropout, college enrollment, GPA, and course failure. I find that increased hours worked significantly increase high school dropout and course failure and decrease college enrollment and GPA for both males and females. These effects may be driven in part by changes in school attendance, as well as changes in expectations and preferences regarding higher education. While increased labor supply is most harmful during the academic year, I find that working during the summer also has deleterious effects. Heterogeneity analyses suggest that the harmful effects of increased labor supply are larger for White females than for Black females.

Index words: Labor markets, Labor supply, Employment, Youth, Childhood, Time use, Mental health, Education

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Contents

List of Figures

List of Tables

Chapter 1

The Effects of Labor Supply on Adolescent Mental Health

1.1 Introduction

In 2019, according to data from the Youth Risk Behavior Survey, 37% of US high school students had persistent feelings of sadness, 19% had considered suicide, and 9% had attempted suicide (Bitsko et al., 2022). Poor mental health in adolescence is associated with a range of adverse outcomes. For example, adolescent depression is associated with worse academic outcomes, such as poor test scores, increased high school dropout rates, and decreased tertiary school enrollment (e.g., Fletcher, 2008; 2010; Cornaglia et al., 2015). Poor youth mental health is also associated with poor mental health in adulthood, including increased depressive symptoms, anxiety, and suicidality (Weissman et al., 1999; Fergusson et al., 2007; Johnson et al., 2018), and worse labor market outcomes, such as decreased wages and increased rates of unemployment (e.g., Smith and Smith, 2010; Goodman et al., 2011; Fletcher, 2013; Lundborg et al., 2014). Despite these alarming statistics, much remains to be learned about the determinants of adolescent mental health. Adolescent mental health has worsened over the last decade, especially among females (Daly, 2022), underscoring the importance of studying the underlying causes.

In this paper, I estimate the effects of labor supply on adolescent mental health. Many individuals first enter the labor market during adolescence.[1] In 2023, approximately 33% of those aged 16 to 19 were employed (U.S. Bureau of Labor Statistics, 2023).[2] A priori, the effects of work hours during adolescence on mental health are unclear. For example, additional work hours may be beneficial if they increase self-esteem, independence, or confidence in one's own preparedness for the future. In contrast, if work hours

[1]Most child labor in the US is subject to the Fair Labor Standards Act of 1938 (FLSA) (United States Congress, 1938). The FLSA establishes the minimum age of employment to be 14 (for non-agriculture jobs), restricts hours for those under 16, and limits employment in potentially hazardous occupations. These protections may be superseded by stricter state laws, which may include more stringent restrictions related to hours, permits, types of occupations, etc. Some jobs that are common among youth, such as babysitting or assisting a neighbor with yard work, are not covered by the FLSA.

[2]According to BLS data from 1995, a year covered by my analysis, this employment rate was higher at 44.2% (U.S. Bureau of Labor Statistics, 1995).

increase stress, decrease leisure time, or harm academic performance, then they may be detrimental to mental health. Furthermore, additional work hours increase income and may expose individuals to new peer groups. It is unclear how these changes to income and peer groups may influence mental health. Thus, how work hours impact youth mental health is ultimately an empirical question. It is also a policy-relevant question given that several states have or are considering changing their child labor protections, including restrictions on when work hours can be scheduled and the number of hours worked per week.[3]

I estimate the causal effect of hours worked per week during adolescence on mental health using rich data from the National Longitudinal Study of Adolescent to Adult Health (Add Health) from 1994 to 1996. To measure depressive symptoms, I use the Center for Epidemiologic Studies Depression (CESD) Scale, which is composed of questions related to mental health and well-being. I estimate the effect of work hours separately for students interviewed during the summer versus the school year, using the reported typical number of hours worked in a summer or non-summer week, respectively. A challenge in identifying the causal effects of youth labor supply on mental health is that labor supply is likely correlated with unobservables that also impact mental health. For example, an individual's motivation, social skills, or underlying ability may be correlated with both their labor supply and their mental health status. Therefore, I use a novel control function approach developed by Caetano et al. (2023) to correct for endogenous selection into employment. This method exploits bunching at zero hours of work, where there is a statistically significant discontinuity in the mental health outcome. The main assumption of the method is that the confounders and selection effect are discontinuous at zero hours worked, while the treatment effect is continuous. That is, the discontinuity in mental health is attributed to selection rather than treatment. Therefore, at the bunching point, I can indirectly identify the treatment effect by isolating the effect of the confounders.

After correcting for selection on unobservables using the control function approach, I find that increased hours worked lead to a statistically significant increase in depressive symptoms for females. An additional hour worked per week increases the CESD-19 score by 0.091 points (a 0.72% increase from the sample mean) during the school year and by 0.074 points (a 0.62% increase from the sample mean) during the summer for females. I find little evidence of statistically significant effects for males in the aggregate. The control function approach allows me to investigate effects along important dimensions of heterogeneity, such as parental education, age, race and ethnicity, and urbanicity of the attended school. While the heterogeneity analyses show interesting patterns, I often cannot reject that the effects across groups are the same, though there are exceptions. Females with more-educated parents experience larger adverse effects of work hours on mental health than those with less-educated parents. Additionally, there are notable differences in results by race. For both males and females, White adolescents are more negatively affected

[3] For example, SB345 in New Hampshire, which was enacted in 2022, increased legal working hours for 16- to 17-year-olds from 30 to 35 hours for students enrolled in school and allows youth to work night shifts (New Hampshire General Court, 2022). A4222 in New Jersey increased the allowed hours per week from 40 to 50 hours during the summer (New Jersey State Legislature, 2022). HB49 in Florida allows for their 30-hour-per-week cap during the school year to be waived by the parent or school superintendent (Florida Legislature, 2024). Although less common, a few states have introduced bills to strengthen child labor laws. However, the majority of recent legislation that strengthens child labor protections does not affect the intensive margin, but rather increases penalties for violations of child labor laws (e.g., Alabama Legislature, 2024; Virginia General Assembly, 2024).

than Black adolescents. Notably, an additional hour of work results in improved mental health for Black male adolescents.

I explore additional outcomes that are not only important for human capital development, but that may be related to the observed mental health-labor supply relationship. Specifically, increased work hours result in substitution away from leisure activities such as sleeping, engaging in hobbies, socializing, or playing sports. For females, there is an increase in passive leisure such as television watching. I also find that increased work hours result in an increased probability of smoking and, for females during the school year and males during the summer, increased delinquency. For some subsamples, there is an increase in the probability of binge drinking, as well as having friends who engage in risky behaviors such as smoking and alcohol consumption. Overall, increased labor supply is harmful to the mental health of adolescent females, results in substitution away from active leisure, and increases substance use and the likelihood of having friends who use substances.

This paper contributes to the large body of literature studying the effects of employment on adolescent outcomes, including academic achievement, substance use, delinquency, and time use.[4] The broad consensus is that adolescent labor supply has null or adverse effects on educational outcomes such as grades, test scores, and dropout rates.[5] Some studies find that labor supply during the school year, especially among students who work more intensive hours, has significant harmful effects on grades, test scores, dropout rates, and tertiary school enrollment (Tyler, 2003; Parent, 2006; Apel et al., 2008; Staff et al., 2010). Other work finds no significant effects on these outcomes (Rothstein, 2007; Sabia, 2009; Buscha et al., 2012). Additional research has studied the causal effect of adolescent work on delinquency (e.g., theft, property destruction, violence, substance use, etc.), finding no effect or a small reduction in delinquency (Paternoster et al., 2003; Apel et al., 2008; Lesner et al., 2022). Furthermore, there is evidence of meaningful time-use substitution. For example, Kalenkoski and Pabilonia (2012) find that school-year employment decreases time that students spend on studying and on leisure activities such as television watching.

I contribute to this literature by estimating the causal effects of youth labor supply on mental health, which is a key input into human capital development. There is limited research studying the relationship between youth employment and mental health outcomes.[6] The few studies that exist rely on selection-on-observables strategies, such as propensity score matching, or are purely descriptive, and therefore do not fully account for concerns about selection and endogeneity. For example, Mortimer et al. (2002) study how work quality associates with adolescent mental health. They find that students report higher well-

[4] Neyt et al. (2019) thoroughly reviews the literature that estimates the impacts of working during secondary and post-secondary education. My discussion focuses only on secondary school.

[5] A few papers find small positive effects on educational outcomes. For example, in the Danish context, Lesner et al. (2022) finds causal evidence that increased work hours among students in grade 9 leads to higher exit exam scores and decreased dropout. Other papers find that the effects are non-linear. For example, DeSimone (2006) finds increasing returns on GPA of work among high school seniors, but that these returns diminish for those working more than 15 hours per week. Some papers find mixed impacts on academic outcomes. Lee and Orazem (2010), for example, find that increased working hours decreases tertiary school enrollment but also decreases high school dropout rates.

[6] Some work in the development literature (e.g., Trinh, 2020; Jayawardana et al., 2023) finds harmful effects of child labor on mental health. However, these results are not necessarily generalizable to the effects of youth labor supply in developed countries.

being when they are satisfied with their wages and have funds to socialize, and report being more depressed when they have stressful jobs. Mortimer et al. (1996) find that adolescents who work high intensity jobs do not have worse mental health or self-esteem than unemployed adolescents. Shin et al. (2020) and Avci et al. (2022) study the relationship between extensive margin part-time employment and adolescent mental health in South Korea and Turkey, respectively, and find that part-time work is associated with increased suicidality among high school students. Jo et al. (2015) find that part-time employment is associated with increased suicidality among middle school students in South Korea, but not among high school students. Similarly, Monahan et al. (2011) rely on propensity score matching to examine the relationship between extensive margin part-time employment (20 hours or more per week) and adolescent well-being among a sample of high school students in the US. They find no significant effects on depressive symptoms.

While there is a large literature examining the effects of working while in secondary school, this is one of the first papers to examine the causal effects of adolescent work hours on mental health. Additionally, most of the literature does not separately consider the effects of working during the summer versus the school year.[7] I allow for effect heterogeneity based on whether respondents are interviewed during the academic year or summer, as the trade-offs adolescents face may differ depending on whether school is in session. For example, work hours during the summer may not crowd out study time but may crowd out leisure activities. These differences may manifest in heterogeneous effects of work hours on mental health. While the effects of an hour of work on the average CESD score are not statistically different in the school year versus the summer, the effects on more extreme measures of mental health do differ. Work hours have larger adverse effects on suicidality and the probability of reaching clinically important thresholds in the CESD score for females in the school year compared to those in the summer. Therefore, females face particularly concerning effects of increased work hours during the school year.

In addition to focusing on a policy-relevant and unexplored outcome, I contribute methodologically to the literature on the effects of adolescent labor supply. Studies that estimate the effects of school-year employment largely use instrumental variables (IV), such as local labor market conditions and variation in state child labor laws, to address endogenous selection into work.[8] However, as noted by Buscha et al. (2012), there are reasons to worry that these instruments do not satisfy the exclusion restriction.[9] For

[7]There are a few exceptions. For example, Baert et al. (2022) study the impacts of working during secondary school on tertiary enrollment for students who work only during the summer and students who work during both the summer and school year. They find that working during both the summer and school year decreases the likelihood of tertiary school enrollment but working exclusively in the summer does not. Those who work during both the summer and school year have a higher probability of being employed following secondary school graduation compared to those who work only during the summer. Paternoster et al. (2003) separates the effects of intensive school-year and summer employment on delinquency and problem behaviors, finding no notable differences. Lee and Orazem (2010) consider summer hours as a sensitivity check, although their main focus is on hours worked during the academic year. Oettinger (1999) controls for previous summer employment but does not separately estimate effects using hours during the summer. There is also a strand of literature focused solely on summer employment programs (e.g., Leos-Urbel, 2014; Davis and Heller, 2020; Schwartz et al., 2021).

[8]For example, Tyler (2003) and Apel et al. (2008) exploit variation in state child labor laws that govern how many hours an adolescent can work. Rothstein (2007) uses both variation in state child labor law and other local labor market conditions, such as the local teen unemployment rate and average teen wage rate. Lee and Orazem (2010) exploit various instruments, including interstate variation in truancy laws, birth month of the student, expected age in grade 9, and local earnings.

[9]It is also common within this literature for studies to include individual fixed effects and exploit within-person variation (e.g., Oettinger, 1999; Sabia, 2009) or to include both fixed effects and the aforementioned instrumental variables

example, in my context, there could be concern that stricter child labor laws could be correlated with states prioritizing child mental health and well-being through other state policies, such as through the education system. Therefore, the instrument would be correlated with the outcome through a channel other than hours worked. Furthermore, IV estimates identify the local average treatment effect (LATE) among those induced to work by variation in the instrument. Instead, I use a novel control function approach to provide average treatment effect (ATE) estimates of hours worked. Additionally, this method allows for extensive analysis of heterogeneous treatment effects. As such, I explore effect heterogeneity by the adolescent's age, parental education, race and ethnicity, and school location.

This paper also contributes to the literature investigating the causal relationship between employment and mental health outcomes, which has predominately focused on adults. There is ample evidence that unemployment has harmful effects on adult mental health (e.g., Paul and Moser, 2009; Gathergood, 2013; Schaller and Stevens, 2015; Cygan-Rehm et al., 2017; Picchio and Ubaldi, 2024) and that employment can be protective against poor mental health (Llena-Nozal, 2009; Huber et al., 2011; Schuring et al., 2017). However, employment can have deleterious effects on adult mental health given poor or stressful working conditions, lack of autonomy, or workplace conflict (Plaisier et al., 2007; Cottini and Lucifora, 2013; Maclean et al., 2015; Belloni et al., 2022; Spearing, 2025). However, the effects of working on adult mental health likely do not generalize to the adolescent population. Adolescents have different social expectations, constraints, and time-use trade-offs compared to adults. Evidence suggests peer influence peaks in adolescence, which may alter preferences toward working. Additionally, they may be introduced to new, perhaps older, peer groups through employment that could influence their mental health. Lastly, adolescence is a life stage when individuals are both particularly vulnerable to poor mental health and are developing social and non-cognitive skills. Therefore, adolescents could be differentially affected by work and the accompanying stress compared to adults. This paper provides novel evidence of the causal effect of work hours on mental health for adolescents, a group for whom we know little about this relationship.

The remainder of this chapter is organized as follows. Section 1.2 describes the data. Section 1.3 discusses the empirical strategy. Section 1.4 presents the results for the full sample, heterogeneity analyses, and additional outcomes. Section 1.5 concludes.

1.2 Data

This paper uses the National Longitudinal Study of Adolescent to Adult Health (Add Health), a nationally representative, longitudinal survey that follows 7^{th} to 12^{th} grade students until adulthood from the 1994-95 school year until 2018.[10] Students were selected from 132 schools (80 high schools and 52 middle schools).

(e.g., Rothstein, 2007; Apel et al., 2008). However, many have noted that a fixed effects approach may not sufficiently address endogeneity in the academic achievement context, given that many confounders are unlikely to be time invariant (e.g., Oettinger, 1999; Lesner et al., 2022). Similar concerns apply to my context.

[10]This research uses data from Add Health, funded by grant P01 HD31921 (Harris) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD), with cooperative funding from 23 other federal agencies and foundations. Add Health is currently directed by Robert A. Hummer and funded by the National Institute on Aging cooperative agreements U01 AG071448 (Hummer) and U01AG071450 (Aiello and Hummer) at the University of North

Over 90,000 students were randomly selected from these schools to complete an in-school survey and a subset of 20,745 of these students were randomly selected to participate in an in-home survey. I use the first two waves of the in-home survey, which cover the 1994-95 and 1995-96 school years when the students are in 7^{th} to 12^{th} grade, to gather information about outcomes during secondary school. This data includes detailed demographic, social, familial, and health information. I treat this data as a repeated cross-section.

To create my sample, I include those aged 14-18 years old who report that they are either currently in school or, if answering during the summer, were in school during the previous school year. I use four subsamples to conduct my analysis: females during school months, females during summer months, males during school months, and males during summer months. I define an individual as being in school if they report that school is currently in session during a non-summer month.[11] I define an individual as being in the summer season if they report that school is not in session during the months of May to August.[12] I also require that all observations have non-missing information for all components used to construct the main mental health measure, as well as non-missing data on hours worked and employment. After making these restrictions, I have an analytical sample of approximately 25,000 observations.

1.2.1 Outcome Variables

I use the Center for Epidemiologic Studies Depression Scale (CESD) to measure depressive symptoms. The CESD is made up of a series of questions related to mental health and well-being within the last week.[13] Historically, the CESD score is based upon a set of 20 questions, however, not all 20 questions are available in Add Health. Therefore, I use the CESD-19 that is based upon 19 questions, which is commonly used in the literature (e.g., Rushton et al., 2002; Goodman et al., 2003; Fletcher, 2010; Joyce and Early, 2014). CESD questions are answered on a scale from 0 to 3, according to how often respondents report that they experience a given symptom. Possible answers are (0) "never or rarely", (1) "sometimes", (2) "a lot of the time", or (3) "for most of the time or all of the time". The CESD-19 score is constructed by summing together all components, creating a scale from 0 to 57. A higher CESD-19 score represents a higher level of depressive symptoms.

I investigate additional mental health outcomes for robustness. Throughout most of my analysis, I use the CESD-19 score ranging from 0 to 57; however, to capture clinically important moments along the CESD score distribution I use the cutoffs defined by Radloff (1977) and Roberts et al. (1991) that represent increased likelihood for clinical depression. That is, I create dummy variables for a CESD-19

Carolina at Chapel Hill. Add Health was designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill. For more information about the Add Health design, see Harris et al. (2019).

[11] I exclude individuals who answer that school is in-session during July to avoid capturing summer school. As some schools are likely in-session during June and August, I include individuals in the school-year sample during these months if they report that school is currently in-session.

[12] Individuals who reported school not being in session outside of these months were not included in the summer sample, as they capture other breaks, such as winter or spring breaks. To define the summer, I use August as a cut off, as many schools return after Labor Day in early September.

[13] See Table A1 for a full list of questions used to construct the CESD-19 score and Figure A1 for the distribution of the CESD-19 for each subsample.

score of 16 or higher to signify a high likelihood of at least mild depression and a cutoff of 24 or higher for a high likelihood of severe depression.[14] I also include a five-item version of the CESD, the CESD-5, which focuses on questions directly related to depressive mood. Lastly, I include indicators for whether the respondent has contemplated or attempted suicide in the last year.

Finally, I explore related outcomes such as time use, risky behaviors, and delinquency. For time use, I study the reported hours spent watching television and sleeping in the last week. I also examine chores, hobbies, socializing, exercising, and engaging in active sports, which are measured categorically rather than in hours. Therefore, I consider two versions of these outcomes. First, is whether respondents report doing the activity in the last week. Second, is a dummy variable for whether they report engaging in that activity more than twice a week. For risky behaviors, I examine alcohol and binge drinking in the last year, smoking in the last month, and whether respondents have friends who engage in substance use. Lastly, I explore a delinquency scale which is a sum of 15 questions related to delinquent behaviors in the last year.[15]

1.2.2 Treatment Variables

I measure hours worked using the reported number of hours worked in a typical summer or non-summer week. However, these questions do not refer to any specific reference period, while the questions used to construct the CESD-19 score reference the past week. To make the reference periods for the employment and CESD questions more comparable, I condition the hours worked variable on the question "In the last 4 weeks, did you work for pay for anyone outside of your home?" That is, I create a new hours worked variable that uses the reported typical hours per week if they reported "yes" to participating in paid employment in the last 4 weeks. If they answered "no" to working in the last 4 weeks, I record their hours worked as zero.[16] The goal is to decrease the measurement error caused by the mismatched reference periods. I assume that a respondent's reported typical number of hours worked reflects the number of hours they worked in the last 4 weeks.

Table 1.1 shows descriptive statistics for the final sample. On average, females report more depressive symptoms than males, and higher likelihoods of contemplating and attempting suicide as well as reaching clinically significant thresholds for depression. This is especially stark during the school year. Additionally, females report lower average working hours, both during the school year and summer. There are no notable differences between subsamples in terms of average observable characteristics.

[14] To be conservative, I apply the cutoff of 24 to both males and females, although 22 is the cutoff for clinical depression established by Roberts et al. (1991) for males. Additionally, these cutoffs are designed for the 20-item CESD, not the 19-item CESD, therefore they are slightly stricter when applied to the CESD-19.

[15] See Table A2 for a full list of the 15 questions included in the delinquency scale.

[16] This change affects approximately 7% of both female subsamples and 9% and 10% of the school-year and summer male subsamples, respectively. That is, these individuals are assigned zero hours worked for the analysis. Results using reported hours that are not conditioned on recent employment are similar to those shown in Table 1.2, albeit noisier.

Table 1.1. Descriptive Statistics across Samples

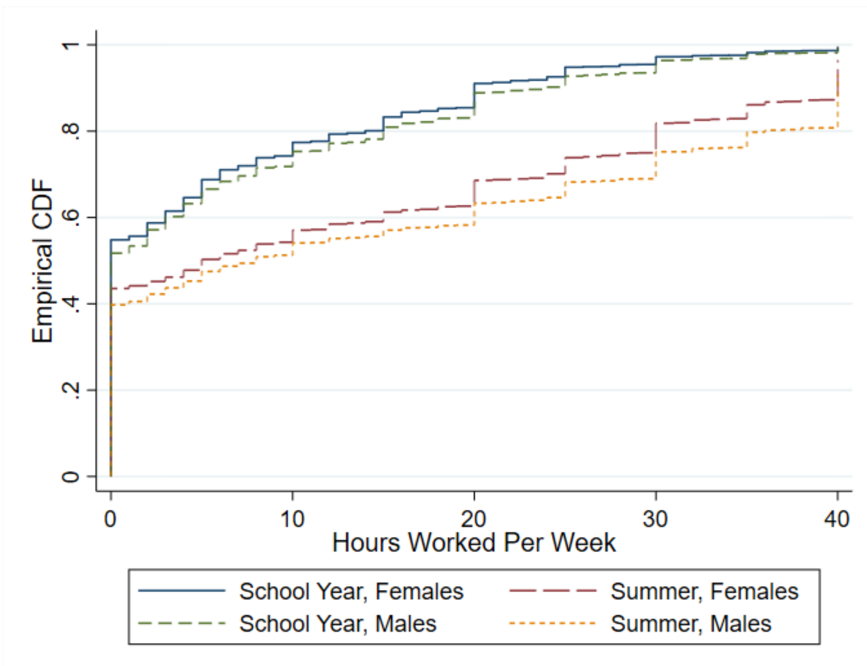
	Females, School Year		Males, School Year		Females, Summer		Males, Summer	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Mental Health Outcomes								
CESD-19 (0-57)	12.66	8.11	10.65	6.79	11.85	8.12	9.93	6.67
Contemplated suicide	0.15	0.36	0.09	0.29	0.15	0.36	0.09	0.29
Attempted suicide	0.05	0.22	0.02	0.15	0.05	0.22	0.02	0.14
CESD-5 (0-15)	3.03	2.76	2.30	2.21	2.83	2.76	2.16	2.20
CESD-19$\geq$16	0.32	0.47	0.21	0.41	0.28	0.45	0.18	0.38
CESD-19$\geq$24	0.10	0.30	0.05	0.21	0.09	0.29	0.04	0.21
Hours Worked & Bunching								
Works zero hours ($H = 0$)	0.55	0.50	0.52	0.50	0.43	0.50	0.39	0.49
School year hours worked ($H \geq 0$)	6.18	9.74	7.00	10.78	7.54	11.30	8.75	12.64
Summer hours worked ($H \geq 0$)	10.21	15.71	12.66	17.96	14.21	16.80	16.80	18.79
School year hours worked ($H > 0$)	13.68	10.37	14.47	11.50	15.97	11.66	17.85	12.78
Summer hours worked ($H > 0$)	25.05	15.30	27.33	17.19	25.06	15.03	27.64	16.77
Control Variables								
Age (years)	15.95	1.28	16.05	1.29	16.02	1.33	16.08	1.33
Hispanic	0.19	0.39	0.20	0.40	0.13	0.34	0.13	0.34
Race								
White	0.60	0.49	0.61	0.49	0.64	0.48	0.66	0.47
Black	0.23	0.42	0.21	0.41	0.23	0.42	0.20	0.40
Other	0.17	0.37	0.18	0.38	0.13	0.34	0.14	0.34
Maternal Education								
No degree	0.18	0.39	0.15	0.35	0.13	0.34	0.11	0.32
High school	0.32	0.47	0.33	0.47	0.32	0.47	0.33	0.47
Some college	0.19	0.39	0.17	0.38	0.19	0.39	0.17	0.38
College	0.24	0.43	0.26	0.44	0.26	0.44	0.29	0.45
Missing	0.07	0.25	0.09	0.29	0.09	0.28	0.10	0.30
Paternal Education								
No degree	0.12	0.33	0.11	0.32	0.09	0.29	0.10	0.29
High school	0.23	0.42	0.24	0.43	0.23	0.42	0.23	0.42
Some college	0.12	0.33	0.13	0.34	0.12	0.33	0.13	0.34
College	0.20	0.40	0.23	0.42	0.23	0.42	0.25	0.43
Missing	0.33	0.47	0.28	0.45	0.33	0.47	0.29	0.45
Grade								
7^{th} grade	0.01	0.08	0.01	0.11	0.02	0.14	0.02	0.15
8^{th} grade	0.11	0.32	0.12	0.32	0.14	0.34	0.15	0.35
9^{th} grade	0.20	0.40	0.19	0.39	0.20	0.40	0.21	0.41
10^{th} grade	0.24	0.43	0.23	0.42	0.22	0.41	0.23	0.42
11^{th} grade	0.24	0.43	0.25	0.43	0.22	0.41	0.22	0.41
12^{th} grade	0.20	0.40	0.19	0.39	0.20	0.40	0.17	0.38
Has siblings	0.79	0.41	0.78	0.41	0.79	0.41	0.81	0.40
Attends rural school	0.16	0.37	0.17	0.38	0.18	0.38	0.18	0.38
Observations	6302		6217		6670		6371	

Note: The table shows descriptive statistics for the sample of person-wave observations, based upon Add Health waves 1 and 2. Excluding age, control variables are indicators.

1.3 Empirical Strategy

I use the control function approach of Caetano et al. (2023), which takes advantage of bunching at zero hours worked to correct for endogeneity and to estimate the average treatment effect of hours worked. The arguments and explanations below follow those of Caetano et al. (2023). Figure 1.1 shows the cumulative density function of hours worked per week, the treatment variable, for each of my subsamples. There is substantial bunching present at zero hours worked per week for each group, ranging from approximately 39% for males during the summer to 54% for females during the school year.

Figure 1.1. Empirical CDF of Hours Worked per Week



Note: The figure shows the estimated CDF of hours worked per week, by gender and school versus summer months.

Following Caetano et al. (2023), I assume that there are different types of adolescents represented at the bunching point. There are individuals who are exactly indifferent toward working and there are individuals who are averse to working (i.e., far from indifference). To formalize this concept, I introduce a selection variable, H^*, in addition to the treatment variable, H. H is the number of hours worked that is observable in the data and H^* is an unobservable index of confounders that influence an individual's choice of hours worked per week.

Unlike H, which has a natural lower bound at zero, there is no non-negativity constraint for H^*. Importantly, there will be individuals who have a negative value of H^*. We can consider H^* to be a measure of indifference between working and not working, such that those with a more negative value have characteristics that result in being more averse to working. One can also refer to H^* as the "type" of the individual, as it represents the amalgamation of characteristics that lead to them choosing H^* hours per week. H^* reflects a combination of factors, such as individual preferences, legal constraints, parental and social influences, costs and benefits, etc. that would lead to a certain number of hours worked being chosen. For those who work positive hours, I assume that their type is $H^* = H$, such that their observed choice of hours worked reveals their underlying type. However, the non-negativity constraint of H results in two groups of individuals represented at $H = 0$, such that I cannot assume that their type is $H^* = 0$. There are those who are truly indifferent between not working and working a small number of hours, such that their type is $H^* = 0$. That is, they are at an interior solution. However, there are also individuals at a corner solution at $H = 0$ so that the non-negativity constraint is binding and their type is instead some value $H^* < 0$. These individuals are of varying distances away from indifference between working and not working, such that they have preferences against working. However, they cannot choose to work negative hours and must instead work $H = 0$. As a result, there are individuals of numerous types represented at $H = 0$.

I formalize the relationship between the treatment variable H and the selection variable H^* as:

$$\begin{aligned} H &= max\{0, H^*\}, \text{ where } 0 < \mathbb{P}(H^* < 0) < 1 \quad (1.1) \\ &= H^* \cdot \mathbb{1}(H^* \geq 0), \text{ with } \mathbb{P}(H^* < 0) > 0. \quad (1.2) \end{aligned}$$

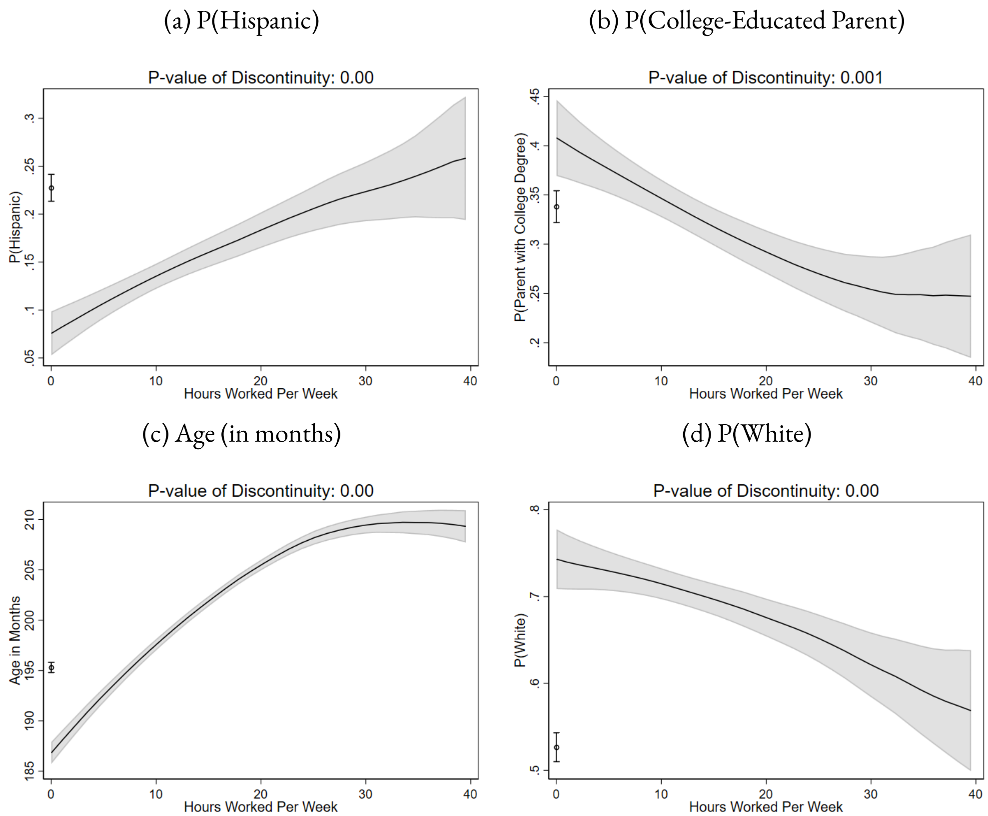
The condition that $0 < \mathbb{P}(H^* < 0) < 1$ requires that there exists some individuals of type $H^* < 0$ at $H = 0$ that are at a corner solution.

I provide evidence of the existence of $H^* < 0$ types present at $H = 0$ by showing discontinuities in values of observables, comparing those at $H = 0$ to those at $H > 0$. We could reasonably assume that individuals of similar types H^* are similar in their characteristics. For example, someone who works 11 hours per week is not particularly different from someone who works 12 hours per week. However, individuals who work zero hours are not necessarily similar to individuals who work a small number of hours, because those at zero hours may not be at an interior solution. For example, Figure 1.2 shows that females during the school year at $H = 0$ vary discontinuously in the value of observables compared to those who work positive hours.[17] On average, those at $H = 0$ are discontinuously different from those at $H > 0$ in terms of age, race, ethnicity, and the probability of having college-educated parents. If everyone at $H = 0$ were of type $H^* = 0$, who are not much different from type $H^* = 1$, there would be no reason for such a discontinuity to exist. Instead, the individuals who are bunched at $H = 0$ are fundamentally different than those working a positive number of hours, supporting the existence of individuals who are at a corner solution. That is, the average type H^* will be discontinuous at $H = 0$ because of the existence of $H^* < 0$ types.[18]

[17] Figures A2 to A4 show these graphs for the other three subsamples.

[18] We can write this discontinuity formally as: $\Delta_{h^*} = \lim_{h\to 0} \mathbb{E}[H^*|H = h] - \mathbb{E}[H^*|H = 0] = 0 - \mathbb{E}[H^*|H = 0] = -\mathbb{E}[H^*|H = 0] \neq 0$. That is, as individuals work fewer hours, their average type approaches 0, specifically $H^* = 0$. The remaining discontinuity, -$\mathbb{E}[H^*|H = 0]$, can be interpreted as the average distance from indifference to working.

Figure 1.2. Evidence of $H^* < 0$ Types (School Year, Female Subsample)



Note: Each panel shows the local linear estimate of the expected value of the variable conditional on H, as well as the expected value of the variable for those at $H = 0$. 95% confidence intervals shown in gray. The *p*-value is for a test for whether there is a discontinuity at zero. The sample includes females during the school year. Bandwidth=10.

In addition to the discontinuities in the observable characteristics, there is also a discontinuity in the outcome variable, the CESD-19 score, at $H = 0$. One possible cause is a discontinuity in the treatment effect. However, it is unlikely that going from 0 hours to just 1 hour per week would result in such a stark difference in mental health. Instead, I posit that the discontinuity in the outcome arises from a discontinuity in the confounders, H^*, as a result of the $H^* < 0$ types at a corner solution at $H = 0$. Figure 1.3 displays the discontinuities before and after controlling for observables for females during the school year. Even after controlling for observables, there is still a notable discontinuity in the expected value of the CESD-19 score at $H = 0$.[19] Figures A5 to A7 show the same graphs for the other subsamples. The right panel displays the results of the discontinuity test of Caetano (2015), which tests for whether the selection effect is equal to zero.[20] Figure 1.3 and Figure A6 provide evidence that there is remaining selection on unobservables for the female subsamples and that individuals at $H = 0$ vary discontinuously on average

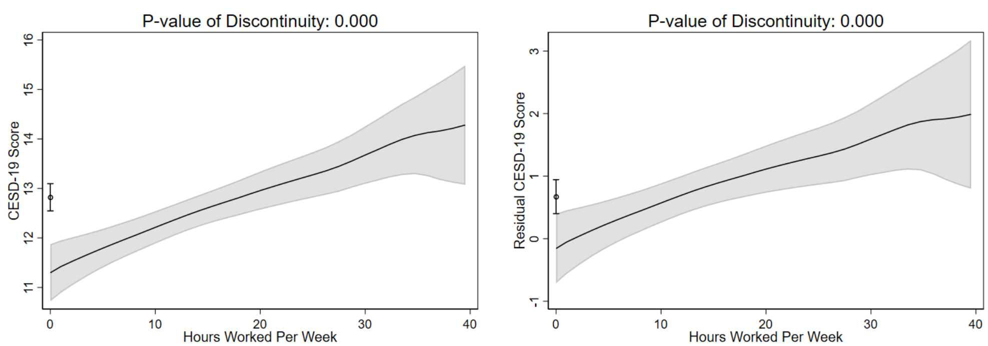
[19] For smoothness, I use a bandwidth of 10. Changing the bandwidth does not alter whether the discontinuity is significant or not, as the *p*-value is based upon the discontinuity at zero.

[20] As shown in Caetano et al. (2023), this corresponds to a test for whether the average residual at $H = 0$ is equal to zero.

in the value of the unobserved confounders (i.e., H^*) compared to those who work a marginally positive number of hours per week. On the other hand, based on Figure A5 and Figure A7 for males, I cannot reject that there is no remaining endogeneity. For consistency, I apply the control function approach, but the correction for unobservable confounders is small for males and has no effect on the estimates.

In addition to showcasing remaining endogeneity, these figures also provide insight into the direction of selection. We can see in the right panels of Figure 1.3 and Figure A6 that female adolescents during the school year and summer who work are negatively selected on unobservables. For males, in Figure A5 and Figure A7, there is no evidence of remaining selection on unobservables. In addition to uncovering the direction of selection, we also already have a good sense of the sign of the causal effect. The slope of the line in the right panels of Figure 1.3 and Figure A6 is equal to the treatment effect plus the selection effect. Assuming the selection effect for $H > 0$ has the same sign as the selection effect for $H = 0$, we know that the selection effect for females is negative. Therefore, we can retrieve the treatment effect by subtracting the negative selection effect from the slope, which will result in a positive treatment effect. That is, we already know that increased work hours are harmful to the mental health of females.

Figure 1.3. Evidence of Selection at $H = 0$ (School Year, Female Subsample)



Note: The left panel displays the local linear estimate of the expected value of the CESD-19 score conditional on the hours worked per week, estimated for $H > 0$, and the expected CESD-19 score for those who do not work ($H = 0$). The *p*-value shown is for a test for whether there is a discontinuity at zero. The right panel shows the local linear estimate of the average CESD-19 score after controlling for observables (race, age, parental education, siblings, Hispanic origin, attending a rural school, and wave). The *p*-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. Bandwidth=10. The sample includes females during the school year.

1.3.1 Control Function Approach

Under the assumption that the treatment effect is continuous at zero, I can exploit the discontinuity in the CESD-19 score at the bunching point to estimate the effect of confounders at $H = 0$ such that it is separate from the treatment effect. I write the relationship between the CESD-19 score and hours worked as:

$$Y = \alpha + \beta H + U, \tag{1.3}$$

where Y is the CESD-19 score, β is the average treatment effect of hours worked on the CESD-19 score, and U is the error term.[21] I assume that U varies linearly with H^*, the index of individual types or confounders. Because I exploit the bunching point at $H = 0$, I only estimate the selection effect at $H = 0$. Therefore, I assume that the selection effect of H^* is the same for adolescents who work positive hours as for those who work zero hours, such that:

$$\mathbb{E}[U|H^*] = \delta H^*. \tag{1.4}$$

Using Equation 1.4, I can expand Equation 1.3 to include both H and H^* as

$$Y = \alpha + \beta H + \delta H^* + \epsilon, \quad E[\epsilon|H, H^*] = 0. \tag{1.5}$$

Therefore, I can write the outcome in terms of the treatment effect β and the selection effect δ. By definition, H^* is unobserved, but I can use a control function to proxy for it so that I can estimate δ. Based on Equation 1.1 and Equation 1.2, I can write H^* as $H^* = H + H^* \cdot \mathbb{1}(H = 0)$. That is, for individuals working a positive number of hours, their H^* is their realized number of hours worked per week, H. Determining H^* for those at $H = 0$ is more complicated. Substituting into Equation 1.5 and taking the expectation of Y conditioned on H, I get:

$$\mathbb{E}[Y|H] = \alpha + \beta H + \delta(H + \mathbb{E}[H^*|H = 0] \cdot \mathbb{1}(H = 0)). \tag{1.6}$$

I can now separate the treatment and selection effects. To see this more clearly, I can rearrange Equation 1.6 as:

$$\mathbb{E}[Y|H] = \alpha + (\beta + \delta)H + \delta(\mathbb{E}[H^*|H = 0] \cdot \mathbb{1}(H = 0)). \tag{1.7}$$

Assuming I can estimate $\mathbb{E}[H^*|H = 0]$, which we can think of as the average distance from indifference at $H = 0$, then I can use $H + \mathbb{E}[H^*|H = 0] \cdot \mathbb{1}(H = 0)$ as a control function to indirectly identify β by estimating δ.[22] However, $\mathbb{E}[H^*|H = 0]$ is unobservable. Therefore, I must make assumptions about the distribution of H^*. I provide quantitative results assuming that the distribution of H^*

[21] I illustrate the empirical strategy without the use of covariates; however, covariates can be added to relax the assumptions of the model.

[22] Note that δ is the ratio of the discontinuity in the outcome, $-\delta\mathbb{E}[H^*|H = 0]$, and the discontinuity in H^*, $\mathbb{E}[H^*|H = 0]$.

follows a semiparametric tobit distribution.[23] However, as discussed later and shown in Figure 1.4, I can draw qualitative conclusions by guessing values of $\mathbb{E}[H^*|H = 0] < 0$. That is, by guessing values, I can identify β without having to choose a distribution. Essentially, the results are robust to misspecification of the control function. Therefore, I present point estimates using the semiparametric tobit distribution for concreteness, but this distributional assumption is not necessary to draw robust qualitative conclusions.

Having established the basics of the model, I now add in controls Z, which includes sex, age-in-quarters dummies, race dummies, parental education dummies, grade dummies, an indicator for having siblings, being of Hispanic origin, attending a rural school, and survey wave. I estimate the following model:

$$\mathbb{E}[Y|H, Z] = \beta H + Z'\tau + \delta[H + E[H^*|H = 0]\mathbb{1}(H = 0)], \tag{1.8}$$

where H is actual hours worked per week, H^* is the individual's "type", and Z is a vector of observed characteristics. β is the average treatment effect of hours worked and δ is the selection effect of the confounder H^*. $H + E[H^*|H = 0]\mathbb{1}(H = 0)]$ is the control function that allows me to control for the individual's type, H^*.

1.4 Results

1.4.1 Main Results

In this section, I present point estimates of β, which is the effect of an additional hour of work per week on the CESD-19 score. Recall that a higher CESD-19 score is associated with worse mental health. Columns (1) and (2) in Table 1.2 show the relationship between hours worked per week and the CESD-19 score without using the control function approach. An additional hour of work per week is associated with worse mental health during the school year, albeit the estimate for males is noisy. For males and females during summer, an additional hour of work is associated with small, insignificant improvements in mental health.

Column (3) in Table 1.2 shows the effect of an additional hour of work per week on the CESD-19 score, assuming a semiparametric tobit distribution of H^*, using the control function approach.[24] Females, both in the school year and summer, face significant harmful effects to their mental health from an additional hour worked per week. For females during the school year, an additional hour of work per week results in

[23] Note that other works using this methodology consider three distributional assumptions: semiparametric uniform, semiparametric tobit, and non-parametric tail symmetry (Caetano et al., 2023). Each distribution yields different values of $\mathbb{E}$, which will lead to different point estimates of β. Previous work (e.g., Caetano et al., 2024a; 2024b) present results largely using the nonparametric tail symmetry distribution. I deviate from this approach, as the nonparametric tail symmetry assumption not only provides the least-conservative point estimates in my application, but because I have bunching above 50%, which violates the requirements of the estimation process to obtain accurate measures of $\mathbb{E}[H^*|H = 0]$ using the nonparametric tail symmetry distribution. However, to be consistent with the literature, I show results using all three distributions in Table A3.

[24] For consistency with other work using this method, Table A3 shows additional estimates assuming a semiparametric uniform or nonparametric tail symmetry distribution for H^*. Qualitative results hold regardless of the distributional assumption.

a 0.0908 point increase in the CESD-19 score from a mean of 12.66. During the summer, females still face harmful effects to their mental health, with a 0.0737 increase in the CESD-19 score from a mean of 11.82. While the effects are smaller in the summer, I cannot reject equality of the effects in the school year and summer. There is no evidence of statistically significant effects on depressive symptoms for males. Albeit imprecisely estimated, for males during the summer, there is some evidence of beneficial mental health impacts from increased working hours.

The table also shows estimates of δ, the average effect of H^* on the CESD-19 score, which provides insight into the direction of selection. In accordance with the discussion in Section 1.3, δ is negative and significant for females, meaning that those who work are positively selected on mental health (i.e., they have fewer depressive symptoms). For males, the estimates of δ are insignificant and close to zero.[25]

[25] This is consistent with Figure A5 and Figure A7, where there is no evidence of significant selection for males.

Table 1.2. The Effect of Working Hours on Depressive Symptoms (CESD-19)

	Uncorrected no Controls (1)	Uncorrected w/ Controls (2)	Corrected w/ Controls (3)
Panel A: Female, School Months (Mean=12.66, N=6296)			
β	0.0262**	0.0243**	0.0908***
	(0.0106)	(0.0115)	(0.0317)
δ			-0.0429**
			(0.0189)
Panel B: Female, Summer Months (Mean=11.82, N=6596)			
β	-0.0124**	-0.0036	0.0737***
	(0.0060)	(0.0067)	(0.0243)
δ			-0.0538***
			(0.0164)
Panel C: Male, School Months (Mean=10.65, N=6201)			
β	0.0124	0.0037	0.0097
	(0.0084)	(0.0089)	(0.0259)
δ			-0.0040
			(0.0163)
Panel D: Male, Summer Months (Mean=9.93, N=6259)			
β	-0.0001	-0.0078	-0.0138
	(0.0047)	(0.0051)	(0.0195)
δ			0.0043
			(0.0136)

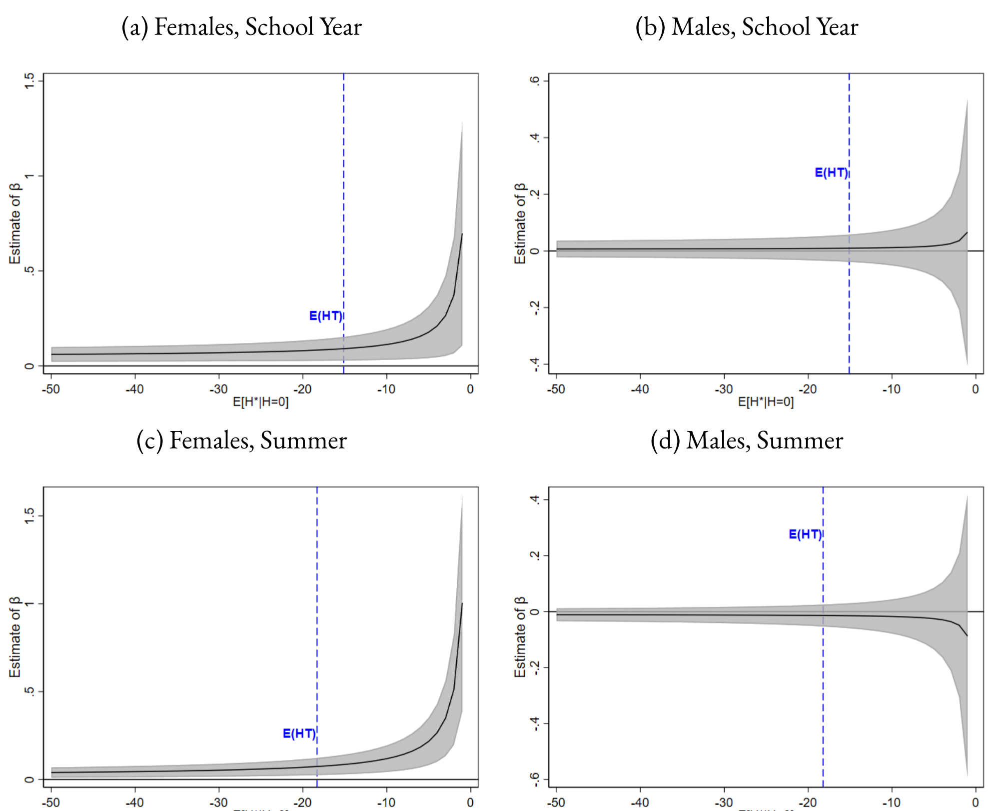
Note: The table shows the effect of one additional hour of work per week on the CESD-19 score. Bootstrapped standard errors in parentheses are based on 500 iterations. Columns (1) and (2) do not include any correction. Column (3) uses the control function approach and a semiparametric tobit distribution for the distribution of H^*. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Without Distributional Assumptions

An advantage of this approach is that I can provide qualitative results without making assumptions about the distribution of H^*. Figure 1.4 shows the results of estimating Equation 1.8 for counterfactual values of $\mathbb{E}[H^*|H = 0]$, ranging from -1 to -50. The black line represents the estimates of β given different values of the expectation, with the only requirement being that the expectation is negative.

I find that, regardless of the estimate of $\mathbb{E}[H^*|H = 0]$, working an additional hour is harmful for mental health for females, both during the summer and the school year. That is, the estimate of β is significant and positive for all tested values of $\mathbb{E}[H^*|H = 0]$. Similar to the results presented in Table 1.2, working an additional hour per week does not have a statistically significant effect on depressive symptoms for males, regardless of the choice of $\mathbb{E}[H^*|H = 0]$. The general patterns persist with point estimates that imply marginally harmful effects for males during the school year, while males during the summer have point estimates that imply marginally beneficial effects. Additionally, the estimates in Figure 1.4 provide an idea of the scope for underestimating versus overestimating the effects of an additional hour worked per week for females. The dotted vertical line in Figure 1.4 represents the $\mathbb{E}[H^*|H = 0]$ obtained from assuming that H^* follows a semiparametric tobit distribution. There is far more scope for underestimating the magnitude of the effects rather than overestimating them using the semiparametric tobit assumption, which produces a fairly negative expectation, $\mathbb{E}[H^*|H = 0]$. That is, the estimates of β stabilize and become smaller in magnitude as the expectation becomes more negative. Therefore, the estimates in Table 1.2 are nearing the lower bound of possible estimates of β resulting from different distributional assumptions about H^*.

Figure 1.4. Estimates of β Given Counterfactual Values of $\mathbb{E}[H^*|H = 0]$



Note: Each panel shows the $\hat{\beta}$ for each sample, given counterfactual values of $\mathbb{E}[H^*|H = 0]$ ranging from -1 to -50. The blue dashed line shows the $\mathbb{E}[H^*|H = 0]$ for the semiparametric tobit distribution. 95% confidence intervals shown in gray.

1.4.2 Additional Robustness

This methodology relies on two partially testable assumptions: a distributional assumption for the estimation of $E[H^*|H = 0]$ and a linearity assumption that selection varies linearly with H^*. I have already shown in Figure 1.4 that my qualitative findings are robust to various guesses of the distribution of H^*. In this section, I discuss the linearity assumption. Recall that the linearity assumption requires that the effect of H^* remain the same for $H > 0$, despite only calculating it at $H = 0$. The assumption would be violated if there are confounders that vary non-linearly with H^*.

Qualitative Robustness to Linearity Assumption

I re-estimate the model on various truncated versions of my sample, following the robustness checks from Caetano et al. (2024a) and Caetano et al. (2024b). That is, I restrict the sample to $H \leq H_{max}$, for H_{max} ranging from 1 to 60. When $H_{max} = 60$, this is equivalent to the full-sample estimates shown in Table 1.2. If the linearity assumption is violated, then the qualitative results will vary significantly as the H_{max} changes. Note, for all truncated samples, I use the same estimates of $E[H^*|H = 0]$. That is, variation in the estimate of β comes solely from nonlinearities in the confounder, not from changes in the expectation. Figure 1.5 shows the results from this test of the linearity assumption, using the same controls used in Table 1.2. Estimates of β are noisy at lower values of H_{max}; however, the main qualitative findings largely do not change as I increase the sample size, such that I am unable to reject that the estimates are the same as the sample size increases. This implies that my main results are not solely artifacts of the linearity assumption. This is especially true for my sample of females during the school year, where the most striking results are concentrated.

Additionally, I can alter how covariates enter the model in order to account for non-linearity. By allowing for controls to enter more flexibly, I weaken the linearity assumption. Following Caetano et al. (2023), I partition my sample into hierarchical clusters based on my set of controls. This allows for flexibility in the estimate of the expectation of H^*, as I allow it to vary by cluster.[26] I also allow controls to enter via indicators for clusters $\hat{C}_k$ to control for variation across clusters, leaving the controls in Z to account for only within-cluster variation, which decreases with an increased number of clusters. Therefore, increasing the number of clusters continues to weaken the linearity assumption. I build on Equation 1.8 to estimate the following model:

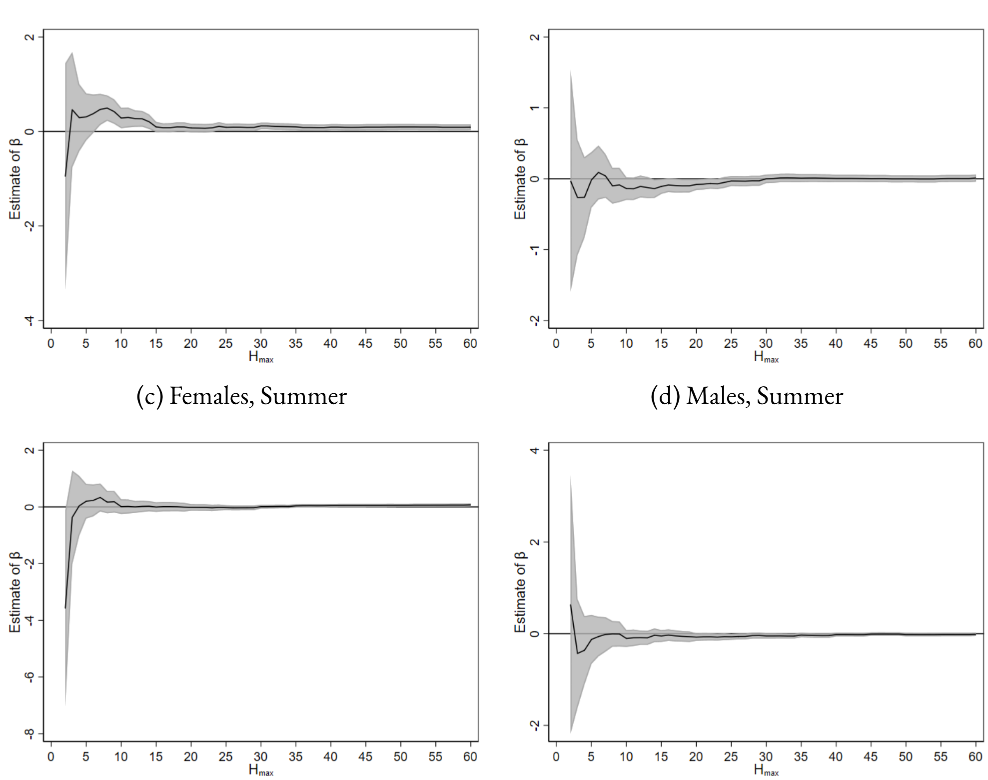
$$\mathbb{E}[Y|H, Z] = \beta H + Z'\tau + \sum_{k=1}^{K} \alpha_k \mathbb{1}(Z \in \hat{C}_k) + \delta[H + E[H^*|H = 0, \hat{C}_K]\mathbb{1}(H = 0)]. \qquad (1.9)$$

Results are robust to the number of clusters. Figure 1.6 shows the estimates of β for varying numbers of clusters from $K = 1$ to $K = 50$, and there is almost no change in the qualitative or quantitative results.

[26] I create clusters separately for males and females. I use $K = 1$ clusters in the main results presented thus far for the CESD-19.

This provides further evidence for the validity of the linearity assumption, especially when considering the amount of variation in the choice of H that the clusters capture.[27]

Figure 1.5. Estimates of β for Truncated Samples of $H \leq H_{max}$



(a) Females, School Year

(b) Males, School Year

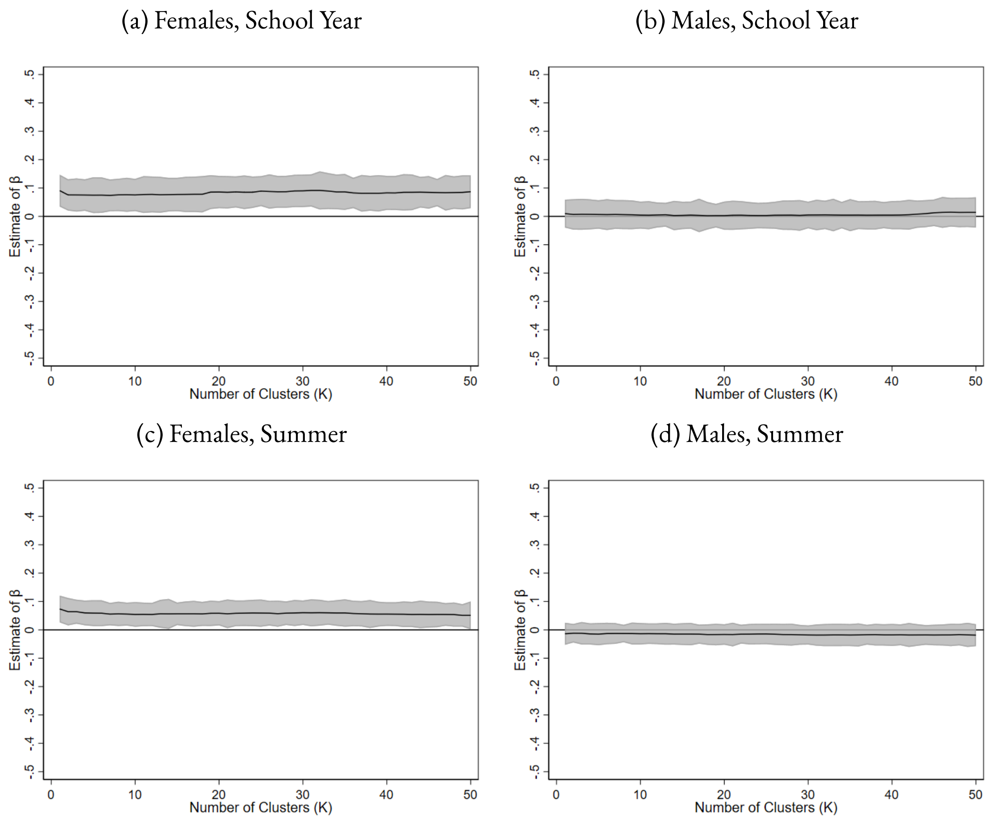
(c) Females, Summer

(d) Males, Summer

Note: The figure plots the estimates of β for truncated samples such that $H \leq H_{max}$. Estimates are shown for $H_{max} \geq 2$. The distributional assumption is the semiparametric tobit distribution. Bootstrapped 95% confidence intervals based upon 100 iterations are shown in gray.

[27] See Figure A9 for the distribution of average hours worked across clusters.

Figure 1.6. Estimates of β for Varying Number of Clusters K



Note: The figure plots the estimates of β for an increasing number of clusters K. Bootstrapped 95% confidence interval based upon 100 iterations shown in gray. Distributional assumption is the semiparametric tobit distribution.

1.4.3 Heterogeneous Effects of Hours Worked on Mental Health

I explore effect heterogeneity across five dimensions: parental education, age, urbanicity of the attended school, race, and Hispanic origin. These dimensions may be associated with different time-use trade-offs, attitudes toward employment, work environments, occupational choice, and general stressors that could impact how the effects of work hours on mental health manifest. First, I consider parental education as a proxy for family socioeconomic status (SES), which could impact the opportunities available to adolescents outside of work.[28] For example, a child from a more advantaged household may face a larger and potentially higher-quality choice set of extra-curricular or leisure activities, which may differentially impact the opportunity cost of an additional hour worked. Parental education may also correlate with the

[28] Someone has a parent with higher education if they report at least one parent having a college degree and has parents without higher education if they report both parents not having a college degree. Respondents who have missing information for both parents' educational level are not included in this analysis. Missing information on parental education could arise because the respondent does not know information about the parent or there is not a guardian figure filling the maternal or paternal role. Note, parental education reflects that of the resident mother and father, not solely biological parents.

academic pressure placed on the adolescent, which may differentially impact the stress of juggling both work and school. Furthermore, the motivation for working may differ by household SES. Those coming from low-SES households may work to help support their family, while those coming from high-SES households are less likely to financially support their family.

The effects of work hours on mental health may also differ by age. Those aged 16 to 18, compared to those aged 14 to 15, are likely preparing for college or future employment. Those preparing for tertiary school may find increased labor supply harmful if it distracts from academic activities. On the other hand, students who are preparing to enter the workforce immediately following graduation from secondary school may find working beneficial if it provides immediate and relevant practical experience. These considerations are likely less pressing for younger adolescents. Additionally, older adolescents likely have more rigorous school schedules and coursework, which may compound the stress of employment such that increased labor supply is harmful to mental health. However, older adolescents are likely more emotionally mature and may have developed more non-cognitive skills to handle the demands and stress of employment.

I also explore effect heterogeneity by the urbanicity of the school that the adolescent attends. Students who live in urban versus rural environments likely face different occupational choice sets. For example, working an agricultural job is more likely in rural communities than in urban communities. Occupations which require manual, outdoor labor likely have different mental health implications than occupations that are less physically demanding but more focused on interpersonal interactions and relationships. Furthermore, attitudes and expectations toward employment may differ in rural versus urban areas. The choice of leisure activities may also differ, which could differentially impact the opportunity cost of an hour of work.

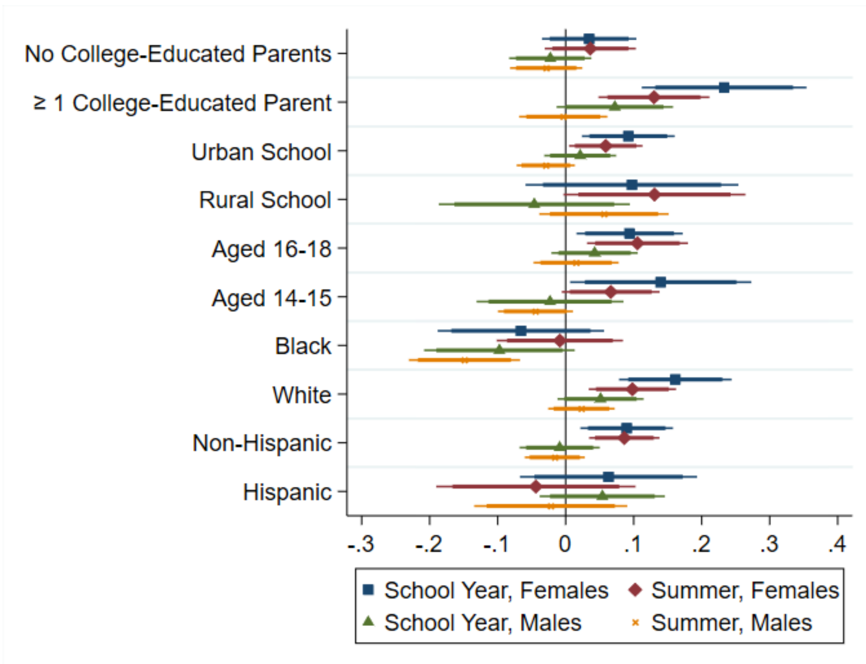
Lastly, I consider race and ethnicity. Employment opportunities may differ across racial and ethnic groups due to discrimination, geographic location, or social networks. Effects may also differ to the extent that race and ethnicity correlate with the quality of the adolescent's home life, familial expectations and attitudes around employment and education, and peer groups.

Figure 1.7 shows the estimated $\beta's$ for the heterogeneity analyses.[29] Females during the school year who have college-educated parents face harmful effects to their mental health as a result of an additional hour of work per week. Additionally, those who are older, White, non-Hispanic, or who attend urban schools see statistically significant harmful effects on their mental health. The patterns are similar for females during the summer, such that there are statistically significant harmful effects on the mental health of those with college-educated parents, who are older, White, and non-Hispanic. Females with more-educated parents do face larger detrimental effects on their mental health that are statistically different compared to those with less-educated parents. White female adolescents also face worse outcomes than Black females. While in the aggregate, males do not see statistically significant effects on their mental health, there is striking effect heterogeneity by race. Black males see improvements in their mental health, while White males see noisy, but detrimental effects as a result of increased working hours. I explore possible candidate mechanisms underlying the stark racial heterogeneity in Section A.3.

[29]See Tables A4 to A8 for point estimates and standard errors.

The differential effects based on parental education, race, and ethnicity are consistent with the descriptive literature on the relationship between adolescent employment and academic outcomes. For example, Hwang and Domina (2017) find that the negative associations between high-intensity adolescent employment and college enrollment and post-secondary degree obtainment are strongest for White adolescents.[30] Similarly, Bachman et al. (2013) find that students with highly-educated parents or from a higher-SES background see negative relationships between intensive employment and GPA. They find that this negative relationship is strongest among White students and the weakest for Hispanic students and Black students.[31] Furthermore, Lee and Staff (2007) find that the positive association between work intensity and school dropout is weaker among those with a high propensity to work, who typically are from lower-SES backgrounds.

Figure 1.7. Heterogeneous Effects of Working Hours on Depressive Symptoms



Note: The figure shows the effects of one additional hour of work on CESD-19 for different groups categorized by parental education, urbanicity of the attended school, age, race, and Hispanic origin. Results are estimated using the semiparametric tobit distributional assumption. I plot 90% and 95% confidence intervals based on bootstrapped standard errors from 500 iterations.

[30]One theory regarding the potential differences in the effects of working on education outcomes suggests that because it is harder for Black youth to find and maintain employment due to discrimination or location, those who are employed may have a higher level of motivation and skill compared to White youth (Bachman et al., 2013). For example, McLoyd and Hallman (2020) show that work intensity among Black adolescents is positively associated with expecting to complete college, while this association is negative for White youth.

[31]Johnson (2004) finds similar associations regarding substance use and work intensity, such that White adolescents see a positive relationship between work and substance use, while non-White adolescents do not.

1.4.4 Effects on Additional Mental Health Outcomes

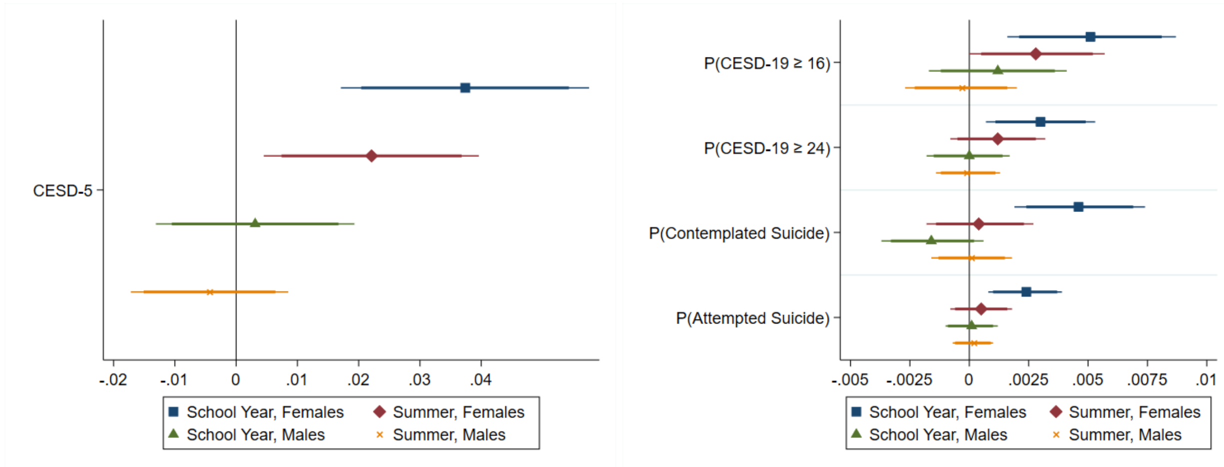
I next explore the impacts of work hours on additional mental health measures. I first consider the CESD-5, a version of the CESD score that focuses on questions explicitly tied to depression.[32] Next, I examine the probability of CESD-19$\geq$16 and the probability of CESD-19$\geq$24. These represent clinically significant cut-offs for mild and severe clinical depression, respectively. Lastly, I consider the probability of contemplating suicide or attempting suicide in the last year. For the binary outcomes, I report marginal effects using a probit specification.[33]

For the CESD-5, I find that females during both the school year and the summer have increased depressive symptoms from an additional hour worked per week. Similar to the results of Table 1.2, males see only null effects on the CESD-5. Females during the school year see an increase in the probability of having a CESD-19$\geq$16 and $\geq$24 of 0.51 percentage points from a mean of 32.1% and 0.30 percentage points from a mean of 10.1% for each additional hour worked, respectively. Females during the school year also face an increase in the probability of contemplating and attempting suicide of 0.46 percentage points from a mean of 15.5% and 0.24 percentage points from a mean of 5.4% for each additional hour worked, respectively. Other than a marginally significant increase in the probability of having a CESD-19 $\geq$ 16 among females during the summer, I do not find statistically significant effects on these measures for the other subsamples. Overall, an additional hour of work per week has harmful effects for females during the school year, not only on the average CESD-19 score, but on measures that indicate more serious mental health concerns. Despite females during the summer seeing increased depressive symptoms when considering the average CESD-19 score and the CESD-5 score, there are no statistically significant increases in the likelihood of experiencing more extreme mental health outcomes as a result of increased work hours, such as severe depression or suicidality.

[32] The CESD-5 includes the following questions: “you felt you could not shake off the blues", “you felt depressed", “you were happy", “you enjoyed life", “you felt life was worth living".

[33] Although not used before in practice by other papers applying this method, Caetano et al. (2023) show that the control function approach can be used with nonlinear models such as probit. Results are qualitatively similar when estimated using a linear probability model instead.

Figure 1.8. The Effects of Working Hours on Additional Mental Health Outcomes



Note: The figure shows the effects of one additional hour of work per week on various measures of mental health. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Results are estimated using the semiparametric tobit distributional assumption. Results in the right figure are estimated using a probit model. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

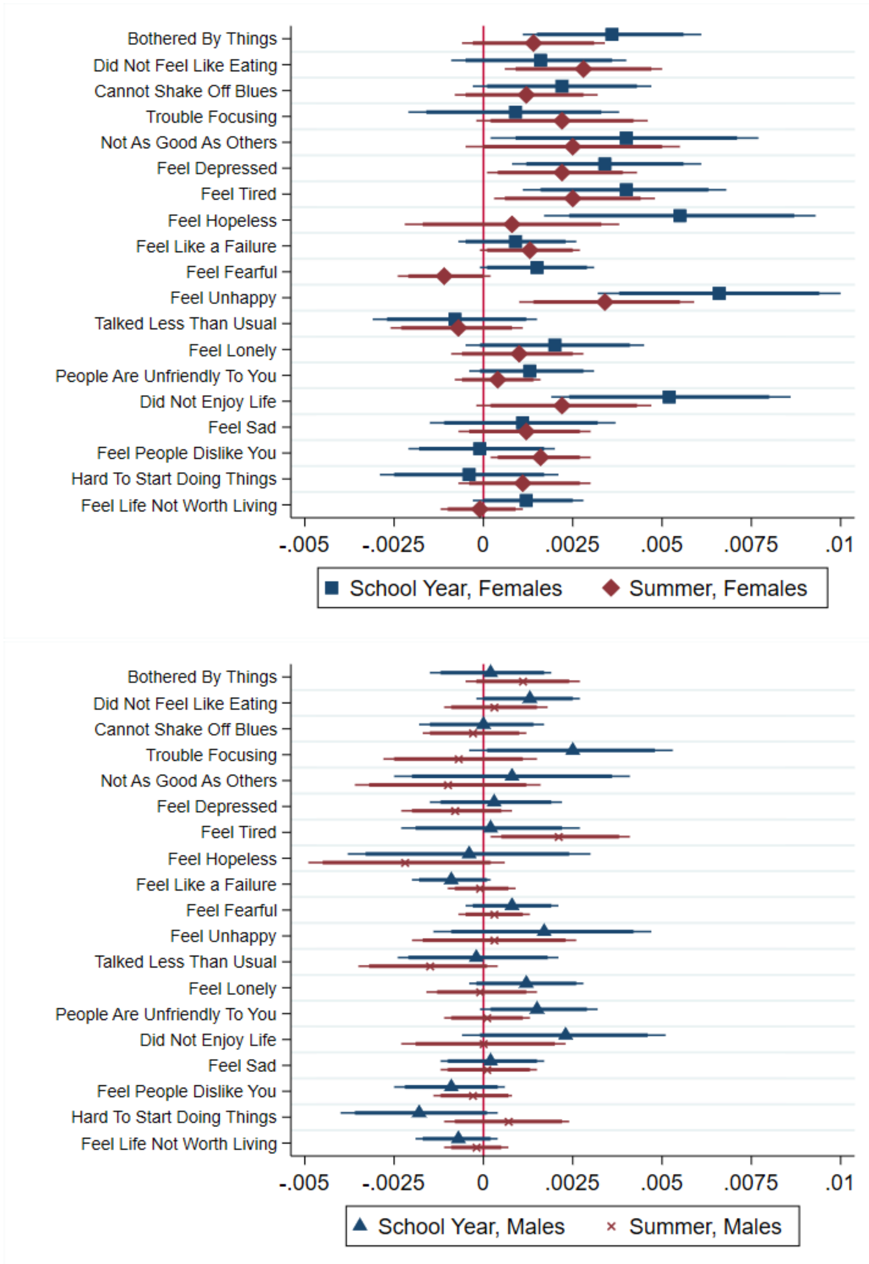
Lastly, I estimate the effects of an additional hour worked per week on each component of the CESD-19. I create a binary variable that categorizes an individual as not experiencing a particular symptom if they report that they "rarely or never" or "sometimes" experienced it in the last week and as experiencing the symptom if they report that they experienced the symptom "for a lot of the time" or "for most of the time or all of the time" in the last week. The goal is to explore particular dimensions of mental health that are impacted by increased working hours. Certain components of the CESD-19 score may be especially indicative of mental health concerns that could manifest into extreme outcomes such as suicidal ideation. For example, Carleton et al. (2013) notes that items such as "people are unfriendly to you" or "people dislike you" may capture conditions other than depression, such as social anxiety. On the other hand, items such as "feels unhappy" or "feels hopeless", are directly tied to an increased depressive mood. If the items that are impacted are those that are most aligned with depression, then an increased CESD-19 score may have more pressing clinical implications.

I find that for females during the school year, there are increases in the probabilities of reporting that they are bothered, they feel they are not as good as others, they feel depressed, they feel tired, they feel hopeless, they feel unhappy, and they do not enjoy life. Similarly, females during the summer have an increased likelihood of reporting that they did not feel like eating, they have trouble focusing, they feel depressed, they feel tired, they feel unhappy, they do not enjoy life, and they feel people dislike them. For males during the school year, there are marginally significant increases in the likelihood of reporting that they are having trouble focusing and that people are unfriendly to them. Meanwhile, for males during the summer, there is only a significant increase in feeling tired.

Comparing the effects across individual components, it is unsurprising that it is primarily females during the school year who report increases in more extreme mental health outcomes, such as the likelihood of having clinical depression and increased suicidality. Although females see increases in these concerning CESD-19 components during both the school year and summer, the increases are larger during the school year for items such as feeling hopeless, feeling unhappy, and not enjoying life, though the differences are not necessarily statistically significant. A majority of the components that seem to drive the results for females are classified as part of the depressive affect or the reverse of the positive affect domains of the CESD score, which are directly linked to depressive mood (Radloff, 1977).[34] Therefore, the components that are most impacted for females during the school year are those more likely to manifest in clinically significant depression or suicidal ideation.

[34]The CESD score broadly reflects four domains of symptoms: depressive affect, positive affect, somatic, and interpersonal (Radloff, 1977). The depressive affect includes questions about the blues, feeling depressed, feeling lonely, feeling sad, and crying. The positive affect includes questions about feeling good, hopeful, happy, and enjoying life (these questions have been reverse coded when calculating the CESD score). The somatic domain includes questions about being bothered easily, lack of appetite, trouble keeping your mind on what you were doing, everything feeling like it takes effort, not being able to sleep, and having problems getting going. The interpersonal domain includes questions on feeling that people are unfriendly to and dislike you.

Figure 1.9. The Effects of Working Hours on Components of the CESD-19 Score



Note: The figure shows the effects of one additional hour of work on binary measures of each component of the CESD-19 score. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Results are estimated using the semiparametric tobit distributional assumption and a probit model. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

Overall, it appears that females, especially during the school year, are differentially impacted compared to males, such that males see null effects in the aggregate for all mental health outcomes. There are several possible reasons for the stark differences in effects by gender. First, occupations may differ such that females tend to work in jobs that are more damaging to their mental health.[35] Add Health does not contain occupational information for adolescents. In Section A.2, I use the Current Population Survey (CPS) to provide insight into the occupational composition of working adolescents during the same period as my Add Health sample. Table A13 shows that a little less than 40% of both males and females report working in the service industry. However, only 16% of males report being in sales, compared to 37% of females. In addition, 9% of males report being in agricultural occupations such as farming, forestry, and fishing, compared to only 2% of females, and 23% report being handlers, equipment cleaners, helpers, and laborers, compared to only 4% of females. Additionally, Table A14 shows the ten most commonly reported detailed occupations broken down by gender. For example, for males, the top three occupations are food service (30%); freight, stock, and material handlers (15%); and sales, retail, and personal services (15%). For females, the top three occupations are sales, retail, and personal services (36%); food service (28%); and administrative support and clerical work (8%). Females tend to work in occupations that require more social interactions, notably with customers. Males, on the other hand, tend to work in occupations that require more manual labor, but not as much interpersonal interaction. It could be the case that the occupations that require frequent interaction with others are particularly harmful to females' mental health, especially if those social interactions tend to be unpleasant, damaging to self-esteem, or stressful.

Second, there could be differences in how males and females are affected by changes in their peer group. Working adolescents are introduced to individuals outside of their standard school-based friend groups. These peers may differ from those they usually interact with and may be older. Depending on the behaviors these coworkers encourage or the situations they introduce, increased interaction with them due to increased labor supply could be detrimental to mental health. Due to the contrast in occupations (e.g., manual labor compared to retail), the composition of coworkers that males and females are exposed to likely differ as well. Furthermore, changes in time use in response to increased labor supply may differ across males and females, which could contribute to the observed effect heterogeneity by gender. For example, females may see worse impacts from decreased sleep than males do (Forest et al., 2022), especially given that research indicates that female adolescents already have worse sleep quality (Lewien et al., 2021).

In addition to effect heterogeneity by gender, the detrimental effects of work hours on female mental health are larger during the school year versus the summer. This could simply be due to the additional academic stress or differences in occupations in the summer versus the school year. Peer groups and time use could be important as well. For example, during the summer when there is more time available for

[35] Research shows that for adults, workplace characteristics are important when considering the effects of labor supply on mental health. Therefore, variation in occupations can be an important source of effect heterogeneity. The consensus is that occupations with greater physical demands or mental stress (Plaisier et al., 2007) and those with the less autonomy (Cottini and Lucifora, 2013; Spearing, 2025) are especially damaging to mental health. Maclean et al. (2015) notes that workplace problems, largely among co-workers, can also lead to poor mental health among adults. Occupations involving higher levels of co-worker and social interaction could, therefore, be more detrimental to their mental health. However, effects could be fairly different for adolescents, given the difference in their physical and emotional development compared to adult workers.

socializing, working may not crowd out time spent with non-work friends. However, during the school year, when there is less available time, a larger share of socializing outside of the school environment may be done with coworkers instead. Additionally, during the summer, there is more time available to spend on leisure activities such as sports and hobbies. However, during the school year when leisure time is more limited due to the school day, homework, and extracurricular activities, substituting away from leisure in response to increased labor supply may be more harmful to mental health. I explore the effects of adolescent labor supply on time use, peer groups, and risky behaviors in the following section.

1.4.5 Additional Outcomes: Time Use, Risky Behaviors, Delinquency

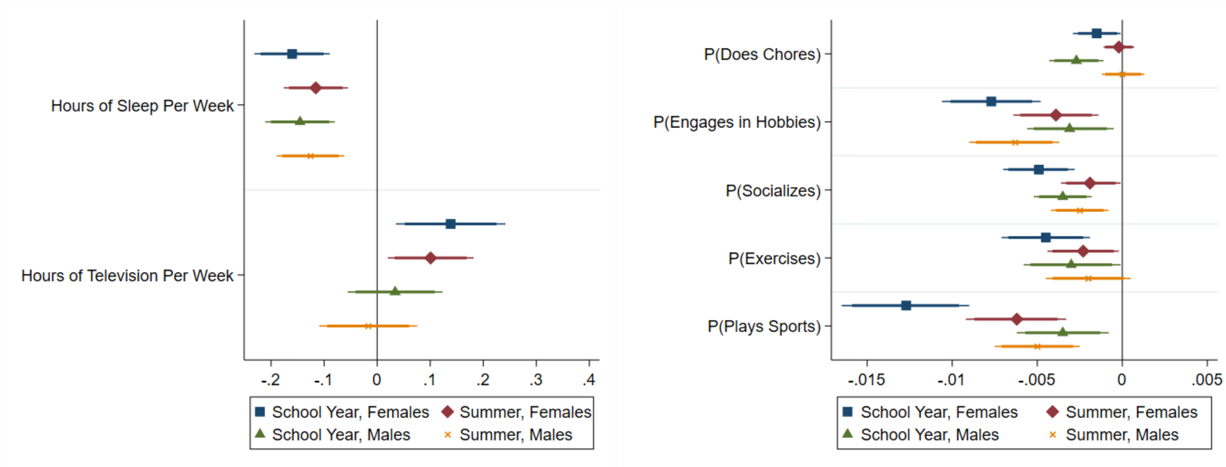
Next, I consider other outcomes that are important for human capital development and may be relevant to the mental health-labor supply relationship. These results only show that working an additional hour per week affects other outcomes and I cannot formally identify or quantify the role these outcomes play in the observed relationship between mental health and labor supply. Some outcomes, such as time use and risky behaviors, could underlie the effects of work hours on mental health. I include these results to paint a more complete picture of the effects of work hours on the behaviors of adolescents that may be related to mental health.

Time Use

In this section, I examine how work hours affect the probability of doing chores, engaging in hobbies, socializing, exercising, or playing active sports in the last week. I also examine the number of hours a week spent sleeping and watching TV. Figure 1.10 shows the results for these time use outcomes, including the probability of engaging in an activity in the last week. There are statistically significant time-use substitutions for all subsamples as a result of increased working hours. An additional hour of work per week results in decreases in hours of sleep per week and the probabilities of engaging in hobbies, playing sports, and socializing in the last week for all subsamples. Most subsamples see a decreased likelihood of exercising in the last week resulting from an additional hour of work per week. Interestingly, males and females during the school year also have a decreased likelihood of doing chores in the last week.

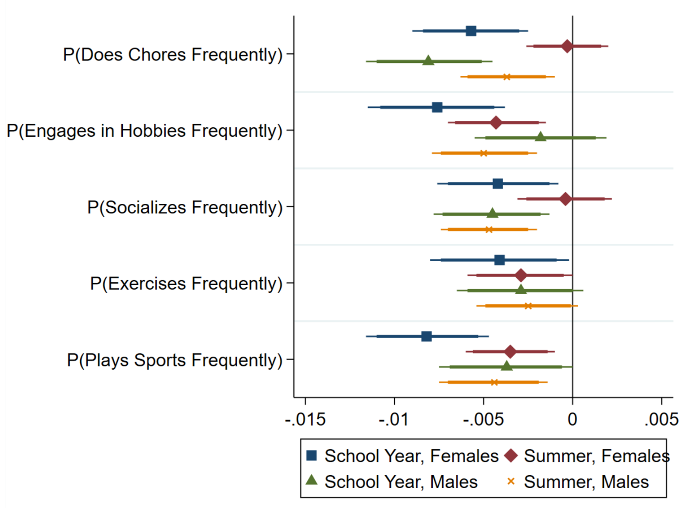
Figure 1.11 shows the effects of an increase of one work hour per week on the probability of engaging in these activities frequently, which I define as engaging in the activity more than twice a week. All subsamples see decreases in the probability of frequently playing sports, while females also see decreases in the probability of frequently exercising. Lastly, most subsamples see a decreased likelihood of engaging in hobbies frequently, socializing frequently, and doing chores frequently.

Figure 1.10. The Effects of Working Hours on Time Use



Note: The figure shows the effects of one additional hour of work per week on various time-use outcomes. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Results are estimated using the semiparametric tobit distributional assumption. Results in the right figure are estimated using a probit model. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

Figure 1.11. The Effects of Working Hours on Time Use



Note: The figure shows the effects of one additional hour of work per week on various measures of time use. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Results are estimated using the semiparametric tobit distributional assumption and a probit model. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

The patterns of time-use substitutions may be helpful for understanding the differential effects of work hours on mental health. During the school year, females see a few unique adjustments that may contribute to the negative effects on their mental health. They see a large decrease in the probability of playing sports as a result of increased labor supply. Playing sports includes active team sports such as soccer or softball, likely extra-curricular activities during the school year. Females during the school year also experience the largest decreases in the probabilities of engaging in hobbies frequently and socializing with friends in the last week. The effects on hobbies, socializing, and sports may indicate an important change in the peer groups and social life of adolescent females. Females also see statistically significant increases in the number of hours they spend watching television as a result of additional working hours. Thus, females seem to substitute active leisure, such as hobbies and sports, for less social and more passive types of leisure, such as television watching, which could be harmful to mental health.

Risky Behaviors and Delinquency

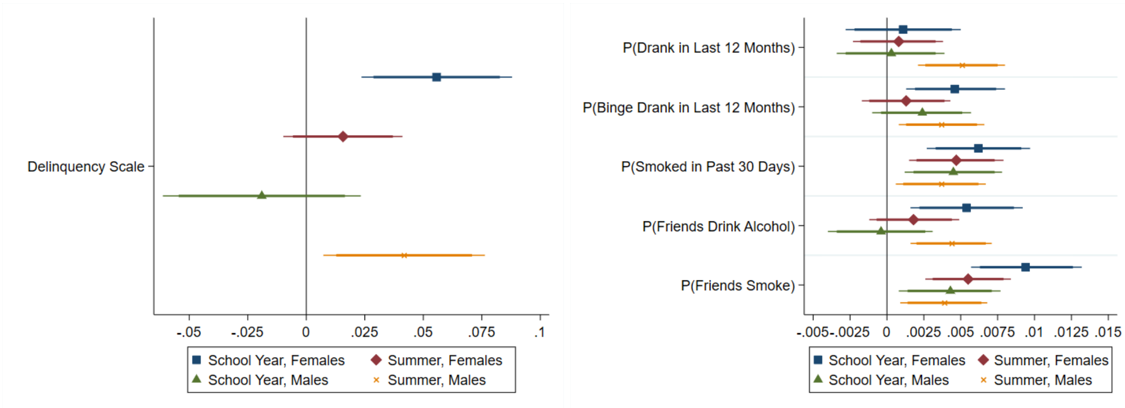
Next, I explore the effects of an additional hour of work on risky behaviors such as substance use and delinquency. Specifically, I examine the probability of drinking and binge drinking alcohol in the past 12 months, the probability of having smoked in the last 30 days, and the probability of having friends who engage in risky behaviors. Figure 1.12 shows results for substance use and delinquency.[36]

An additional hour of work per week results in an increased probability of smoking in the past 30 days for all subsamples. Increasing work hours also increases the probability of having drunk in the last 12 months for males during the summer and of binge drinking in the last 12 months for some subsamples. Females during the school year and males during the summer see an increase in the probability of having close friends who drink alcohol, and all subsamples see increases in the probability of having close friends who smoke. Lastly, the delinquency scale captures the frequency at which individuals engage in illegal or delinquent behaviors in the last 12 months. Both females during the school year as well as males during the summer see increases in delinquency. However, females during the school year have the most notable change, with a 0.0557 increase in the delinquency scale, from a mean of 3.307, from an additional hour of work per week.

It appears that working additional hours per week results in increased substance use as well as changes to the peer group, such that there is a higher probability that peers engage in substance use. The increased probability of substance use and change in peer group for females during the school year is particularly interesting given the robust harmful effects to their mental health from increased labor supply. Coupled with their increase in delinquency, these behavioral changes may contribute to the effects on mental health or could be the result of declining mental health. These changes are not unique to females during the school year nor are the effects statistically different compared to the effects for the other subsamples. However, especially in regard to peer effects, it could be that these influences are more harmful for the mental health of females.

[36] Table A12 reports the detailed point estimates and standard errors for this figure. Figure A8 shows a breakdown of the effects on the delinquency scale components, following the same method used in Figure 1.9 for the components of the CESD-19 score.

Figure 1.12. The Effects of Working Hours on Delinquency and Substance Use



Note: The figure shows the effects of one additional hour of work per week on various measures of delinquency and substance use. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. The distributional assumption is the semiparametric tobit distribution. Results in the right figure are estimated using a probit model. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

1.5 Conclusion

I estimate the causal effects of hours worked on adolescent mental health. I account for endogenous selection into employment using a novel control function approach. This method relies on the assumption that while the treatment effect is continuous, confounders are discontinuous at zero hours of work due to bunching, so discontinuities in mental health can be used to estimate the effect of the confounders. After correcting for endogeneity using the estimated selection effect, I find that an additional hour of work per week results in significant increases in depressive symptoms for females, both during the summer and school year. I find little evidence of statistically significant impacts on mental health for males in the aggregate. In addition to increased depressive symptoms, during the school year, females have an increased probability of reaching clinically significant thresholds for developing severe depression, as well as contemplating or attempting suicide. Additionally, I find evidence of effect heterogeneity by parental education and race. That is, females with a college-educated parent face larger declines in mental health as a result of an additional hour of work compared to those without a college-educated parent. White adolescents face more harmful effects on mental health than Black adolescents. In fact, Black male adolescents see an improvement in mental health when work hours increase.

I explore other outcomes such as time use and risky behaviors, which are not only important for our understanding of human capital development and how adolescents adjust their behavior to more intensive employment, but are possibly tied to the mental health-labor supply relationship. I find that an additional hour of work results in meaningful time use substitutions. That is, adolescents decrease the time they spend sleeping, decrease the likelihood of engaging in active leisure, and increase passive leisure.

Additionally, there is a shift toward risky behaviors, such that an additional hour of work results in an increased probability of smoking, as well as increased probabilities of having friends who smoke. For some groups, more work hours increase the probability of having friends who drink alcohol.

Overall, there is a stark difference between the effects for males and females. That is, female adolescents see statistically significant harmful effects to their mental health, while male adolescents see null or, on rare occasion, beneficial effects to their mental health as a result of increased working hours. The detrimental effects on mental health are worse for females during the school year compared to other subsamples, including females during the summer. There is more to learn about the mechanisms underlying the mental health-labor supply relationship and why females during the school year are particularly vulnerable to poor mental health compared to their peers. It is beyond the scope of this paper to formally identify mechanisms; however, two candidate mechanisms emerge that may explain the changes in mental health: changes in peer groups and leisure activities. First, the results suggest that there is a change in social patterns and peer groups, namely decreased socializing and active sports, as well as increased likelihood of having friends who engage in risky behaviors. Some of the harmful mental health impacts for females during the school year could result from interacting less with their peers from school and more with co-workers, who may differentially expose them to behaviors and situations that are harmful to mental health. Secondly, there are clear patterns of substituting away from leisure activities or substituting active leisure (i.e., sports, exercising, and hobbies) for passive leisure (i.e., television watching). Therefore, adolescents may not only engage in less leisure, but for females, they may also engage in leisure that is less beneficial to their mental health. Future work aimed at formally identifying the mechanisms that underlie the effects of adolescent labor supply on mental health is a promising avenue of research.

The results have important implications for policies affecting youth employment, specifically those impacting intensive margin employment. Policy aimed at strengthening child labor protection by decreasing permitted hours worked per week, specifically during the school year, could improve the mental health of adolescent females. However, current state-level policy is moving toward weakening child labor protections by allowing increased working hours, which could have detrimental spillovers to female mental health. On the other hand, for Black males, legislation relaxing the caps on working hours could improve mental health outcomes. Mental health is but one outcome to consider when designing child labor policies. Nevertheless, the results highlight a challenge for policy making as some groups may be helped and others harmed by policies that encourage more work among adolescents.

Chapter 2

The Effects of Adolescent Labor Supply on Educational Outcomes

2.1 Introduction

As of 2023, data from the Current Population Survey (CPS) indicates that 33% of youth aged 16-19 are employed (U.S. Bureau of Labor Statistics, 2023).[1] Given the sizable youth labor market participation, it is important to understand how changes in labor supply impact adolescent outcomes. Adolescent employment likely has particularly notable impacts on educational outcomes, as it may alter how youth engage with or value their schooling. These effects are especially important to understand given that education likely predicts a wide range of later-life outcomes.[2]

The effects of adolescent labor supply on educational outcomes are ambiguous. Working could be beneficial if it complements educational success through the development of human capital and skills such as time management, responsibility, and critical thinking. However, labor supply could also crowd out students' time for homework, studying, or extracurricular activities. If adolescents substitute away from leisure and rest or experience work-specific stress, working can result in exhaustion or worsened mental health, negatively impacting educational outcomes. Employment also introduces youth to new peer influences, which may alter their behavior, and may shift their preferences regarding continued education by either reinforcing the desire to pursue further education or revealing career paths that do not require further education. These changes in preferences may affect a wide range of educational outcomes as students alter their academic focus. This paper addresses this empirical question while offering relevant policy insights as several states seek to reform child labor laws by relaxing youth work hours restrictions.[3]

[1] Youth have become less attached to the workforce over time, as their employment rates have declined over the past few decades. During my analysis period, 1994-1996, approximately 44% of adolescents participated in the labor force (U.S. Bureau of Labor Statistics, 1995).

[2] For example, GPA and enrollment in high-level math courses positively correlate with future labor market earnings (Joensen and Nielsen, 2009; French et al., 2015).

[3] In general, youth are subject to strict restrictions under the federal Fair Labor Standards Act (FLSA) (United States Congress, 1938). Upon reaching age 16, youth labor restrictions regarding hours worked are largely determined at the state

I use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health) to study the causal effects of labor supply on youth educational outcomes. Specifically, I estimate the effect of an additional hour worked per week on high school dropout, college enrollment, absenteeism, GPA, and course failure. I implement the novel control function approach of Caetano et al. (2023) to address concerns regarding endogenous labor supply. This method leverages the sizable bunching of individuals working zero hours per week. When examining non-working individuals (who are bunched at zero hours worked per week), there are considerable differences in not only key observables but also educational outcomes. This method posits that the discontinuities in outcomes stem from a discontinuity in confounders rather than the treatment itself. By estimating a ratio of the discontinuity in outcomes to the discontinuity in confounders at zero hours worked, I can identify the effect of selection on educational outcomes and thereby indirectly determine the treatment effect.

I find that additional working hours have detrimental effects on educational outcomes for both male and female students. For females, an additional hour of work per week results in a 0.27-ppt increase in the probability of dropping out of high school (a 6.75% increase from the sample mean) and a 0.66-ppt decrease in the probability of enrolling in college (a 0.90% decrease from the sample mean). An additional hour of work also results in reductions in both cumulative GPA at the end of high school and yearly GPA of 0.0208 and 0.0216 points (a 0.76% and 0.79% decrease from the sample means), respectively. I find that an additional hour worked per week leads to increased probabilities of ever failing a course, both throughout the entire high school career and per academic year, of 1.13 ppt and 0.68 ppt (a 2.53% and 2.67% increase from the sample means), respectively. The harmful impacts on academic achievement are larger for females than for males, although these differences are not always statistically significant. Males experience a 0.24-ppt increase in the likelihood of dropping out of high school (a 3.63% increase from the sample mean) and a 0.55-ppt decrease in the likelihood of enrolling in college (a 0.84% decrease from the sample mean). Working an additional hour per week results in decreases in cumulative and yearly GPA of 0.0091 and 0.0111 points (a 0.37% and 0.46% decrease from the sample means), respectively. Each additional hour increases the probability of ever failing a course by 0.46 ppt (a 0.81% increase from the sample mean) and of ever failing a course in a given academic year by 0.37 ppt (a 1.05% increase from the sample mean). In robustness exercises, I am able to confirm that there is a larger scope for underestimating the magnitude of effects than overestimating.

I perform several additional analyses. I examine effect heterogeneity, the effects of summer employment, and the effects of labor supply on student engagement and expectations. My analysis reveals substantial effect heterogeneity, largely by race. The adverse effects are generally larger in magnitude for White females relative to Black females. I find that increased work hours in the summer results in detrimental effects similar to those during the school year, albeit smaller in magnitude. Notably, unlike school-year

level. A majority of new child labor laws reduce restrictions on how many hours per week a child can work or how many hours they are permitted to work per day. Iowa SF 542 increases the allowable number of hours a minor under 16 can work from 4 hours per day to 6, but did not alter the maximum number of hours allowable per week (Iowa General Assembly, 2023). Indiana HB 1093 removes all hours restrictions on 16- and 17-year-olds (Indiana General Assembly, 2024). A few states have passed legislation to tighten restrictions on child labor supply. For example, Illinois SB3646 restricts minors under 16 from working more than 18 hours a week when school is in session (Illinois General Assembly, 2024).

work, an additional hour of work per week during the summer does not result in an increased likelihood of dropping out of high school. Regarding student engagement, I find that increased labor supply increases school absenteeism but has no impact on difficulty completing homework or classroom attention. Increased labor supply also results in decreased expectations and desire to attend college, especially for females.

There exists a large literature on the effects of extensive- and intensive-margin employment on numerous adolescent educational outcomes.[4] Research that has addressed concerns regarding selection into employment, typically exploiting instrumental variables or within-person variation, has yielded somewhat mixed results, though most find non-positive impacts on academic outcomes.[5] Of these papers, several estimate adverse impacts of adolescent employment, especially intensive employment, on academic outcomes including test scores (Tyler, 2003; Holford, 2020), high school dropout (Parent, 2006; Apel et al., 2008), college enrollment (Lee and Orazem, 2010), and GPA (Oettinger, 1999). In contrast, several studies report null effects of youth employment on academic achievement and educational outcomes. For instance, Rothstein (2007), Sabia (2009), and Lee and Orazem (2010) find no impact on high school GPA resulting from increased work intensity. Similarly, Buscha et al. (2012) detect no impact on reading or math achievement scores. Deviating from the literature finding non-positive effects, both Lee and Orazem (2010) and Lesner et al. (2022) estimate small positive effects on the probability of completing high school.

This paper contributes to the literature on the effects of adolescent labor supply on educational outcomes in several ways. First, I use a new methodological approach that relies on less restrictive identifying assumptions than those used in prior studies and that recovers average treatment effects. Most studies exploit within-person variation or use instrumental variables in order to estimate the effect of labor supply on educational outcomes. However, methods relying on within-person variation may produce biased estimates of the treatment effect if there are individual-specific time-varying confounders (Oettinger, 1999).[6] For example, student motivation and ability likely vary as knowledge, preferences, and opportunity costs of working change. Additional confounders such as family circumstances, peer effects, or physical and mental health may vary throughout the academic career and influence both academic success and labor market decisions. Consequently, it is unclear whether the estimated effects can be attributed to within-person changes in labor supply or to changes in confounders, thereby producing biased results.

Furthermore, instrumental variable approaches are unlikely to adequately satisfy the exclusion restriction. The most commonly used instrument, state child labor laws (e.g., Tyler, 2003; Rothstein, 2007; Apel et al., 2008), may not be valid as states that prioritize protections against child labor law violations are likely also states that prioritize and promote educational success (Buscha et al., 2012). Other commonly used instruments, such as local labor market conditions (e.g., Parent, 2006; Rothstein, 2007;

[4] Neyt et al. (2019) provide a thorough review of this literature, including the effects of working during collegiate years.

[5] On occasion, these results vary by gender. For example, Dustmann and Van Soest (2008) and Montmarquette et al. (2007) find that increased labor intensity primarily adversely impacts the dropout decisions of males. Additionally, various works find evidence of non-linear effects of employment on educational outcomes, such that low-intensity work has positive or null effects on outcomes (e.g., Oettinger, 1999; DeSimone, 2006; Parent, 2006; Montmarquette et al., 2007).

[6] I cannot use within-person variation to estimate effects for many of my outcomes. As noted in Oettinger (1999), dropout, tertiary enrollment, and cumulative GPA are absorbing states; therefore, changes in an individual's labor market behavior across waves cannot be exploited to estimate causal effects.

Lee and Orazem, 2010), are potentially problematic as they likely impact academic outcomes through channels other than own employment decisions. For example, local labor market conditions impact parental employment decisions, stress in the household, and school funding. Even instruments specifically related to the youth labor market may correlate with peer behavior, general local economic conditions, and expectations around teen employment.

The Caetano et al. (2023) bunching approach has numerous strengths compared to the previously employed empirical approaches. Notably, I estimate the average treatment effect (ATE) of increased labor supply on educational outcomes. This differs from IV approaches which only estimate the local average treatment effect (LATE) of compliers whose behavior is induced to change by the instrument. Although approaches exploiting within-person variation estimate an ATE, we may not be convinced of the credibility of these estimates in light of time-varying confounders. Furthermore, this method does not rely on an exclusion restriction but rather on assumptions regarding the continuity of the treatment effect. Additionally, this method is well suited to exploring effect heterogeneity, allowing me to investigate differential effects by gender, race, ethnicity, parental education, and urbanicity.[7]

In addition to the methodological advantages, the richness of the Add Health data allows me to make further contributions.[8] First, I analyze a comprehensive bundle of outcomes that are typically not studied together using one consistent empirical approach and sample. I examine the effects on cumulative and long-term outcomes, such as high school dropout, college enrollment, and cumulative GPA, while also studying year-specific outcomes such as yearly GPA. I also study course failure, which captures students who are particularly at risk for academic difficulties, rather than just GPA changes that may result from moderate declines in academic achievement. Course failure may require students to repeat courses or attend summer school, or even lead to high school dropout in extreme cases. I can link individuals to their transcript data, providing me with both objective transcript and self-reported measures of academic success. The self-reported measures also allow me to explore outcomes related to academic engagement such as homework, attention, absenteeism, and expectations.

Lastly, I examine the effects of summer labor supply, as individuals in Add Health report their typical work hours during both the summer and school year. Various papers have considered summer employment to some extent, but typically as a covariate or as a sensitivity check for the effects of school-year labor supply (e.g., Oettinger, 1999; Lee and Orazem, 2010; Baert et al., 2022). Instead, I examine various outcomes in relation to summer employment and compare the estimated effects between those working during the summer and those working during the school year. The effects of working during the summer may differ from those during the school year for various reasons. Most notably, there is less concern about employment crowding out academic activities during the summer. However, working during the summer may still introduce students to new peers and behaviors, alter preferences and expectations regarding

[7]Note that a small set of the literature explores heterogeneous effects. DeSimone (2006) explores differences by gender, race, age, parental employment, and metropolitan location. Oettinger (1999) examines heterogeneity by race, finding the worst impacts among racial minorities.

[8]To my knowledge, Sabia (2009) is the only other study to use Add Health to investigate the causal effects of working on academic outcomes. However, our studies differ significantly. He restricts his sample to adolescents aged 12-15, only uses self-reported GPA to gauge academic achievement, and exploits within-person variation.

higher education, and crowd out certain academic efforts (e.g., studying for standardized tests). Therefore, I examine how summer labor supply affects not only cumulative outcomes but also outcomes during the following academic year.

The remainder of this chapter is organized as follows. Section 2.2 describes the data. Section 2.3 discusses the empirical strategy. Section 2.4 presents the results for the full sample, heterogeneity analyses, and robustness checks. Section 2.5 discusses the results in the context of the literature. Section 2.6 concludes.

2.2 Data

I use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a nationally representative, longitudinal survey following 7^{th} to 12^{th} grade students from wave 1 (during the 1994-95 school year) until wave 5, when they are 33-45 years old. I primarily use the first two waves of the in-home survey, which cover the 1994-95 and 1995-96 school years, to study students during their high school career. Individuals are interviewed once during each wave, which may occur during the school year or the summer.[9,10]

I restrict my sample to individuals aged 14-18 years old who are currently enrolled in high school and have non-missing information on employment.[11] I create two types of samples depending on the nature of the outcome. For outcomes that are contemporaneous to an academic year, I use all observations in waves 1 and 2 that report being enrolled in high school. Therefore, I treat the data as a repeated cross-section. For outcomes that are cumulative across years of high school, or occur following high school completion, I only allow for each individual to be included once. In my main specification, I only include an individual the last time I observe them in high school.[12] These restrictions result in a final analytical sample of approximately 21,000 observations for academic-year outcomes and approximately 14,000 observations for cumulative outcomes. See Table B1 and Table B2 for descriptive statistics of the academic year and cumulative sample, respectively.[13]

[9] When interviewed during the summer, they are asked to answer questions about the previous academic year. For example, an adolescent interviewed for wave 1 in June 1995 answers questions about the 1994-1995 school year.

[10] Individuals who were seniors in high school during wave 1 are not interviewed in wave 2, but are interviewed in subsequent waves.

[11] Many of my outcomes are sourced from high school transcripts, therefore I restrict my sample to be exclusively high school students for consistency. Additionally, I restrict my analysis to students who work no more than 60 hours a week.

[12] This will, by design, create a sample that is heavily composed of juniors and seniors. However, as shown in Table B7, qualitative results are mostly robust to only including individuals the first time I observe them in high school, which includes more freshmen and sophomores. The exception is that the estimates of β for high school dropout and college enrollment are no longer statistically significant and slightly smaller in magnitude for males.

[13] Tables B3 to B6 shows the descriptive statistics for each sample type when restricting on having transcript information and on having information on high school dropout and college enrollment.

2.2.1 Key Variables

I study the effects of working on various educational outcomes. I first study high school dropout and tertiary school enrollment. These outcomes are measured during wave 4, when individuals are 24 to 32 years old. An individual is considered to have dropped out of high school if they are observed at wave 4 to not have either a high school diploma or a GED.[14] Someone is considered to have enrolled in college if they report having completed some college education in wave 4. Next, I study cumulative high school outcomes, namely the probability of ever failing a course and cumulative GPA at the end of the student's high school career. These outcomes come from the Adolescent Health and Academic Achievement (AHAA) study education files, which is an expansion of the original Add Health study.[15] These data are collected during wave 3, when individuals are aged 18 to 24, and include information reported from the school or included on the student's high school transcript. Collection of this information is not dependent on completion of high school and is included for all students regardless of whether they obtained a high school degree if they consent to their transcript being collected. Lastly, I study academic-year outcomes. Specifically, I examine yearly GPA and the probability of failing a course.[16] Information on yearly GPA and course failure are taken from the AHAA education data.

I measure hours worked during the academic year using the reported number of hours worked in a typical non-summer week. I assume that a respondent's reported typical number of hours worked reasonably reflects their behavior in the given academic year. For the analysis of summer employment, I use a similar measure of their reported number of hours worked in a typical summer week. Individuals report both typical hours worked during summer and non-summer week regardless of whether they are interviewed during school or summer months. Note that Add Health does not report information on adolescents' occupations. See Section A.2 for a breakdown of occupation categories for this age group in the CPS.

2.3 Empirical Strategy

I employ the control function approach developed by Caetano et al. (2023) to uncover the causal relationship between adolescent labor supply and educational outcomes. This section includes an overview of the methodology, as well as relevant information unique to the studied outcomes. For a more comprehensive

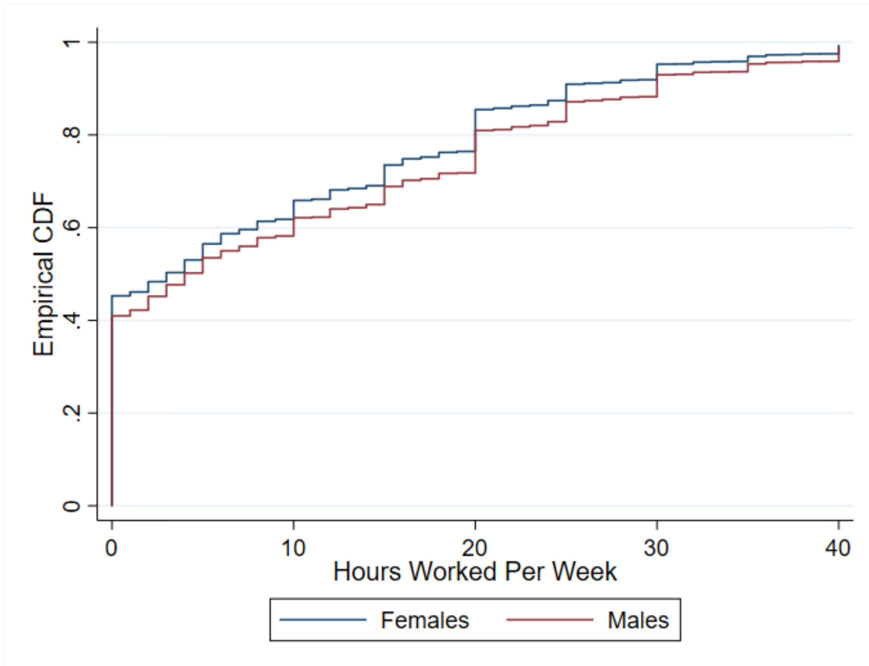
[14]Qualitative results are robust to considering receipt of a GED as a form of high school dropout. That is, those who received a GED, or any other equivalency certificate, are considered to not have a high school diploma (and to have dropped out). These results are also robust to using high school dropout and college enrollment estimates in wave 3 rather than wave 4.

[15]The AHAA study was funded by grants from the National Institute of Child Health and Human Development (01 HD40428-02) to the Population Research Center, University of Texas at Austin, Chandra Muller (PI), and from the National Science Foundation (REC-0126167) to the Population Research Center, University of Texas at Austin, Chandra Muller and Pedro Reyes (Co-PI).

[16]I also consider self-reported GPA using student's self-reported grade in four classes: history, English, science, and math. I assign a value of 4.0 for an A, 3.0 for a B, 2.0 for a C, and, following Sabia (2009), a 0.5 for a D or an F. I take the average of their GPA for those four courses to create the self-reported yearly GPA. This differs from the measure of GPA reported on the transcript, which contains all classes taken by the student, including electives, arts, etc. Results are robust to using self-reported GPA and are available upon request.

discussion of this method, please refer to Section 1.3. This method relies on bunching at zero hours worked per week to estimate the effect of selection, as evidenced by the notable bunching in Figure 2.1.

Figure 2.1. Empirical CDF of Hours Worked per Week



Note: The figure shows the estimated CDF of hours worked per week by gender. This figure uses the academic-year sample, as defined in Section 2.2.

I assume that each individual has an underlying type (or index of confounders), H^*, which is predictive of their choice of hours worked per week, H. In this framework, H^* is a selection variable and H is a treatment variable. While H is bound at zero, as one cannot work a negative number of hours, H^* has no such restriction. For ease of interpretation, H^* represents a value of indifference toward work, where higher (positive) values imply that an individual has a combination of factors that lead them toward being inclined to work and lower (negative) values imply a combination of factors that lead them to being inclined not to work. A value of $H^* = 0$ implies true indifference between not working and working a marginally small number of hours.[17] The factors contained within H^* could include individual preferences or constraints such as parental requirements, job availability, legal landscape, etc. that may impact the choice of work.

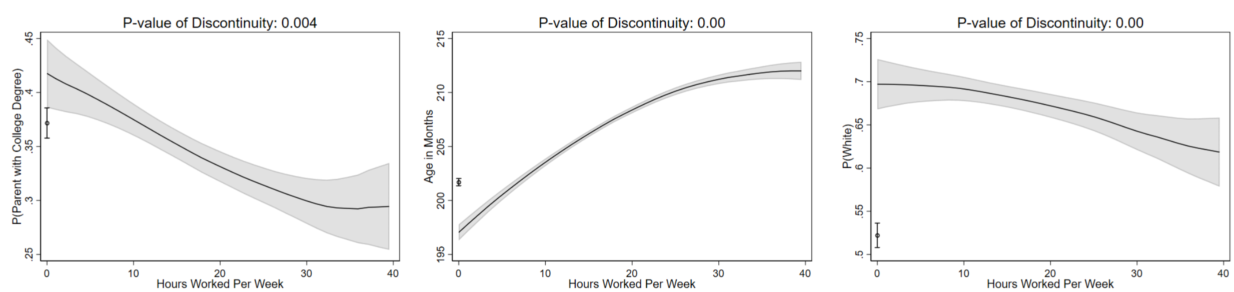
[17]Another way to consider H^* is in terms of shocks. For example, someone with an $H^* = 0$ would require a very small shock to be inclined to work a small number of hours, such as if their parents provide them an allowance to purchase gas to drive to work. Someone with an $H^* = -10$ may require a larger shock, such as their friend beginning work at a local establishment, motivating them to get a job at the same location. Furthermore, someone with $H^* = -30$ would require an even larger shock. For example, both parents being laid off from work, requiring the child to help support their parents in covering expenses.

For positive values of H, $H^* = H$. That is, for employed adolescents, the observed working hours reveal their underlying type. For example, a child observed working $H = 13$ hours a week must have an $H^* = 13$ that led them to work that number of hours.[18] By contrast, those at $H = 0$, who do not work, may be composed of multiple types: $H^* = 0$, who are indifferent between working and not working, and $H^* < 0$, who have varying levels of aversion to working. These negative types will still be observed at $H = 0$, along with those of $H^* = 0$ types, due to the non-negativity constraint of H.[19] Consequently, the average value of H^* at $H = 0$ will be negative. This results in a discontinuity of the average H^* at $H = 0$, such that children present at $H = 0$ are likely discontinuously different than those observed at marginally positive values of H.

The existence of $H^* < 0$ types is evidenced by the discontinuities at $H = 0$ present in Figure 2.2, which shows discontinuities in a set of key observables (age, race, and parental education) for females. The presence of these discontinuities implies that those at $H = 0$ are different on average from those at marginally positive values of H, likely because of the existence of the $H^* < 0$ types. Additionally, they are not only different in their observable characteristics, but also in their average values of outcomes, as shown in Figure 2.3. These discontinuities remain even after controlling for observables, as shown in Figure 2.4.[20]

Figure 2.2. Evidence of $H^* < 0$ Types (Female Subsample)

(a) P(College-Educated Parent) (b) Age (in months) (c) P(White)



Note: Each panel shows the local linear estimate of the expected value of the variable conditional on H, as well as the expected value of the variable for those at $H = 0$. 95% confidence intervals shown in gray. The *p*-value is for a test for whether there is a discontinuity at zero. The figure includes only females and uses the academic-year sample. Bandwidth=10.

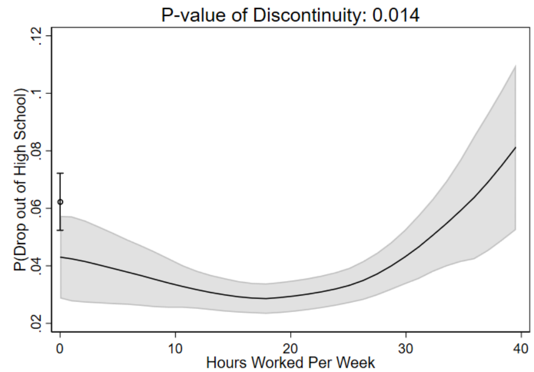
[18] Conceptually, while various children may be of a given type H^*, they could have a different combination of factors that result in them being that H type. The underlying values for the determinants of type H^* are unimportant for the methodology, as only the aggregated H^* index is considered in estimation.

[19] The rule regarding the relationship between H and H^* can be summarized by: $H = H^* \cdot \mathbf{1}(H^* \geq 0)$.

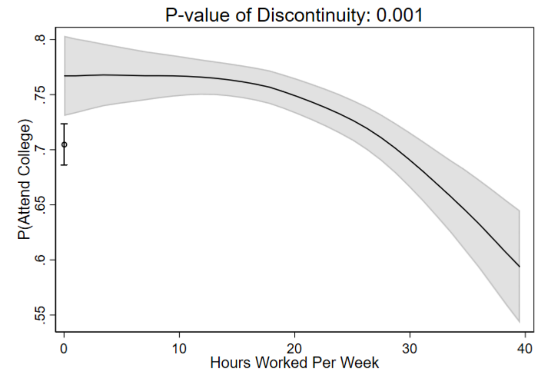
[20] Males also experience similar discontinuities, as shown in Figures B1 to B3.

Figure 2.3. Evidence of Selection at $H = 0$ (Female Subsample)

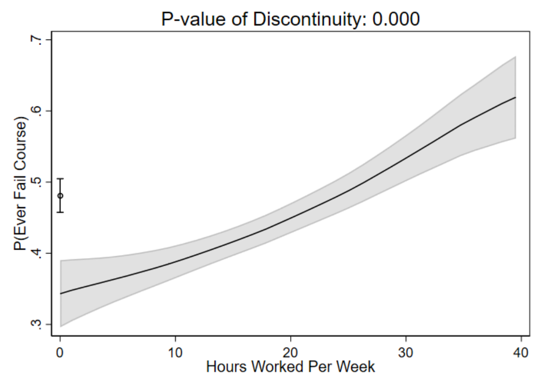
(a) P(High School Dropout)



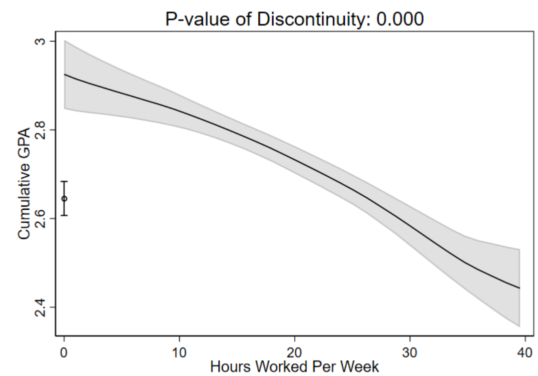
(b) P(College Enrollment)



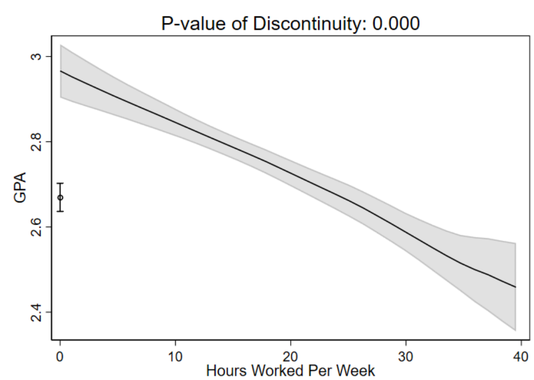
(c) P(Ever Fail a Course)



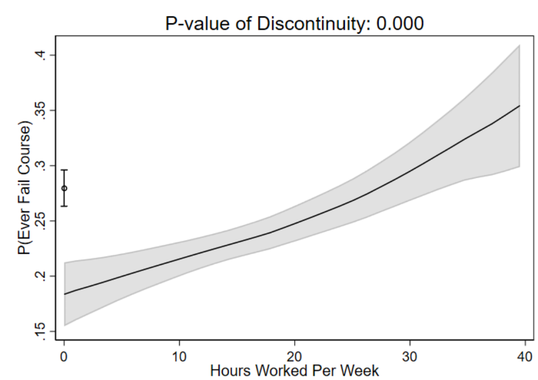
(d) Cumulative GPA



(e) Academic-Year GPA

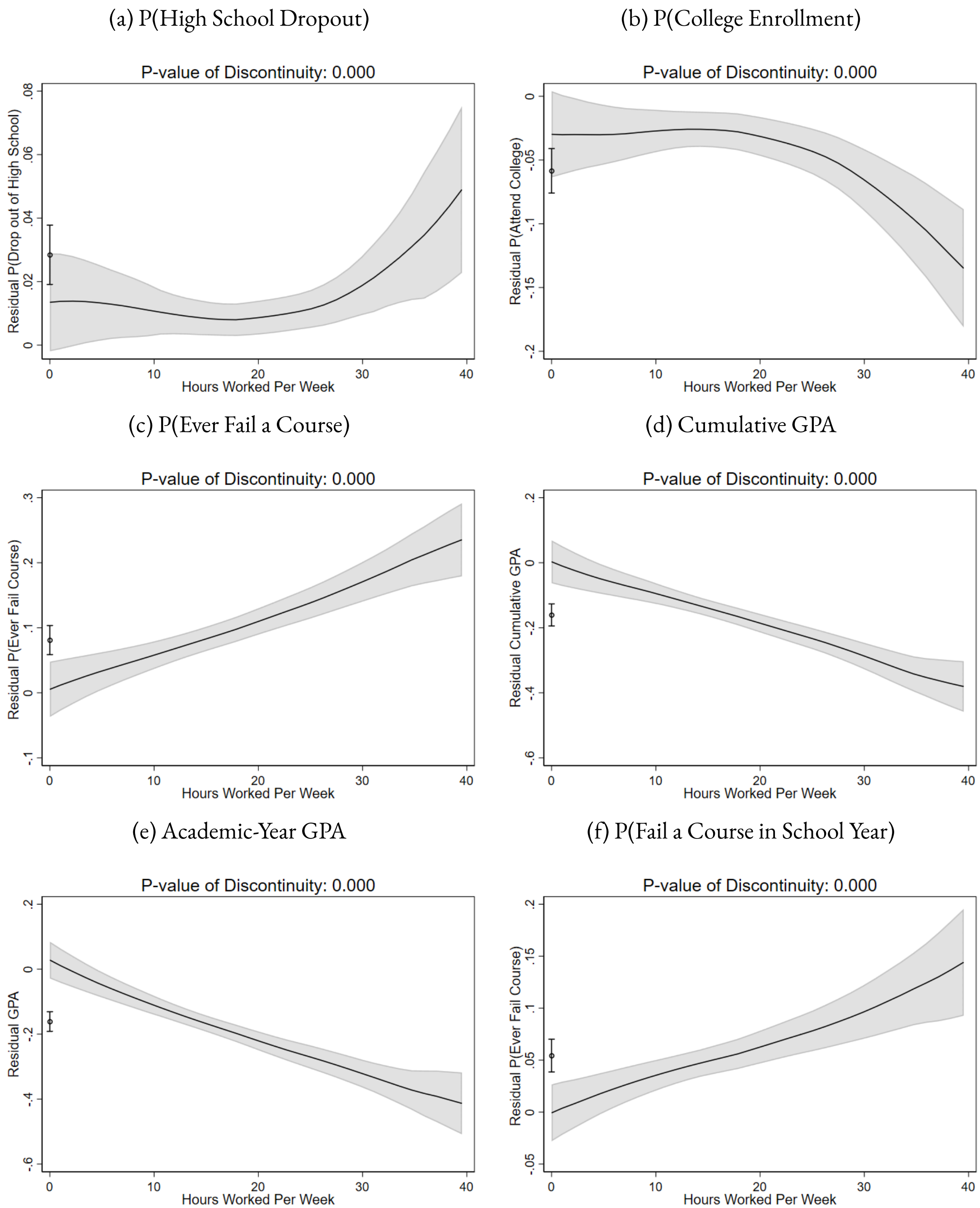


(f) P(Fail a Course in School Year)



Note: The figure shows local linear estimates of the expected value of educational outcomes conditional on the hours worked per week for $H > 0$ and the expected outcomes for $H = 0$. The p-value shown is for a test for whether there is a discontinuity at zero. 95% confidence intervals shown in gray. The figure includes only females and uses the academic-year sample. Bandwidth=10.

Figure 2.4. Evidence of Selection on Unobservables $H = 0$ (Female Subsample)



Note: The figure shows the local linear estimate of the residualized outcomes (controlling for race, age, parental education, siblings, Hispanic origin, urbanicity, and wave) for $H > 0$ and the residualized outcome for those who do not work ($H = 0$). The p-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. The figure includes only females and uses the academic-year sample. Bandwidth=10.

This method asserts that the reason for the discontinuity in Figure 2.4 is not the treatment effect, as working just a marginally positive number of hours per week should not result in such dramatic discontinuities in outcomes. Instead, the discontinuity in observed outcomes is attributed to the discontinuity in confounders, H^*. Therefore, the effect of selection can be estimated by taking a ratio of the discontinuity in outcomes and the discontinuity in confounders.

Figure 2.4 and Figure B3 provide insight into both the direction of selection and the qualitative findings. In Figure 2.4, individuals who work are positively selected for GPA and college enrollment, and negatively selected for high school dropout and course failure.[21] Furthermore, the slope of the plot represents the treatment effect, β, plus the selection effect, δ. Assuming the sign of δ estimated at $H = 0$ is the same at $H > 0$, we can determine the sign of the treatment effect. For cumulative and school-year GPA, the slope of the line is negative. The treatment effect will be equal to the negative slope minus the positive selection effect. For course failure, the treatment effect will be equal to the positive slope minus the negative selection effect. For high school dropout, the slope is fairly flat and then becomes positive. Given that selection is negative, the treatment effect should be positive. For college enrollment, the slope is flat and then becomes negative. As selection is positive, the treatment effect must be negative. Thus, without further estimation, I know that the effect of increased labor supply will be negative for GPA and college enrollment and positive for course failure and high school dropout.

To demonstrate this formally, consider a simple model relating outcome, Y, to treatment, H, and confounders, H^*, such that:

$$Y = \alpha + \beta H + \delta H^* + \epsilon, \quad E[\epsilon | H, H^*] = 0, \tag{2.1}$$

where β is the treatment effect of H and δ is the selection effect of the confounders contained within H^*. Technically, H^* is unobserved. However, H^* remains unidentified only for those of types $H^* \leq 0$, since at positive values of H, $H^* = H$. As a portion of H^* types are unobservable, I will proxy for H^* using a control function. The control function is derived from the relationship between H^* and H, $H = H^* \cdot \mathbf{1}(H^* \geq 0)$. I can rewrite this relationship as:

$$H^* = H + H^* \cdot \mathbf{1}(H = 0). \tag{2.2}$$

Substituting Equation 2.2 into Equation 2.1 and taking the expectation of Y given H provides a clearer representation of the control function approach. That is,

$$\mathbb{E}[Y|H] = \alpha + \beta H + \delta(H + \mathbb{E}[H^*|H = 0] \cdot \mathbb{1}(H = 0)), \tag{2.3}$$

where $H + \mathbb{E}[H^*|H = 0] \cdot \mathbf{1}(H = 0)$ is the control function for the unobserved H^*. Here it is clear that Y varies with H at a rate of $\beta + \delta$, but is discontinuous at $H = 0$ with a discontinuity of $\delta\mathbb{E}[H^*|H = 0]$.

[21] As shown in Figure B3, males are also negatively selected for high school dropout and positively selected for college enrollment. However, there is no evidence of selection for cumulative GPA or course failure.

In order to isolate the treatment effect, β, I will need to identify δ to correct for selection, requiring that I estimate $\mathbb{E}[H^*|H = 0]$.[22]

Importantly, as I do not observe H^*, I make assumptions about the distribution of H^* in order to estimate $\mathbb{E}[H^*|H = 0]$. I assume that the distribution of H^* is a semiparametric tobit distribution. However, results are robust to different distributional assumptions, and qualitative results do not rely on the specification of H^*. Once I have estimated $\mathbb{E}[H^*|H = 0]$, I can identify δ to indirectly identify the treatment effect. Note, that the methodology relies on one additional assumption: the selection effect estimated from those observed at $H = 0$ is the same for those at $H > 0$. That is, the selection effect of H^* is linear. While these assumptions are necessary to obtain point estimates, qualitative results rely on a weaker assumption: that the sign of δ at $H = 0$ is the same as the sign of δ at $H > 0$. Given these assumptions, I estimate the following model using OLS:

$$\mathbb{E}[Y|H, Z] = \beta H + Z'\tau + \delta[H + E[H^*|H = 0]\mathbb{1}(H = 0)], \tag{2.4}$$

where H is actual hours worked per week, H^* is the individual's type, and Z is a vector of observed characteristics, including sex, age-in-quarters dummies, race dummies, parental education dummies, grade dummies, an indicator for having siblings, being of Hispanic origin, attending a rural school, and survey wave. β is the average treatment effect of hours worked and δ is the selection effect of H^*.

2.4 Results

2.4.1 Main Results

Table 2.1 columns (1)-(4) show the effects of an additional hour worked per week on various cumulative academic outcomes, including the likelihood of high school dropout, college enrollment, ever failing a course, and cumulative GPA.[23,24] For females, for each additional hour worked per week, there is a 0.27-ppt increase in the likelihood of dropping out of high school, from a mean of 4%. There is also a 0.66-ppt decrease in the likelihood of enrolling in college, from a mean of 73.9%. I also observe detrimental effects from increased work hours on academic achievement, such that an additional hour increases the likelihood of ever failing a course by 1.13 ppt (from a mean of 44.7%) and decreases cumulative GPA by 0.0208 points (from a mean of 2.74).

[22] Intuitively, we can think of $\mathbb{E}[H^*|H = 0]$ as the average distance from indifference toward working for those who do not work.

[23] These results are robust to the inclusion of school fixed effects, as seen in Table B8. Inclusion of school fixed effects decreases the significance and magnitude of δ, which is sensible as the effect of selection based on school characteristics would have originally been contained within δ.

[24] Sample sizes vary across columns due to data availability requirements. Columns (1)-(4) use the cumulative outcomes sample. Furthermore, columns (1)-(2) use high school dropout and college enrollment data recorded in wave 4, while columns (3)-(4) are restricted to individuals with available wave 3 education data. Columns (5)-(6) also rely on transcript data from wave 3 but use the academic-year sample that includes all individual-wave observations, yielding larger samples.

For males, an additional hour worked per week leads to a 0.24-ppt increase in the likelihood of dropping out of high school (from a mean of 6.6%) and a decrease of 0.54 ppt in the likelihood of enrolling in college (from a mean of 65.3%). The effects on academic achievement for males are also notable, with a 0.46-ppt increase in the likelihood of ever failing a course and 0.0091-point decrease in cumulative GPA, from means of 56.8% and 2.46, respectively. While the effects across gender are generally similar, the effects on cumulative GPA are significantly larger for females, representing a 0.76% decrease from the mean compared to a 0.37% decrease from the mean for males.

Next, I estimate the effects on academic-year outcomes in columns (5) and (6). For females, an additional hour worked per week decreases yearly GPA by 0.0216 points and increases the likelihood of failing a course in that academic year by 0.68 ppt. These patterns are similar for males, with detrimental effects on yearly GPA and course failure of 0.0111 points and 0.37 ppt, respectively. Therefore, results suggest that increased labor supply is harmful to academic achievement, both yearly and cumulatively, and educational attainment.

Table 2.1. The Effect of Working Hours on Educational Outcomes

	Cumulative Outcomes				Academic-Year Outcomes	
	P(HS Dropout)	P(Enrolled in College)	P(Ever Failed Course)	GPA	GPA	P(Ever Failed Course)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Females						
β	0.0027***	-0.0066***	0.0113***	-0.0208***	-0.0216***	0.0068***
	(0.0010)	(0.0018)	(0.0022)	(0.0035)	(0.0032)	(0.0016)
δ	-0.0020***	0.0045***	-0.0062***	0.0121***	0.0115***	-0.0038***
	(0.0007)	(0.0012)	(0.0015)	(0.0024)	(0.0022)	(0.0011)
N	5600	5600	4332	4332	6581	6581
Mean	0.040	0.739	0.447	2.736	2.729	0.255
Panel B: Males						
β	0.0024**	-0.0055***	0.0046**	-0.0091**	-0.0111***	0.0037**
	(0.0011)	(0.0019)	(0.0023)	(0.0037)	(0.0032)	(0.0018)
δ	-0.0012	0.0025*	-0.0008	0.0015	0.0019	-0.0006
	(0.0008)	(0.0014)	(0.0017)	(0.0027)	(0.0023)	(0.0012)
N	4921	4921	3931	3931	6020	6020
Mean	0.066	0.653	0.568	2.457	2.433	0.352

Note: The table shows the effect of one additional hour of work per week on various educational outcomes. Controls include dummies for age-in-quarters, grade, race, maternal and paternal education, whether the individual has siblings, wave of interview, Hispanic origin, and urban vs. rural environment. Results are estimated using the semiparametric tobit distributional assumption. Bootstrapped standard errors in parentheses are based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

2.4.2 Robustness of Main Results

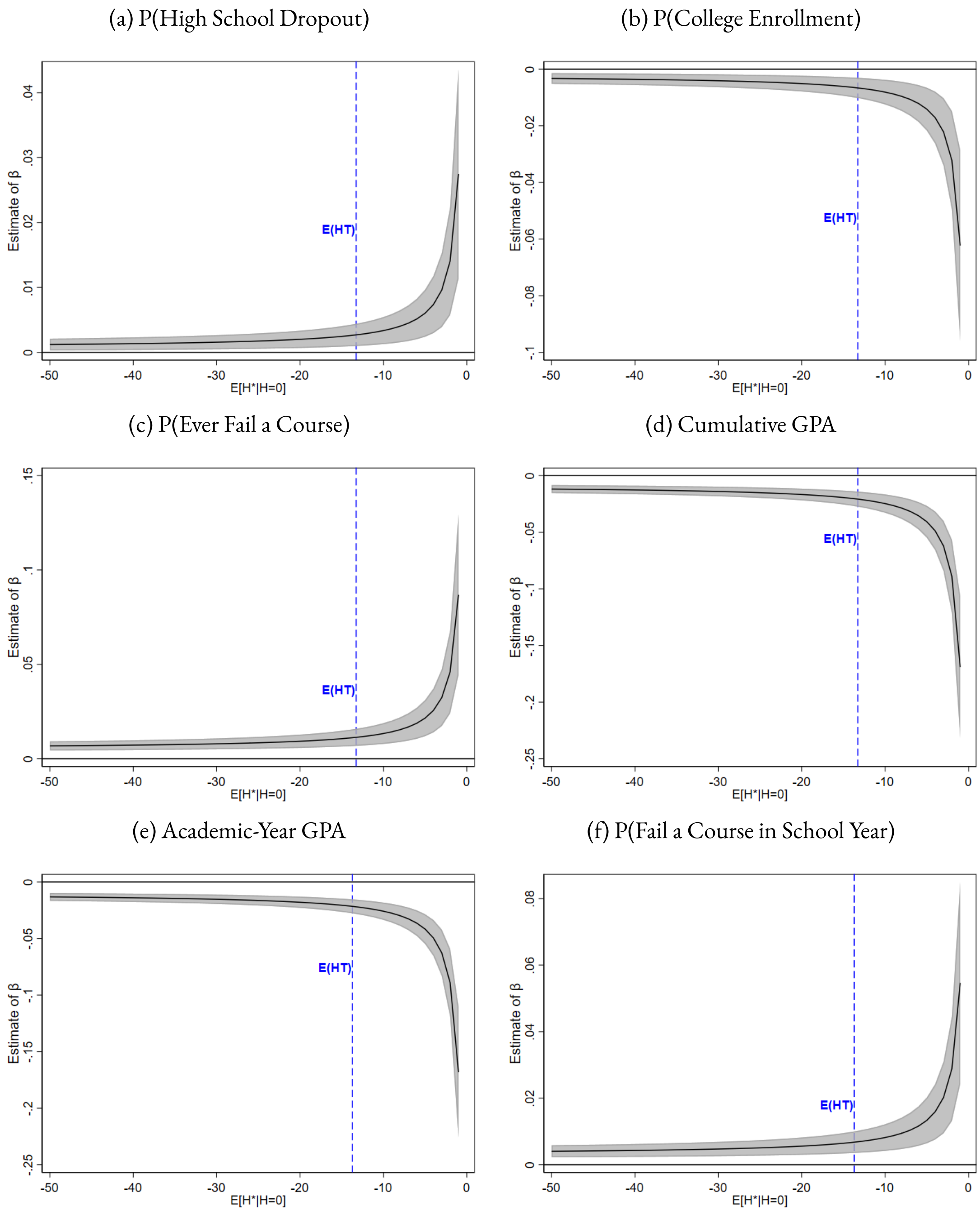
This section provides evidence supporting the testable assumptions of the Caetano et al. (2023) control function approach. First, I probe the distributional assumption, which assumes a distribution of the confounder H^* to obtain point estimates. Second, I probe the linearity assumption, which states that the effect of H^*, δ, which is estimated at $H = 0$, must be the same for values $H > 0$. I test each of these assumptions in turn.

Qualitative Robustness Distributional Assumption

To provide point estimates in Table 2.1, my empirical strategy requires assumptions regarding the distribution of the confounders, H^*. A distributional assumption allows me to estimate $\mathbb{E}[H^*|H = 0]$, which is included in the control function, as seen in Equation 2.4. Alternatively, I can guess values of $\mathbb{E}[H^*|H = 0]$ to obtain qualitative results. Therefore, I test a range of values for $\mathbb{E}[H^*|H = 0]$ from -1 to -50. Figure 2.5 and Figure 2.6 present the distribution of estimates of β, shown by the black line, obtained when assuming various guesses of $\mathbb{E}[H^*|H = 0]$.

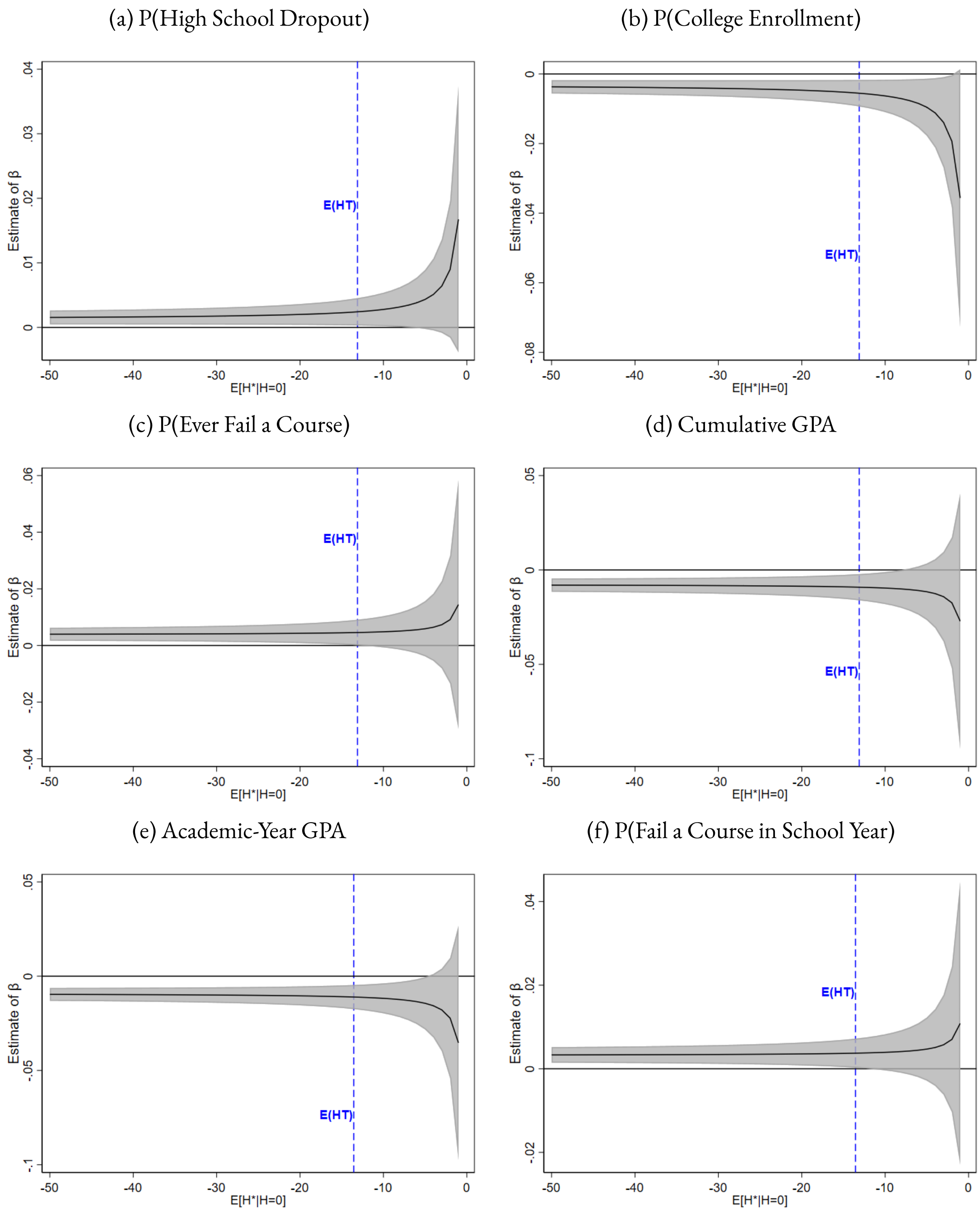
Figure 2.5 shows that for the female sample, qualitative results do not rely on the distribution assumption. Regardless of the value of $\mathbb{E}[H^*|H = 0]$, an additional hour of work always results in increased high school dropout, decreased college enrollment, increased course failure, and decreased GPA. The results are less precise for males, as seen in Figure 2.6. The signs remain consistent across the range of $\mathbb{E}[H^*|H = 0]$, but as the assumed values for $\mathbb{E}[H^*|H = 0]$ approach zero, the estimates become statistically insignificant. However, I likely could not reject that these estimates are the same as those at larger values of $\mathbb{E}[H^*|H = 0]$. Both figures suggest that the main results are fairly robust to misspecifications of the distribution of H^*. Note that the dotted vertical line represents the estimated $\mathbb{E}[H^*|H = 0]$ obtained when using the semi-parametric tobit assumption. Therefore, the figures show that the point estimates of β provided in Table 2.1 are conservative, and I have a greater likelihood of underestimating the magnitude of effects rather than overestimating them.

Figure 2.5. Estimates of β Given Counterfactual Values of $\mathbb{E}[H^*|H=0]$ (Female Subsample)



Note: Each panel shows the $\hat{\beta}$ for each sample, given counterfactual values of $\mathbb{E}[H^*|H=0]$ ranging from -1 to -50. The blue dashed line shows the $\mathbb{E}[H^*|H=0]$ for the semiparametric tobit distribution. 95% confidence intervals shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

Figure 2.6. Estimates of β Given Counterfactual Values of $\mathbb{E}[H^*|H=0]$ (Male Subsample)



Note: Each panel shows the $\hat{\beta}$ for each sample, given counterfactual values of $\mathbb{E}[H^*|H=0]$ ranging from -1 to -50. The blue dashed line shows the $\mathbb{E}[H^*|H=0]$ for the semiparametric tobit distribution. 95% confidence intervals shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

Qualitative Robustness to the Linearity Assumption

The linearity assumption requires that the estimated selection effect, δ, be the same for all values of H^*.[25] As I add in observable controls, this assumption should become weaker. There are two tests for the validity of the linear assumption. First, following Caetano et al. (2023), I examine how estimates of β change with various truncations of the sample. I re-estimate the model for every version of the underlying sample that works $H \leq H_{max}$ hours per week, where H_{max} gets progressively larger.[26] If the linearity assumption does not hold, then smaller values of $H^* \leq H_{max}$ will result in different conclusions than larger values of $H^* \leq H_{max}$. Figure 2.7 and Figure 2.8 showcase how the estimates of β change using different truncated samples for females and males, respectively. The estimates for samples using small values of H_{max} are imprecise, especially in some panels of Figure 2.7, but stabilize around $H = 10$ to $H = 15$. Although the estimates from lower values of H_{max} are noisy, the overall qualitative results remain fairly consistent across the truncated samples.[27]

The second test of the linearity assumption involves altering the controls used in the estimation (Caetano et al., 2023; 2025). I assign individuals to K discretized clusters, $\hat{C}_K$, based upon their observable characteristics. The clusters enter the estimating equation in two ways. First, I include indicators for each cluster. Second, the expectation of H^* will now vary by clusters and is denoted as $E[H^*|H = 0, \hat{C}_K]$. Therefore, I can rewrite the estimating equation from Equation 2.4 as

$$\mathbb{E}[Y|H, Z] = \beta H + Z'\tau + \sum_{k=1}^{K} \alpha_k \mathbb{1}(Z \in \hat{C}_k) + \delta[H + E[H^*|H = 0, \hat{C}_K]\mathbb{1}(H = 0)]. \tag{2.5}$$

Including clusters allows for a more flexible specification of the controls, which weakens the linearity assumption. As explained in Caetano et al. (2024a), the indicators for clusters, $\mathbb{1}(Z \in \hat{C}_K)$, absorb all of the across-cluster variation, leaving $Z'\tau$ to absorb within-cluster variation. As the number of clusters increase, the observations within each cluster naturally become more similar. Therefore, increasing the number of clusters should continue to weaken the linearity assumption, as $Z'\tau$ has less variation to absorb. As shown in Figure 2.9 and Figure 2.10, increasing the number of clusters from K=1 to K=50 does not change the results.[28] That is, allowing for a more flexible, non-parametric specification of controls in a way that should capture potential non-linearities does not alter the results. Therefore, these figures support the validity of the linearity assumption in this application.

[25] Formally, the basic outcome equation is $Y = \beta H + U$. The linearity assumption is written as: $\mathbb{E}[U|H^*] = \alpha + \delta H^*$, producing Equation 2.1, which allows separate modeling of β and δ.

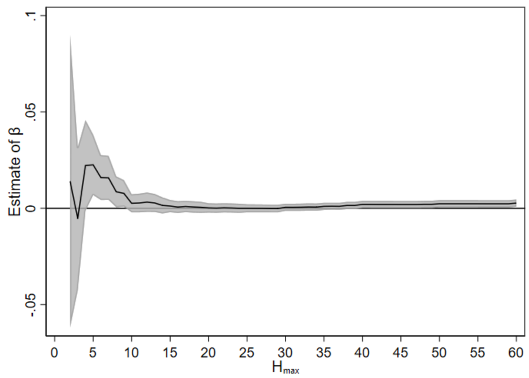
[26] When $H_{max} = 60$, the estimates are equivalent to those shown in Table 2.1.

[27] In most cases, I cannot reject that the qualitative results remain the same as I increase H_{max}. However, estimates for the effects on GPA in Figure 2.7 are notably more negative when $H_{max} < 15$, possibly indicating non-linear treatment effects.

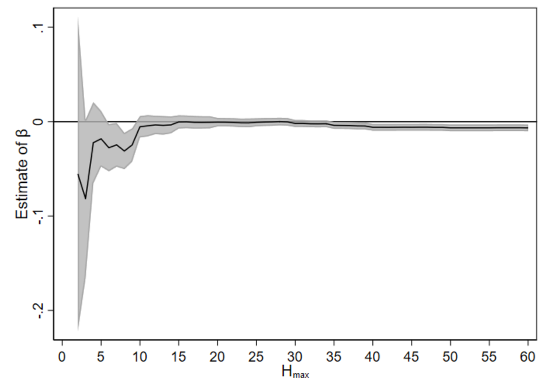
[28] Table 2.1, as well as all other reported results in this chapter, use $K = 1$ cluster.

Figure 2.7. Estimates of β for Truncated Samples of $H \leq H_{max}$ (Female Subsample)

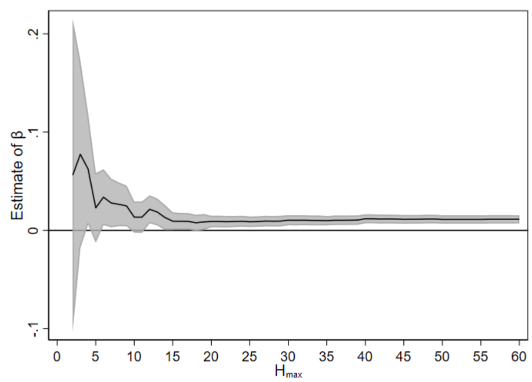
(a) P(High School Dropout)



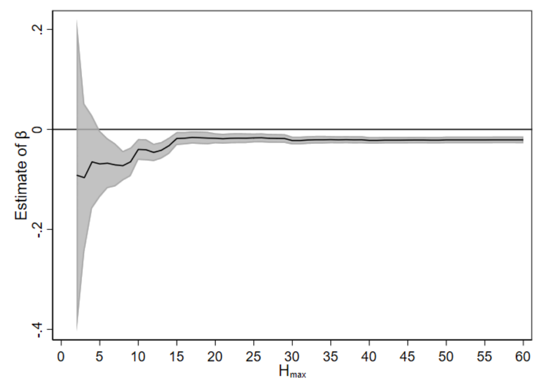
(b) P(College Enrollment)



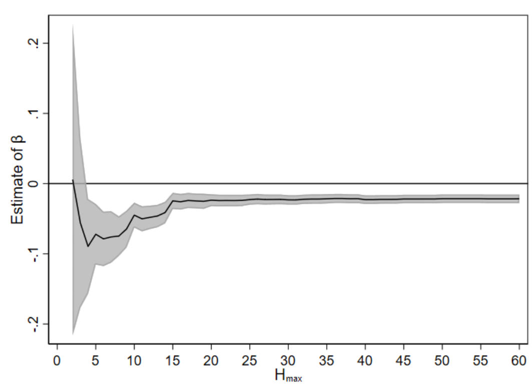
(c) P(Ever Fail a Course)



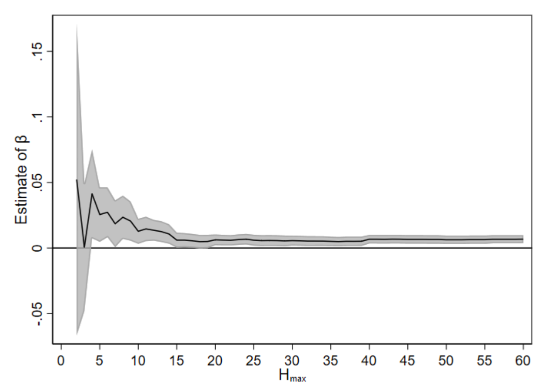
(d) Cumulative GPA



(e) Academic-Year GPA

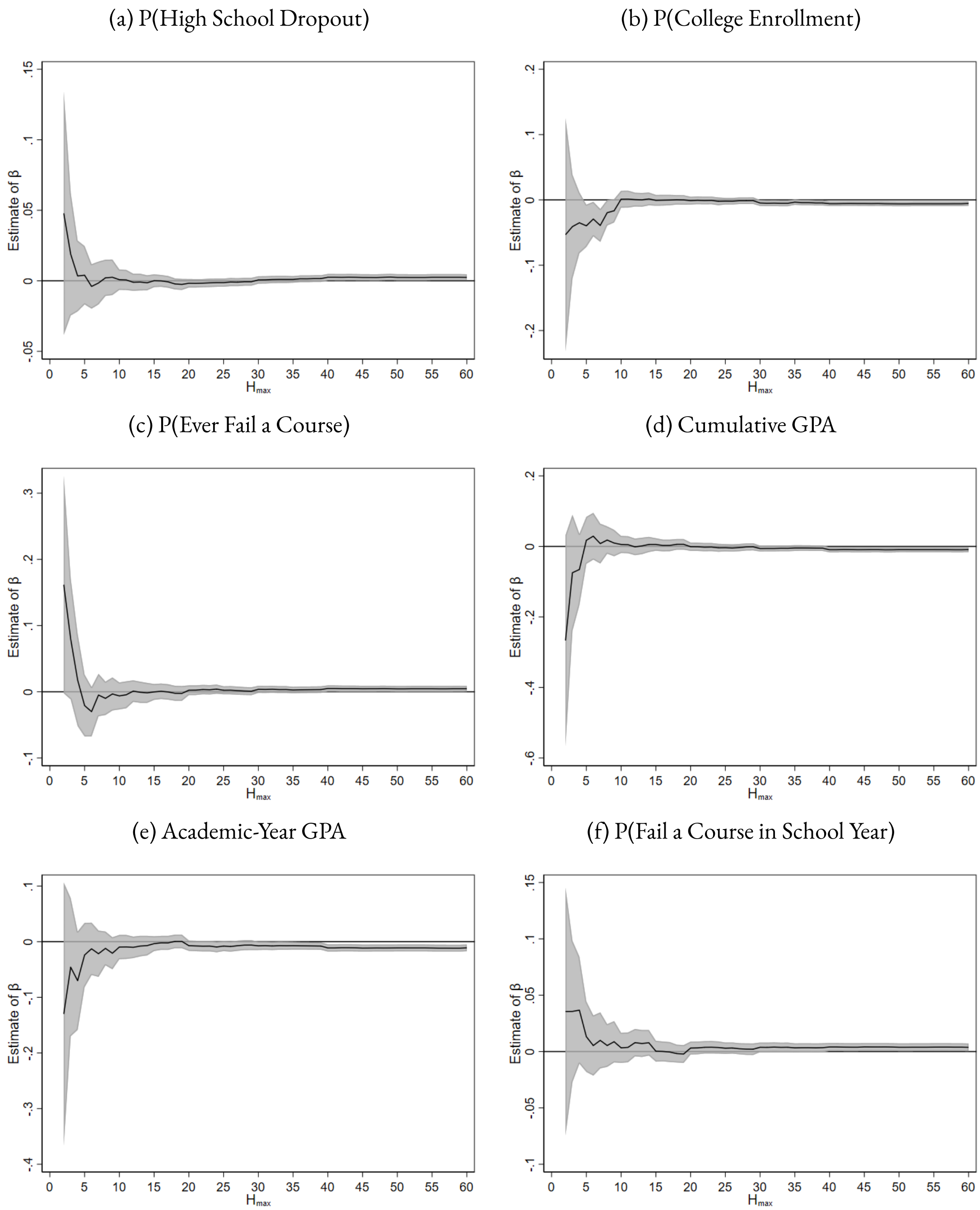


(f) P(Fail a Course in School Year)



Note: The figure plots the estimates of β for truncated samples such that $H \leq H_{max}$. Estimates are shown for $H_{max} \geq 2$. Bootstrapped 95% confidence intervals based upon 100 iterations are shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

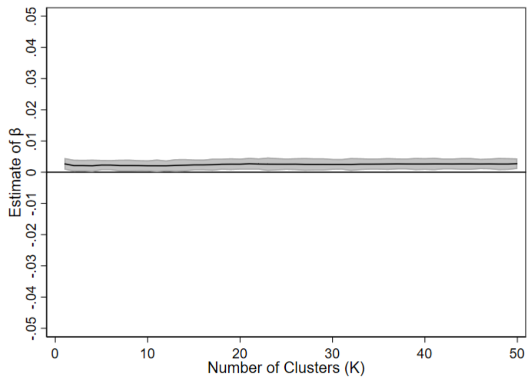
Figure 2.8. Estimates of β for Truncated Samples of $H \leq H_{max}$ (Male Subsample)



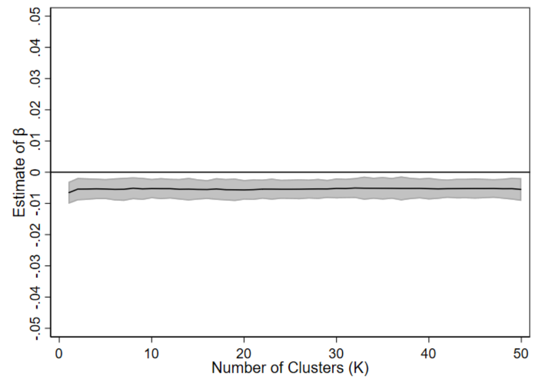
Note: The figure plots the estimates of β for truncated samples such that $H \leq H_{max}$. Estimates are shown for $H_{max} \geq 2$. Bootstrapped 95% confidence intervals based upon 100 iterations are shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

Figure 2.9. Estimates of β for Varying Number of Clusters (Female Subsample)

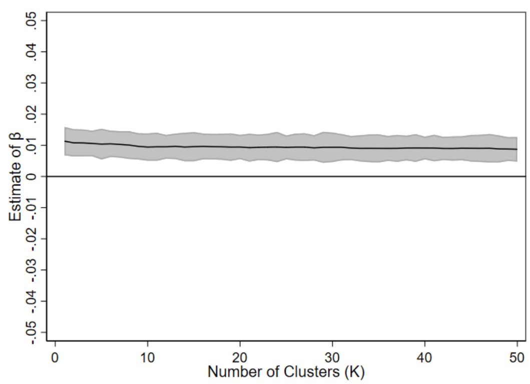
(a) P(High School Dropout)



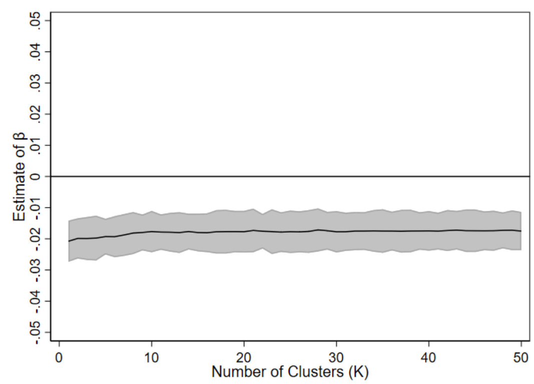
(b) P(College Enrollment)



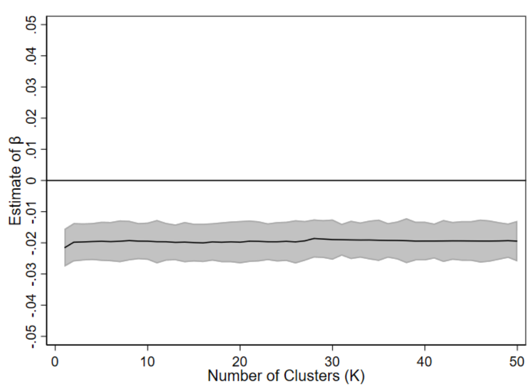
(c) P(Ever Fail a Course)



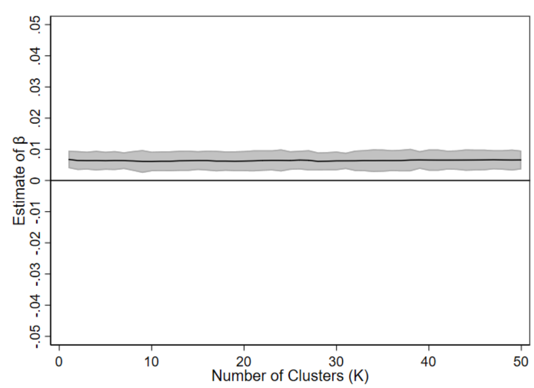
(d) Cumulative GPA



(e) Academic-Year GPA

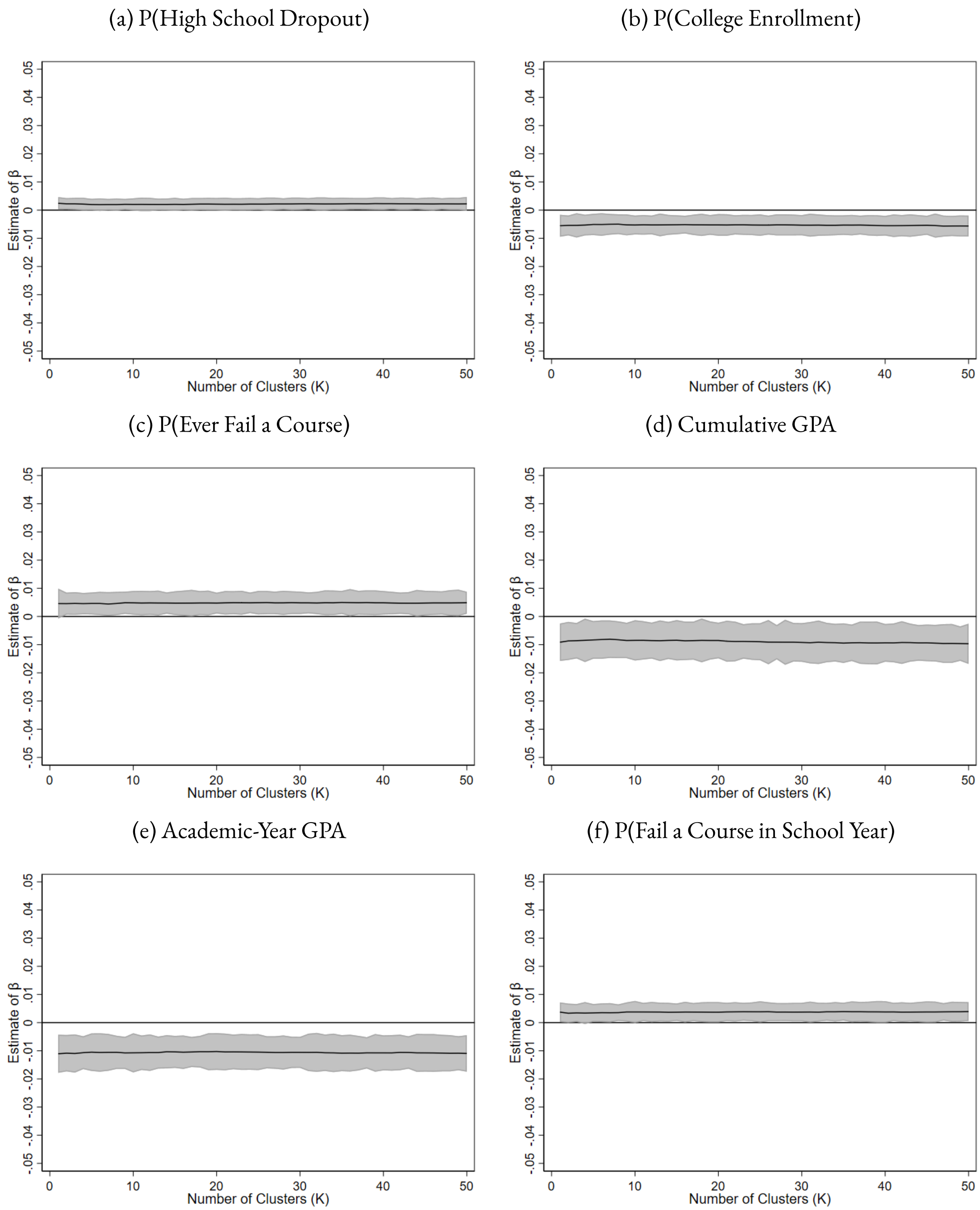


(f) P(Fail a Course in School Year)



Note: The figure plots the estimates of β for increasing number of clusters. Bootstrapped 95% confidence intervals based upon 100 iterations are shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

Figure 2.10. Estimates of β for Varying Number of Clusters (Male Subsample)



Note: The figure plots the estimates of β for increasing number of clusters. Bootstrapped 95% confidence intervals based upon 100 iterations are shown in gray. Panels (a)-(d) use the cumulative outcomes sample while panels (e)-(f) use the academic-year sample described in Section 2.2.

2.4.3 Heterogeneous Effects of Hours Worked on Education

The effects of intensive-margin employment may vary by individual characteristics. In this section, I estimate the models separately by race, ethnicity, urbanicity, and parental education. These factors may be associated with differences in how adolescents value employment, education, and the related trade-offs. These characteristics may also relate to the types of occupations, work environments, and peers accessible to students.[29]

I first consider differences by parental education, specifically whether an adolescent has at least one college-educated parent, which is likely associated with family socioeconomic status and parental expectations regarding education. For students with highly educated parents, educational expectations may affect students' attitudes toward work and employment. It could be that students with highly educated parents may choose to work fewer hours so as not to disrupt their academics. They could also have different time substitution patterns such that employment primarily crowds out leisure rather than time devoted to academic achievement. If these students are particularly focused on building academic skills or preparing resumes for college admissions, they may choose occupations that complement their academic goals. Additionally, to the extent that parental education correlates with socioeconomic status, students may work for different reasons, such as financially supporting their family.

I also examine heterogeneity by urbanicity, defined as whether a student attends an urban or rural school.[30] Adolescents likely have access to different types of occupations dependent on where they live. For example, those living in urban and suburban areas may have more access to retail occupations compared to those in rural areas, who may have more access to agricultural occupations. The characteristics of occupations may differentially impact educational outcomes. For instance, agricultural jobs likely involve manual labor in the heat, which may be exhausting. Communities may also have differing expectations surrounding employment and education, access to academic resources, and quality and quantity of nearby colleges, depending on where they are located.

Lastly, I consider heterogeneous effects by race and ethnicity. It is possible that race and ethnicity are correlated with different familial and cultural attitudes towards the value of work and school, including expectations surrounding high school completion and receipt of post-secondary degrees. Differences in these expectations may alter youth preferences and affect how they balance employment and education. Additionally, there may be differences in the types of occupations available to students by race and ethnicity, as discrimination may preclude minority students from some occupations. These differences are particularly important if non-minority students have differential access to occupations that either allow for a better school-work balance or provide training in skills that are complementary to academic success.

Figures 2.11 to 2.13 show significant effect heterogeneity of an additional hour worked per week on various educational outcomes across demographic groups. Overall, for both cumulative and yearly outcomes, there is notable effect heterogeneity by race, such that White students see larger detrimental impacts than Black students. Specifically, White females experience larger harmful effects on high school dropout, col-

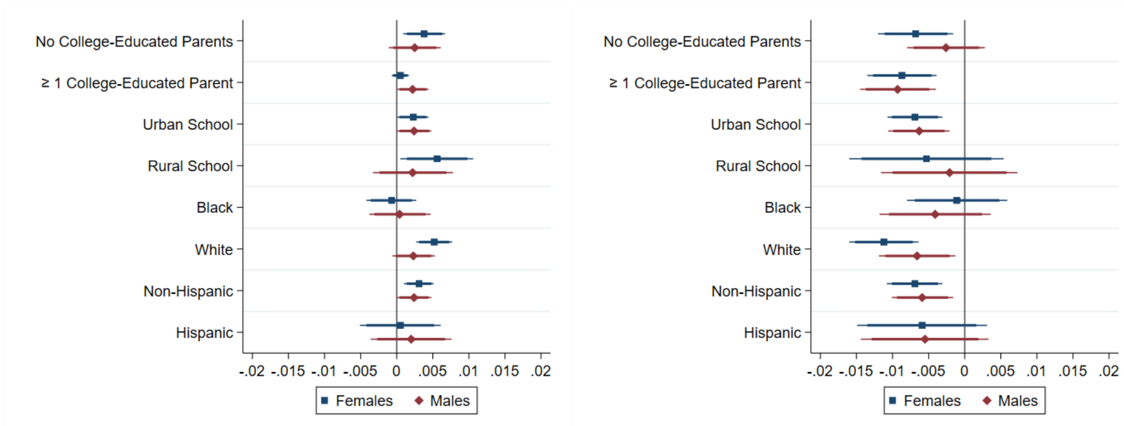
[29] Section 1.4.3 contains a similar discussion regarding differences by these characteristics, in the context of mental health. The discussion below will follow closely, as much of the reasoning remains the same.

[30] The urban designation contains both urban and suburban schools.

lege enrollment, and GPA compared to Black females. Females with less-educated parents see greater harmful impacts on the probability of high school dropout. Patterns weakly suggest that students in rural schools may face more detrimental impacts from increased labor supply, although these differences are often not statistically significant. There is little evidence of effect heterogeneity by Hispanic ethnicity.

Figure 2.11. Heterogeneous Effects of Working Hours on High School Dropout and College Enrollment

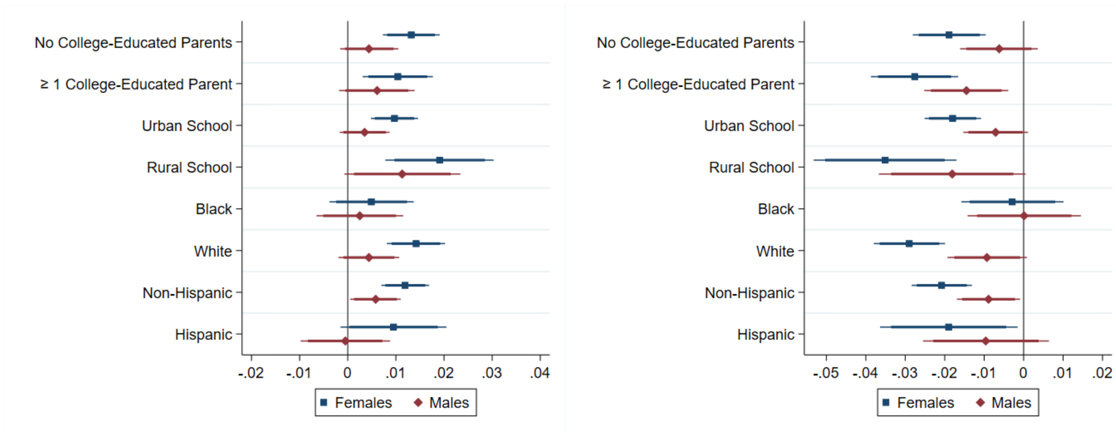
(a) P(High School Dropout) (b) P(College Enrollment)



Note: The figure shows the effects of one additional hour of work on the probability of dropping out of high school and enrolling in college for different groups categorized by parental education, urbanicity of the attended school, race, and Hispanic origin. The figure uses the cumulative outcomes sample, as defined in Section 2.2. Results are estimated using the semiparametric tobit distributional assumption. I plot 90% and 95% confidence intervals based on bootstrapped standard errors from 500 iterations.

Figure 2.12. Heterogeneous Effects of Working Hours on Cumulative GPA and Course Failure

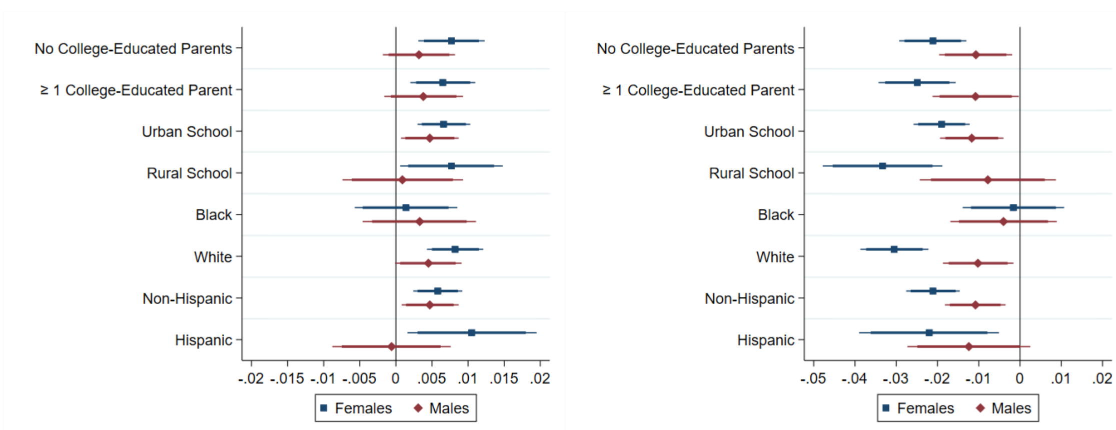
(a) P(Ever Fail a Course) (b) Cumulative GPA



Note: The figure shows the effects of one additional hour of work on cumulative GPA and the probability of ever failing a course for different groups categorized by parental education, urbanicity of the attended school, race, and Hispanic origin. The figure uses the cumulative outcomes sample, as defined in Section 2.2. Results are estimated using the semiparametric tobit distributional assumption. I plot 90% and 95% confidence intervals based on bootstrapped standard errors from 500 iterations.

Figure 2.13. Heterogeneous Effects of Working Hours on Yearly GPA and Course Failure

(a) P(Fail a Course in School Year) (b) Academic-Year GPA



Note: The figure shows the effects of one additional hour of work on yearly GPA and the probability of ever failing a course in a given academic year for different groups categorized by parental education, urbanicity of the attended school, race, and Hispanic origin. The figure uses the academic-year sample, as defined in Section 2.2. Results are estimated using the semiparametric tobit distributional assumption. I plot 90% and 95% confidence intervals based on bootstrapped standard errors from 500 iterations.

2.4.4 Effects of Summer Labor Supply

Next, I consider the effects of summer labor supply on educational outcomes, which could differ from the effects of academic-year labor supply. Summer employment does not necessarily crowd out study time and time to engage in academic endeavors. Nevertheless, some students may still engage in academic activities throughout the summer. Students may choose to take preparatory courses for standardized tests, attend summer programs at universities, receive tutoring, or take required summer classes. Therefore, there is still an avenue by which employment may crowd out time spent on academics, albeit to a lesser degree than during the school year. Additionally, if the impact of labor supply on educational outcomes operates through risky behaviors, harmful peer groups, or changes in preferences toward education, then summer employment could be as impactful as employment during the academic year. On the other hand, working during the summer may allow adolescents to reap the benefits of employment without compromising academic focus as students can glean complementary skills that may assist in academics and build up their resume.

I consider the effects of increased working hours during the summer on cumulative outcomes, including high school dropout, college enrollment, cumulative GPA, and the probability of ever failing a course.[31] I also examine how working during the summer impacts outcomes in the following academic year by linking summer work hours to outcomes for the following school year.[32] Table 2.2 shows the estimated effects of increased summer labor supply on cumulative academic outcomes. Interestingly, there is no statistically significant effect on high school dropout. For females, an additional hour of work per week during the summer results in a 0.43-ppt decrease in the probability of enrolling in college, 0.92-ppt increase in the probability of ever failing a course, and a 0.0141-point decrease in cumulative GPA. There are also impacts on academic achievement in the following year, such that the probability of failing a course in that academic year increases by 0.62 ppt. These patterns persist for males as well. An additional hour worked per week results in a 0.36-ppt decrease in the probability of enrolling in college, 0.76-ppt increase in the probability of ever failing a course, and a 0.0113-point decrease in cumulative GPA. For outcomes in the following academic year, males see a decrease of 0.0131 points in yearly GPA. Compared to working during the academic year, working during the summer has smaller harmful effects. This is most notable for females, for whom the effects on college enrollment and GPA are smaller during the summer months. These differences are smaller for males, who are impacted similarly during the summer and academic year.

[31] For these outcomes, I follow the same procedure for sample construction as described in Section 2.2.

[32] Using the information derived from their transcripts, I connect the wave 1 summer hours (summer 1995) to 1995-96 school year outcomes and wave 2 summer hours (summer 1996) to the 1996-97 school year outcomes. For this analysis, I only include individuals who respond to the survey in May through August and report that school is not currently in session.

Table 2.2. The Effect of Working Hours During Summer on Educational Outcomes

	Cumulative Outcomes				Academic-Year Outcomes	
	P(HS Dropout)	P(Enrolled in College)	P(Ever Failed Course)	GPA	GPA	P(Ever Failed Course)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Females						
β	0.0009	-0.0043**	0.0092***	-0.0141***	-0.0060	0.0062***
	(0.0008)	(0.0017)	(0.0022)	(0.0031)	(0.0046)	(0.0023)
δ	-0.0010	0.0041***	-0.0075***	0.0114***	0.0043	-0.0047***
	(0.0006)	(0.0012)	(0.0016)	(0.0023)	(0.0035)	(0.0017)
N	5565	5565	4304	4304	2228	2521
Mean	0.040	0.739	0.448	2.736	2.759	0.251
Panel B: Males						
β	0.0016	-0.0036*	0.0076***	-0.0113***	-0.0131**	0.0045
	(0.0011)	(0.0020)	(0.0024)	(0.0038)	(0.0058)	(0.0031)
δ	-0.0014	0.0027*	-0.0061***	0.0091***	0.0093**	-0.0032
	(0.0009)	(0.0015)	(0.0019)	(0.0030)	(0.0047)	(0.0025)
N	4844	4844	3874	3874	2003	2276
Mean	0.065	0.654	0.567	2.46	2.436	0.357

Note: The table shows the effect of one additional hour of work per week during the summer on various educational outcomes. Controls include dummies for age-in-quarters, grade, race, maternal and paternal education, whether the individual has siblings, wave of interview, Hispanic origin, and urban vs. rural environment. Results are estimated using the semiparametric tobit distributional assumption. Bootstrapped standard errors in parentheses are based on 500 iterations. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

2.4.5 Student Engagement and Expectations

Given the harmful effects of additional working hours on educational outcomes, it is important to consider the impacts of working on student engagement and expectations toward college. Changes in engagement and expectations may underlie the effects of increased labor supply on educational outcomes. Following Sabia (2009), I consider five possible measures of student engagement and expectations. I examine the number of days of school skipped in an academic year, whether students have difficulty completing homework at least once per week, and whether students have difficulty paying attention in class at least once per week. Columns (1)-(3) of Table 2.3 present the effects of an additional hour worked per week on measures of student engagement. Both males and females see an increase in the number of unexcused absences per year. An additional hour worked per week results in a 0.0958-day increase for females and a 0.1465-day increase for males. Increased working hours do not significantly impact difficulty completing homework or paying attention.

Columns (4) and (5) of Table 2.3 present the effects on student preferences and expectations for college enrollment, which are measured on a scale of 1 to 5 from low expectations and desires (1) to high expectations and desires (5). Especially for females, increased labor supply alters expectations and preferences for attending college. An additional hour worked per week results in a 0.24% decrease from the mean and a 0.39% decrease from the mean in the desire to go to college and the self-reported likelihood of enrolling in college, respectively. Increased labor supply does not alter college enrollment expectations for males, but does decrease the self-reported desire to go to college by 0.16% from the mean.

Table 2.3. The Effect of Working Hours on Student Engagement and Expectations

	Days of School Skipped	P(Trouble Finishing Homework)	P(Trouble Paying Attention)	Wants to Go To College	Likely To Go To College
	(1)	(2)	(3)	(4)	(5)
Panel A: Females					
β	0.0958***	0.0020	0.0014	-0.0109***	-0.0168***
	(0.0230)	(0.0015)	(0.0014)	(0.0029)	(0.0034)
δ	-0.0470***	-0.0007	-0.0002	0.0077***	0.0113***
	(0.0147)	(0.0010)	(0.0009)	(0.0019)	(0.0022)
N	10610	10624	10624	10575	10568
Mean	2.272	0.273	0.281	4.505	4.316
Panel B: Males					
β	0.1465***	0.0011	0.0005	-0.0067**	-0.0043
	(0.0258)	(0.0014)	(0.0013)	(0.0032)	(0.0033)
δ	-0.0697***	0.0000	0.0005	0.0029	0.0014
	(0.0166)	(0.0010)	(0.0009)	(0.0022)	(0.0023)
N	10329	10347	10347	10309	10302
Mean	2.835	0.369	0.345	4.292	4.002

Note: The table shows the effect of one additional hour of work per week during the school year on student engagement and expectations. Controls include dummies for age-in-quarters, grade, race, maternal and paternal education, whether the individual has siblings, wave of interview, Hispanic origin, and urban vs. rural environment. This table uses the academic-year outcomes sample which includes all individual-wave observations, as described in Section 2.2. Results are estimated using the semiparametric tobit distributional assumption. Bootstrapped standard errors in parentheses are based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

2.5 Discussion

In this section, I discuss how my estimated average treatment effects of hours worked on educational outcomes compare to results in the literature. Prior work has primarily relied on instrumental variable strategies that can recover LATE estimates or potentially biased within-person variation approaches. Using the novel control function approach, I exploit a different source of variation to address endogeneity and recover the ATE. Broadly, I find that additional working hours are harmful to educational outcomes. I find negative impacts across genders for both academic achievement and educational attainment outcomes, which is consistent with the broad consensus in the literature that youth labor has non-positive effects on educational outcomes. Beyond exploring the effects of increased work during the academic year on educational outcomes, I examine effect heterogeneity and the effects of labor supply during the summer.

I find evidence that increased intensive-margin employment during the school year is more harmful for White adolescents than Black adolescents. Specifically, I estimate pronounced differences in effects on educational attainment and academic outcomes between White females and Black females. I find greater increases in the likelihood of high school dropout, larger decreases in the likelihood of college enrollment, and larger declines in GPA measures for White females relative to Black females. These differences are not as pronounced for males. Though the point estimates for Black males are smaller than those of White males, the effects are not statistically different from each other.

My findings regarding heterogeneous effects on GPA contradict some of the limited findings in the literature. To my knowledge, only a few papers explore heterogeneous effects of employment on educational outcomes. These papers do not consider additional outcomes such as high school dropout, college enrollment, or course failure, so current knowledge of effect heterogeneity for these outcomes is more limited. Importantly, the methods, underlying samples, and outcomes studied in these studies differ from those used in this paper.

Oettinger (1999) exploits within-person variation and switching behavior in working hours and number of weeks worked per academic year between 11^{th} and 12^{th} grade to estimate the effects of work intensity on yearly GPA. He finds that working is predominantly harmful for Black and Hispanic students, especially those working intensive hours, compared to White students. There is little racial difference for those working few hours per week. DeSimone (2006) examines the effects of work intensity on cumulative GPA for a sample of high school seniors and instruments for work intensity using indicators of other potential sources of income, such as allowance. He finds that GPA is affected more by work intensity for racial minority students compared to White students. Particularly, working is more beneficial for non-White students at low levels of work intensity and more harmful for non-White students at high levels of work intensity. Contrary to my findings, both papers imply that increased work intensity can be more harmful to GPA for non-White students, especially for those working many hours per week.[33] The differences in results may be driven by differences in methodology and treatment. For example, Oettinger (1999) relies on a fixed-effects approach. Additionally, rather than using hours as a continuous measure, Oettinger (1999) creates categories that capture combinations of how many weeks per academic year and how many hours per week each individual works.[34] Therefore, Oettinger (1999) is not capturing the effects of a single hour of work, but rather the effects of changes in employment status encompassing work weeks and hours worked per week. DeSimone (2006), on the other hand, includes hours as a continuous measure. However, he utilizes an IV approach, instrumenting for hours worked with categories of unearned income. Therefore, he estimates the LATE, rather than the ATE.

[33]While these two papers explore heterogeneity in causal effects of working, the literature also reports descriptive evidence of differences by race. In general, descriptive evidence implies that the negative association between adolescent work and academic outcomes is worse among White students (e.g., Bachman et al., 2013; Hwang and Domina, 2017).

[34]There are six categories that are combinations of working 1-13 weeks, 14-25 weeks, and 26 or more weeks and working 1-20 hours per week and more than 20 hours per week.

I also examine the effects of summer employment on educational outcomes. In general, I find that increased labor supply during the summer is harmful to academic achievement and college enrollment. It does not affect dropout decisions, which is unsurprising if dropout is driven by difficulties in balancing school and work specifically. Overall, the magnitude of the effects of an additional hour worked per week during the summer are smaller than during the school year. My findings on summer employment contrast with findings in the limited literature on the effects of summer employment on educational outcomes, which typically finds non-negative effects (e.g., Oettinger, 1999; Baert et al., 2022).[35] These differences can likely be attributed to how I define summer work and how summer work enters the estimation. For example, Oettinger (1999) estimates the effects of summer employment on GPA by including an indicator for any summer employment in the preceding summer in the model of the effects of school-year employment. Baert et al. (2022) compares the effects of extensive-margin summer employment to extensive-margin employment during both the summer and school year on school dropout and college enrollment among Belgian secondary school students. Both find that summer work does not have harmful educational impacts. My approach to comparing the effects of labor supply during the summer and school year is most similar to that of Lee and Orazem (2010), who estimate the effects of both summer and school-year working hours separately in a sensitivity check. Although they do not report the quantitative results, they state that the results are largely the same (null effects on GPA and detrimental effects on college enrollment) as for school-year employment.

2.6 Conclusion

I examine the causal effects of increased youth labor supply on educational outcomes among high school students. I exploit bunching at zero hours worked per week to estimate, and correct for, the effect of selection. This methodology differs from those used in the literature, namely instrumental variable and individual fixed effect approaches. This novel control function approach exploits a discontinuity in the outcome at the bunching point, which, as treatment is assumed to be continuous, is attributed to selection. Therefore, I estimate the effect of selection by using a ratio of the discontinuity in the outcome and the discontinuity in the confounders at the bunching point. After estimating the effect of selection, I can indirectly identify treatment.

Upon correcting for selection, I find that working during the academic year is harmful for educational outcomes. This is true for both males and females and for almost all educational outcomes. Both females and males see large decreases in their likelihood of college enrollment and increases in their likelihood of dropping out of high school. There are also large decreases in their GPA, both yearly and cumulative, as well as increased probability of course failure. These results, especially for GPA and course failure, are more pronounced for females. Additionally, increased summer labor supply has harmful effects on

[35]There is also a strand of literature that studies the impact of summer employment programs, typically finding beneficial impacts of participating in summer employment on various academic outcomes (Leos-Urbel, 2014; Schwartz et al., 2021; Modestino and Paulsen, 2023). These papers are focused on work programs targeted to urban, low-income youth in major cities (e.g., Boston, New York City). These individuals are most likely to benefit from such programs and these benefits likely do not generalize to a broader population.

educational outcomes, excluding the likelihood of high school dropout. Although the estimated effects are larger during the academic year, it appears that the detrimental impacts of working are not exclusive to the school year. Exploring student engagement as a possible candidate mechanism, I find that increased labor supply results in more missed days of school but does not impact homework or in-class attention. However, increased labor supply does decrease the desire and expectation to attend college, especially for females.

These results add to the literature finding non-positive effects of working during adolescence on educational outcomes as I find that increasing work hours during adolescence has detrimental impacts on educational outcomes for both males and females in the aggregate. Therefore, policies that alter the allowable working hours of adolescents should carefully consider the possible spillovers of youth work intensity onto academic achievement and educational attainment. The results suggest that policy specifically aimed at allowing students to work more intensive hours could be harmful to their educational outcomes.

Appendix A

Chapter 1 Appendix

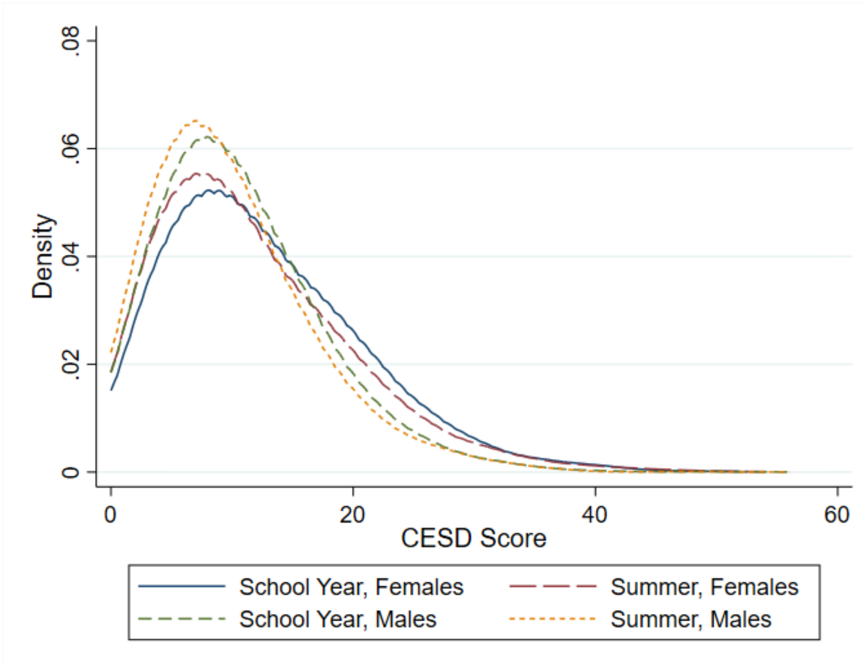
A.1 Additional Tables and Figures

Table A1. CESD Score Questions

You were bothered by things that do not usually bother you.
You did not feel like eating
You felt that you could not shake off the blues.
You felt you were just as good as other people.*
You had trouble keeping your mind on what you were doing.
You felt depressed.
You felt that you were too tired to do things.
You felt hopeful about the future.*
You felt your life had been a failure.
You felt fearful.
You were happy.*
You talked less than usual.
You felt lonely.
People were unfriendly to you
You enjoyed life.*
You felt sad.
You felt that people disliked you.
It was hard to get started doing things.
You felt life was not worth living.

Note: * have been reverse coded. All questions are asked in reference to the last seven days. Questions are answered as follows: 0 for never or rarely, 1 for sometimes, 2 for a lot of the time, and 3 for most of the time or all of the time. The CESD-19 is constructed by summing together the components. Source: Add Health

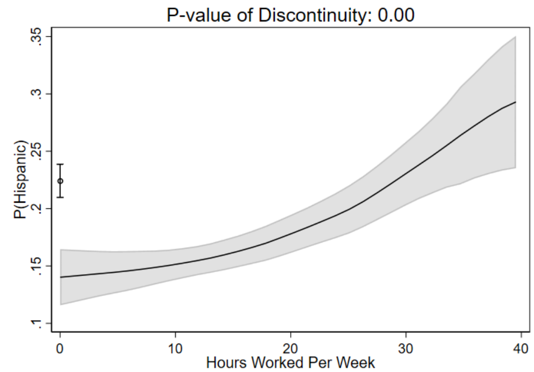
Figure A1. Distribution of CESD-19 score



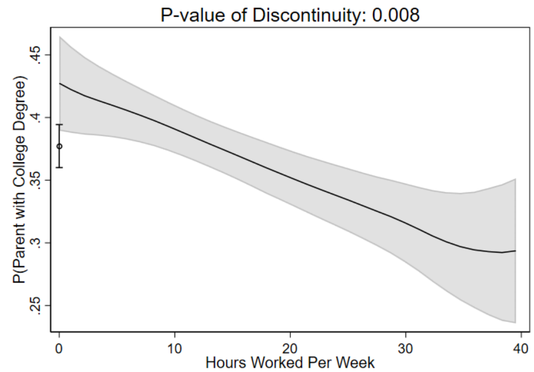
Note: The figure shows the kernel density plots for the CESD-19 score for all four main subsamples.

Figure A2. Evidence of $H^* < 0$ Types (School Year, Male Subsample)

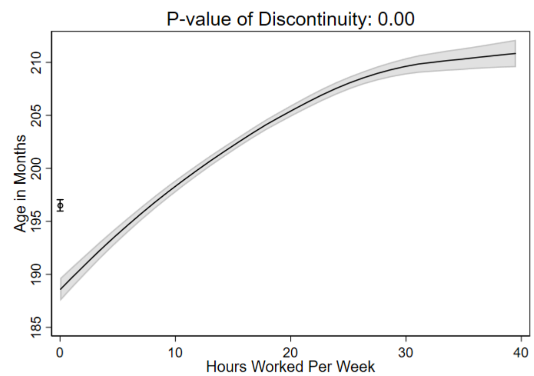
(a) P(Hispanic)



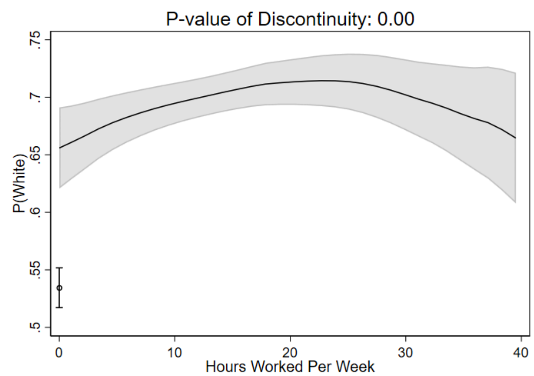
(b) P(College-Educated Parent)



(c) Age (in months)

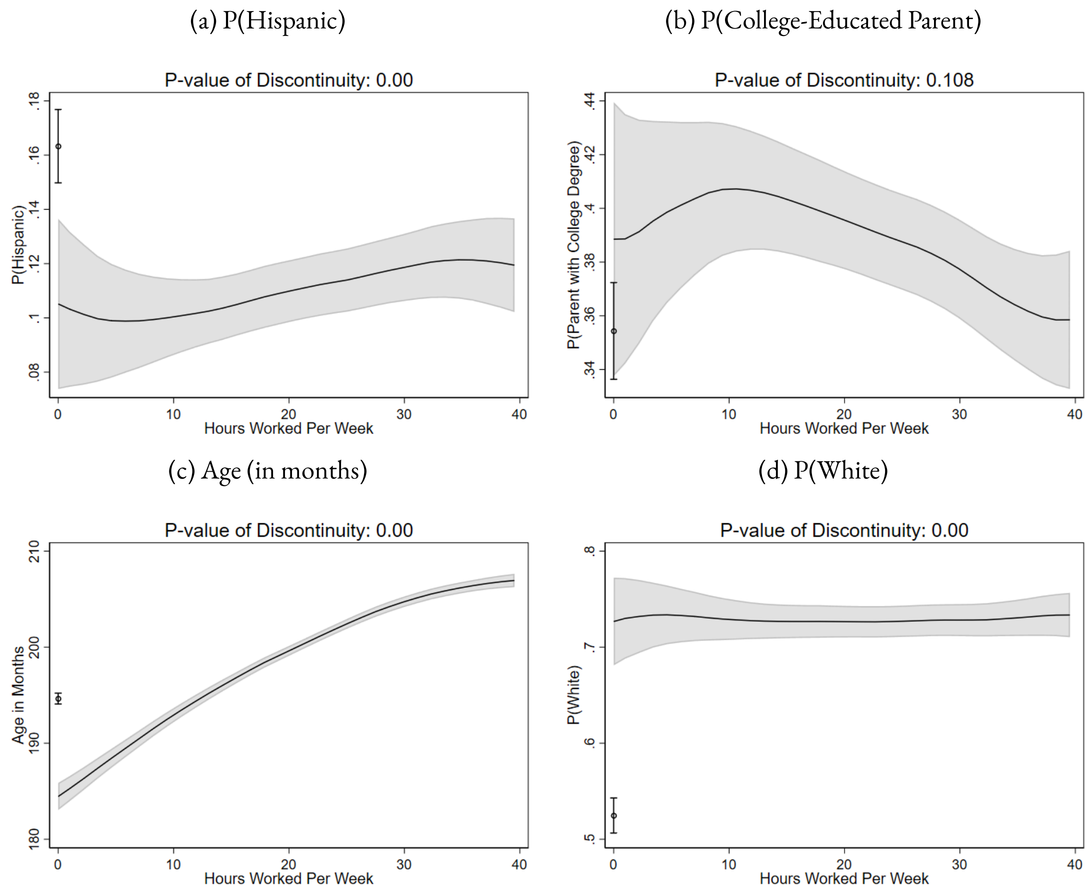


(d) P(White)



Note: Each panel shows the local linear estimate of the expected value of the variable conditional on the hours worked per week as well as the expected value of the variable for those who do not work. 95% confidence intervals shown in gray. The *p*-value is for a test for whether there is a discontinuity at zero. Bandwidth=10. The sample includes males during the school year.

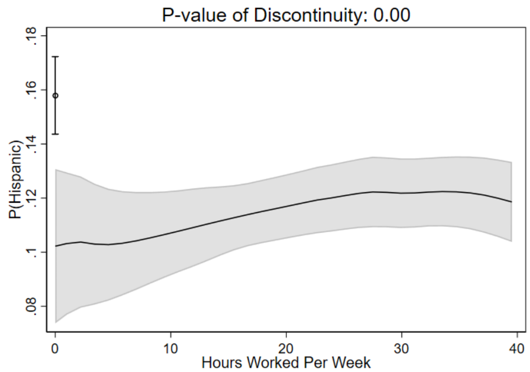
Figure A3. Evidence of $H^* < 0$ Types (Summer, Female Subsample)



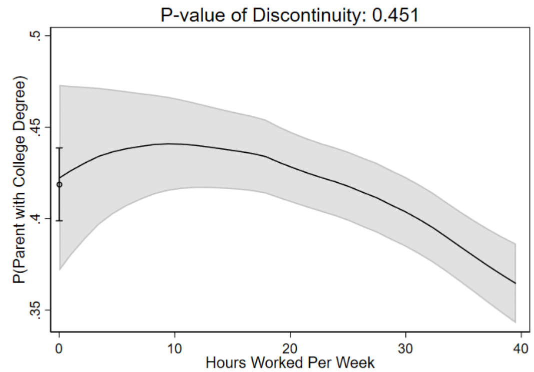
Note: Each panel shows the local linear estimate of the expected value of the variable conditional on the hours worked per week as well as the expected value of the variable for those who do not work. 95% confidence intervals shown in gray. The *p*-value is for a test for whether there is a discontinuity at zero. Bandwidth=10. The sample includes females during the summer.

Figure A4. Evidence of $H^{*} < 0$ Types (Summer, Male Subsample)

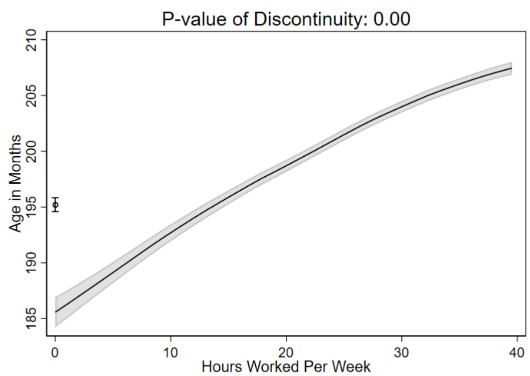
(a) P(Hispanic)



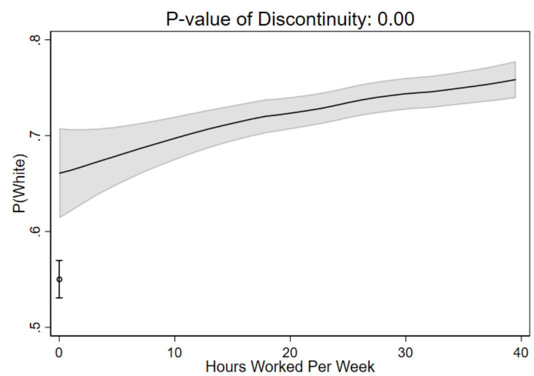
(b) P(College-Educated Parent)



(c) Age (in months)

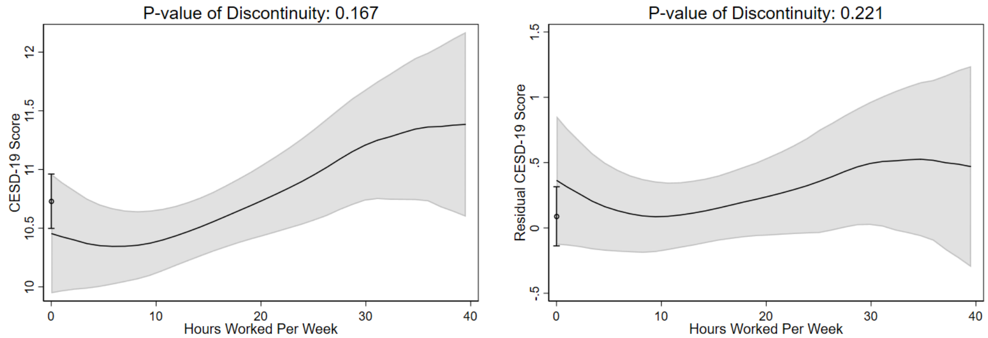


(d) P(White)



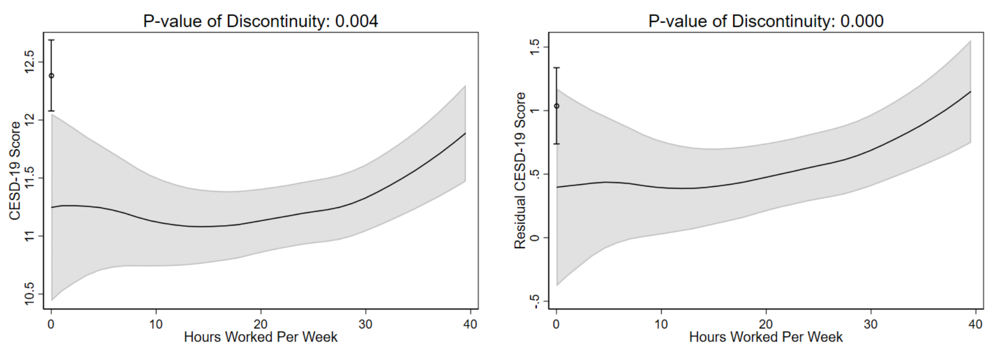
Note: Each panel shows the local linear estimate of the expected value of the variable conditional on the hours worked per week as well as the expected value of the variable for those who do not work. 95% confidence intervals shown in gray. The *p*-value is for a test for whether there is a discontinuity at zero. Bandwidth=10. The sample includes males during the summer.

Figure A5. Evidence of Selection on Unobservables (School Year, Male Subsample)



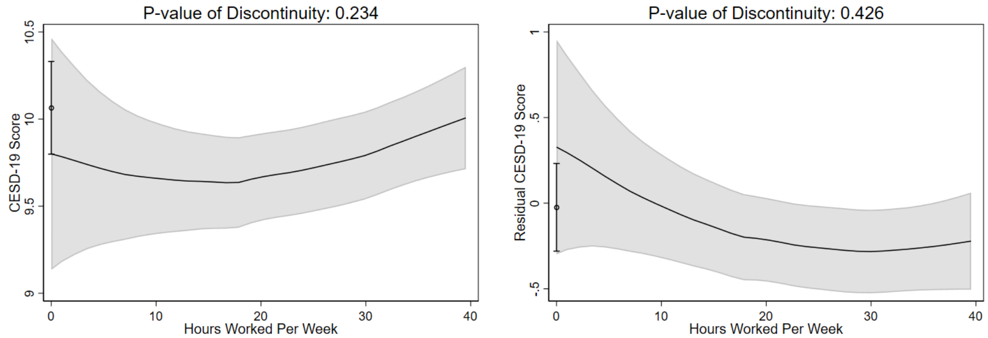
Note: The left panel displays the local linear estimate of the expected value of the CESD-19 score conditional on the hours worked per week, estimated for $H > 0$, and the expected CESD-19 score for those who do not work ($H = 0$). The p-value shown is for a test for whether there is a discontinuity at zero. The right panel shows the local linear estimate of the average CESD-19 score after controlling for observables (race, age, parental education, siblings, Hispanic origin, attending a rural school, and wave). The p-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. Bandwidth=10. The sample includes males during the school year.

Figure A6. Evidence of Selection on Unobservables (Summer, Female Subsample)



Note: The left panel displays the local linear estimate of the expected value of the CESD-19 score conditional on the hours worked per week, estimated for $H > 0$, and the expected CESD-19 score for those who do not work ($H = 0$). The p-value shown is for a test for whether there is a discontinuity at zero. The right panel shows the local linear estimate of the average CESD-19 score after controlling for observables (race, age, parental education, siblings, Hispanic origin, attending a rural school, and wave). The p-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. Bandwidth=10. The sample includes females during the summer.

Figure A7. Evidence of Selection on Unobservables (Summer, Male Subsample)



Note: The left panel displays the local linear estimate of the expected value of the CESD-19 score conditional on the hours worked per week, estimated for $H > 0$, and the expected CESD-19 score for those who do not work ($H = 0$). The *p*-value shown is for a test for whether there is a discontinuity at zero. The right panel shows the local linear estimate of the average CESD-19 score after controlling for observables (race, age, parental education, siblings, Hispanic origin, attending a rural school, and wave). The *p*-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. Bandwidth=10. The sample includes males during the summer.

Table A2. Delinquency Scale Questions

1. In the past 12 months, how often did you paint graffiti or signs on someone else's property or in a public place?
2. In the past 12 months, how often did you deliberately damage property that didn't belong to you?
3. In the past 12 months, how often did you lie to your parents or guardians about where you had been or whom you were with?
4. How often did you take something from a store without paying for it?
5. How often did you run away from home?
6. How often did you drive a car without its owner's permission?
7. In the past 12 months, how often did you steal something worth more than \$50?
8. How often did you go into a house or building to steal something?
9. How often did you use or threaten to use a weapon to get something from someone?
10. How often did you sell marijuana or other drugs?
11. How often did you steal something worth less than \$50?
12. In the past 12 months, how often did you take part in a fight where a group of your friends was against another group?
13. How often were you loud, rowdy, or unruly in a public place?
14. In the past 12 months, how often did you hurt someone badly enough to need bandages or care from a doctor or nurse?
15. In the past 12 months, how often did you get into a serious physical fight?

Note: Each question is answered as follows: 0 for never, 1 for 1 or 2 times, 2 for 3 or 4 times, and 3 for 5 or more times. The delinquency scale is created by summing the total number of all components. Source: Add Health

Table A3. The Effect of Working Hours on Depressive Symptoms (CESD-19)

	Uncorrected no Controls (1)	Uncorrected w/ Controls (2)	Semiparametric Uniform (3)	Semiparametric Tobit (4)	Nonparametric Symmetry (5)
Panel A: Female, School Months (Mean=12.66, N=6296)					
β	0.0262**	0.0243**	0.0871***	0.0908***	0.1532***
	(0.0106)	(0.0115)	(0.0301)	(0.0317)	(0.0581)
δ			-0.0392**	-0.0429**	-0.1053**
			(0.0173)	(0.0189)	(0.0464)
Panel B: Female, Summer Months (Mean=11.82, N=6596)					
β	-0.0124**	-0.0036	0.0721***	0.0737***	0.0695***
	(0.0060)	(0.0067)	(0.0239)	(0.0243)	(0.0233)
δ			-0.0523***	-0.0538***	-0.0497***
			(0.0160)	(0.0164)	(0.0154)
Panel C: Male, School Months (Mean=10.65, N=6201)					
β	0.0124	0.0037	0.0097	0.0097	0.0145
	(0.0084)	(0.0089)	(0.0256)	(0.0259)	(0.0445)
δ			-0.0040	-0.0040	-0.0088
			(0.0161)	(0.0163)	(0.0357)
Panel D: Male, Summer Months (Mean=9.93, N=6259)					
β	-0.0001	-0.0078	-0.0140	-0.0138	-0.0144
	(0.0047)	(0.0051)	(0.0201)	(0.0195)	(0.0214)
δ			0.0045	0.0043	0.0049
			(0.0142)	(0.0136)	(0.0156)

Note: The table shows the effect of one additional hour of work per week on the CESD-19 score. Bootstrapped standard errors in parentheses are based on 500 iterations. Columns (1) and (2) do not include any correction. Column (3) assumes a semiparametric uniform distribution, column (4) assumes a semiparametric tobit distribution, and column (5) assumes a nonparametric tail symmetric distribution for the distribution of H^*. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A4. Heterogeneous Effects of Working Hours on Depressive Symptoms by Parental Education

	Females in School (1)	Females in Summer (2)	Males in School (3)	Males in Summer (4)
Panel A: Parent w/ Higher Education				
β	0.2330***	0.1299***	0.0723*	-0.0035
	(0.0618)	(0.0417)	(0.0437)	(0.0331)
N	2027	2348	2243	2446
Mean	11.726	10.254	10.068	8.936
Panel B: Parent w/o Higher Education				
β	0.0345	0.0362	-0.0226	-0.0286
	(0.0354)	(0.0342)	(0.0308)	(0.0271)
N	4034	3949	3717	3534
Mean	12.977	12.544	10.846	10.408

Note: The table shows the effects of one additional hour of work per week on the CESD score. Panel A includes those who have at least one parent with a college degree. Panel B includes those who report neither parent having a college degree. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semi-parametric tobit assumption. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A5. Heterogeneous Effects of Working Hours on Depressive Symptoms by School Location

	Females in School (1)	Females in Summer (2)	Males in School (3)	Males in Summer (4)
Panel A: Rural School				
β	0.0975	0.1306*	-0.0462	0.0563
	(0.0799)	(0.0683)	(0.0717)	(0.0485)
N	1016	1151	1057	1098
Mean	12.303	12.165	10.129	9.920
Panel B: Urban School				
β	0.0922***	0.0588**	0.0213	-0.0292
	(0.0348)	(0.0276)	(0.0270)	(0.0218)
N	5280	5445	5144	5161
Mean	12.731	11.749	10.757	9.927

Note: The table shows the effects of one additional hour of work per week on the CESD score. Panel A includes those who attend a school in a rural environment. Panel B includes those who attend a school in an urban or suburban environment. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semi-parametric tobit assumption. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A6. Heterogeneous Effects of Working Hours on Depressive Symptoms by Age Group

	Females in School (1)	Females in Summer (2)	Males in School (3)	Males in Summer (4)
Panel A: Age 14-15				
β	0.1397**	0.0663*	-0.0230	-0.0446
	(0.0680)	(0.0367)	(0.0551)	(0.0282)
N	2417	2469	2186	2222
Mean	12.138	11.469	9.810	9.015
Panel B: Age 16-18				
β	0.0940**	0.1055***	0.0425	0.0153
	(0.0398)	(0.0379)	(0.0324)	(0.0320)
N	3879	4127	4015	4037
Mean	12.988	12.033	11.107	10.422

Note: The table shows the effects of one additional hour of work per week on the CESD score. Panel A includes those who are aged 14-15. Panel B includes those who are aged 16-18. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semiparametric tobit assumption. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A7. Heterogeneous Effects of Working Hours on Depressive Symptoms by Race

	Females in School (1)	Females in Summer (2)	Males in School (3)	Males in Summer (4)
Panel A: White				
β	0.1612***	0.0981***	0.0513	0.0231
	(0.0422)	(0.0327)	(0.0323)	(0.0250)
N	3804	4217	3798	4138
Mean	12.018	11.337	9.946	9.323
Panel B: Black				
β	-0.0658	-0.0086	-0.0975*	-0.1489***
	(0.0624)	(0.0474)	(0.0566)	(0.0417)
N	1438	1500	1300	1266
Mean	12.986	12.272	11.140	10.544

Note: The table shows the effects of one additional hour of work per week on the CESD score. Panel A includes those who are White and Panel B includes those who are Black. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semiparametric tobit assumption. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A8. Heterogeneous Effects of Working Hours on Depressive Symptoms by Hispanic Origin

	Females in School (1)	Females in Summer (2)	Males in School (3)	Males in Summer (4)
Panel A: Hispanic				
β	0.0600	-0.0438	0.0540	-0.0220
	(0.0665)	(0.0748)	(0.0470)	(0.0575)
N	1215	885	1219	830
Mean	14.044	13.460	11.811	10.988
Panel B: Non-Hispanic				
β	0.0897***	0.0862***	-0.0089	-0.0160
	(0.0347)	(0.0264)	(0.0301)	(0.0226)
N	5081	5711	4982	5429
Mean	12.331	11.569	10.366	9.763

Note: The table shows the effects of one additional hour of work per week on the CESD score. Panel A includes those who are Hispanic and Panel B includes those who are non-Hispanic. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semiparametric tobit assumption. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A9. The Effect of Working Hours on Various Mental Health Outcomes

	CESD-5	CESD-19 $\geq$ 16	CESD-19 $\geq$ 24	Contemplate Suicide	Attempt Suicide
	(1)	(2)	(3)	(4)	(5)
Panel A: School Months, Females					
β	0.0374***	0.0051***	0.0030**	0.0046***	0.0024***
	(0.0103)	(0.0018)	(0.0012)	(0.0014)	(0.0008)
N	6296	6296	6296	6279	6279
Mean	3.032	0.321	0.101	0.155	0.054
Panel B: Summer Months, Females					
β	0.0221**	0.0028*	0.0012	0.0004	0.0005
	(0.0090)	(0.0015)	(0.001)	(0.0011)	(0.0007)
N	6636	6636	6636	6614	6614
Mean	2.819	0.279	0.089	0.152	0.050
Panel C: School Months, Males					
β	0.0031	0.0012	0.0000	-0.0016	0.0001
	(0.0083)	(0.0015)	(0.0009)	(0.0011)	(0.0006)
N	6208	6208	6208	6177	6177
Mean	2.298	0.207	0.048	0.093	0.022
Panel D: Summer Months, Males					
β	-0.0043	-0.0003	-0.0001	0.0001	0.0002
	(0.0065)	(0.0012)	(0.0007)	(0.0009)	(0.0004)
N	6343	6343	6343	6292	6292
Mean	2.162	0.176	0.044	0.095	0.020

Note: The table shows the effects of one additional hour of work per week on five mental health measures: CESD-19≥16, CESD-19≥24, probability of contemplating suicide, probability of attempting suicide, and the CESD-5. Bootstrapped standard errors in parentheses based on 500 iterations. Point estimates are calculated using the semi-parametric tobit assumption. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Column (1) is estimated using OLS and columns (1)-(4) are estimated using probit. Bootstrapped standard errors in parentheses based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A10. The Effect of Working Hours on Time Use

	P(Does Chores)	P(Engages in Hobbies)	P(Exercises)	P(Socializes)	P(Plays Sports)	Hours of sleep per week	Hours of television per week
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School Months, Females							
β	-0.0015**	-0.0077***	-0.0045***	-0.0049***	-0.0127***	-0.1604***	0.1386***
	(0.0007)	(0.0015)	(0.0013)	(0.0011)	(0.0019)	(0.0363)	(0.0527)
N	6296	6296	6296	6296	6296	6293	6281
Mean	0.972	0.820	0.879	0.926	0.599	51.408	12.877
Panel B: Summer Months, Females							
β	-0.0002	-0.0039***	-0.0023**	-0.0019**	-0.0062***	-0.1157***	0.1008**
	(0.0005)	(0.0013)	(0.0011)	(0.0009)	(0.0015)	(0.0309)	(0.0414)
N	6596	6596	6596	6596	6596	6593	6569
Mean	0.98	0.773	0.866	0.898	0.619	54.412	15.337
Panel C: School Months, Males							
β	-0.0027***	-0.0031**	-0.0030**	-0.0035***	-0.0035***	-0.1454***	0.0338
	(0.0008)	(0.0013)	(0.0015)	(0.0009)	(0.0014)	(0.0335)	(0.0456)
N	6201	6201	6201	6201	6201	6199	6174
Mean	0.957	0.831	0.824	0.947	0.838	52.677	14.948
Panel D: Summer Months, Males							
β	0.0000	-0.0063***	-0.0020	-0.0025***	-0.005***	-0.1257***	-0.0169
	(0.0006)	(0.0013)	(0.0013)	(0.0009)	(0.0013)	(0.0324)	(0.0471)
N	6257	6258	6257	6257	6258	6254	6229
Mean	0.957	0.791	0.797	0.922	0.798	55.097	17.526

Note: The table shows the effects of one additional hour of work per week on various time use measures. Binary outcomes are measures of whether the individual engaged in the activity in the last week. Point estimates are calculated using the semiparametric tobit assumption. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Columns (1)-(5) are estimated using probit and columns (6) and (7) are estimated using OLS. Bootstrapped standard errors in parentheses based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A11. The Effect of Working Hours on Time Use

	P(Does Chores)	P(Engages in Hobbies)	P(Exercises)	P(Socializes)	P(Plays Sports)
	(1)	(2)	(3)	(4)	(5)
Panel A: School Months, Females					
β	-0.0079***	-0.0087***	-0.0037*	-0.0033*	-0.0087***
	(0.0018)	(0.0021)	(0.0020)	(0.0019)	(0.0018)
N	6296	6296	6296	6296	6296
Mean	0.726	0.451	0.542	0.689	0.324
Panel B: Summer Months, Females					
β	-0.0010	-0.0052***	-0.0040**	-0.0005	-0.0042***
	(0.0014)	(0.0015)	(0.0016)	(0.0015)	(0.0013)
N	6596	6596	6596	6596	6596
Mean	0.776	0.394	0.501	0.648	0.278
Panel C: School Months, Males					
β	-0.0084***	-0.0010	-0.0029	-0.0049***	-0.0048***
	(0.0018)	(0.0019)	(0.0019)	(0.0017)	(0.0018)
N	6201	6201	6201	6201	6201
Mean	0.633	0.511	0.548	0.729	0.602
Panel D: Summer Months, Males					
β	-0.0042***	-0.0051***	-0.0030*	-0.0057***	-0.0046***
	(0.0014)	(0.0015)	(0.0016)	(0.0015)	(0.0015)
N	6257	6258	6257	6257	6258
Mean	0.677	0.467	0.489	0.697	0.531

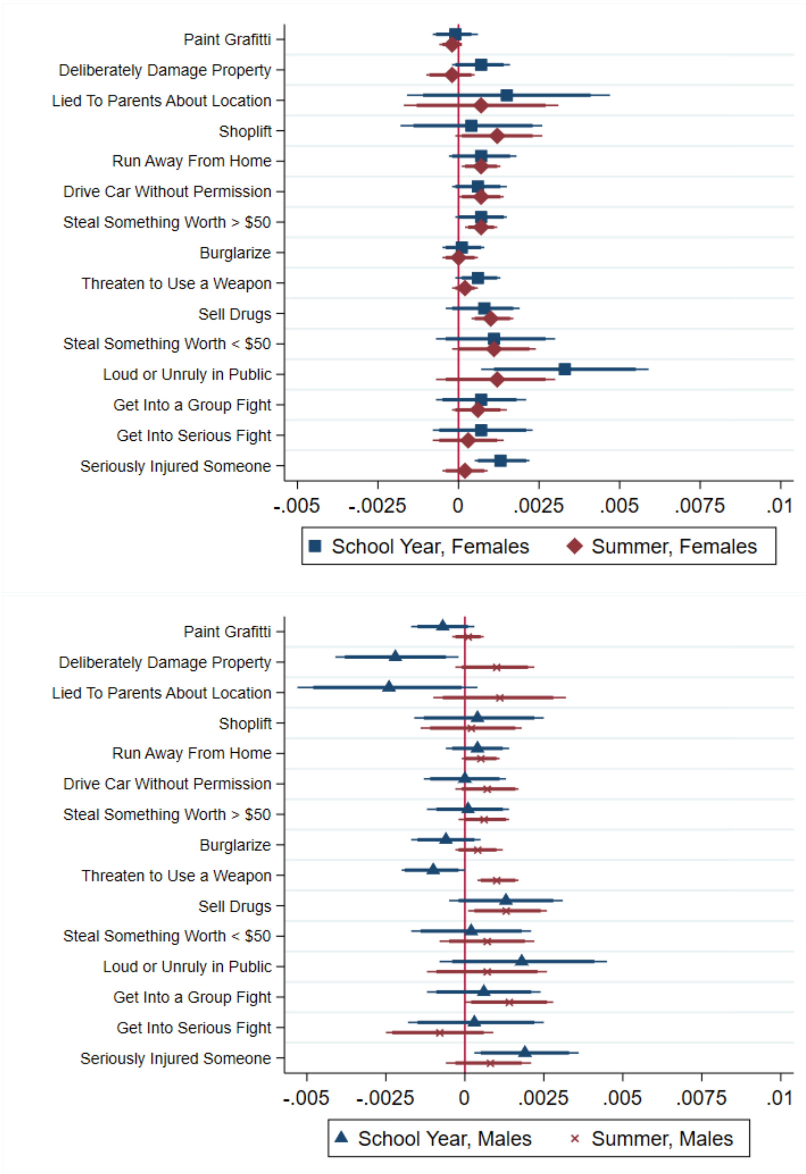
Note: The table shows the effects of one additional hour of work per week on various time use measures. Binary outcomes are measures of whether the individual engaged in the activity more than twice in the last week. Point estimates are calculated using the semiparametric tobit assumption and a probit model. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Bootstrapped standard errors in parentheses based on 500 iterations. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

Table A12. The Effect of Working Hours on Substance Use and Delinquency

	P(Drank in Last 12 Months)	P(Binge Drank in Last 12 Months)	P(Smoked in Past 30 Days)	P(Friends Drink Alcohol)	P(Friends Smoke)	Delinquency Scale
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: School Months, Females						
β	0.0011	0.0046***	0.0062***	0.0052***	0.0093***	0.0557***
	(0.0020)	(0.0017)	(0.0018)	(0.0020)	(0.0019)	(0.0164)
N	6273	5911	5846	6231	6240	6265
Mean	0.466	0.265	0.298	0.582	0.443	3.307
Panel B: Summer Months, Females						
β	0.0008	0.0013	0.0047***	0.0016	0.0053***	0.0157
	(0.0016)	(0.0015)	(0.0016)	(0.0015)	(0.0016)	(0.0130)
N	6568	6111	5950	6522	6538	6558
Mean	0.496	0.277	0.318	0.596	0.466	3.228
Panel C: School Months, Males						
β	0.0003	0.0024	0.0045***	-0.0004	0.0042**	-0.0190
	(0.0019)	(0.0017)	(0.0017)	(0.0017)	(0.0018)	(0.0216)
N	6149	5736	5755	6085	6088	6144
Mean	0.452	0.311	0.306	0.569	0.467	4.418
Panel D: Summer Months, Males						
β	0.0051***	0.0037**	0.0037**	0.0043***	0.0037**	0.0418**
	(0.0015)	(0.0015)	(0.0016)	(0.0015)	(0.0015)	(0.0176)
N	6189	5737	5621	6149	6149	6179
Mean	0.497	0.349	0.328	0.611	0.491	4.535

Note: The table shows the effects of one additional hour of work per week on various risky behavior and delinquency outcomes. Point estimates are calculated using the semiparametric tobit assumption. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Columns (1)-(5) are estimated using probit and columns (6) is estimated using OLS. Bootstrapped standard errors in parentheses based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

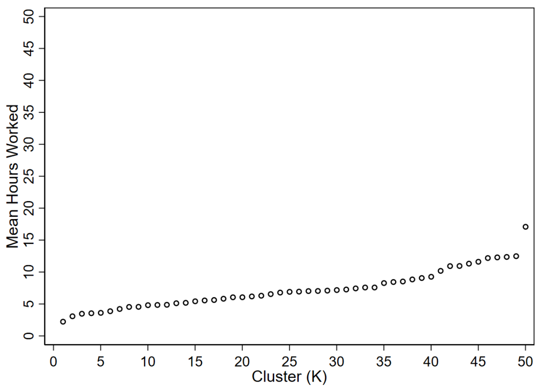
Figure A8. The Effects of Working Hours on Components of the Delinquency Scale



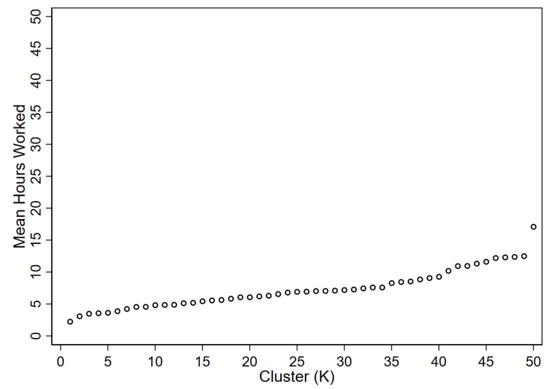
Note: The figure shows the effects of one additional hour of work on binary measures of each component of the delinquency scale. Controls include dummies for age-in-quarters, school grade, race, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. Results are estimated using the semiparametric tobit distributional assumption and probit estimation. 90% confidence intervals, based on bootstrapped standard errors from 100 iterations, are shown.

Figure A9. Average Hours Worked Per Week for Each Cluster

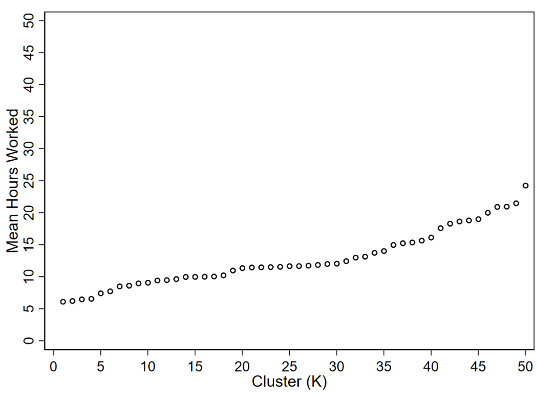
(a) Females, School Year



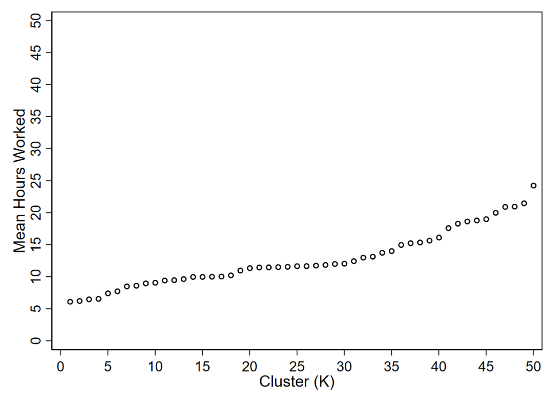
(b) Males, School Year



(c) Females, Summer



(d) Males, Summer



Note: The figure plots the average number of hours worked across $K = 50$ clusters.

A.2 Adolescent Occupation Composition from CPS

The Add Health data does not contain information regarding the characteristics of adolescents' occupations. However, differences in occupational composition likely contribute to the differences in the effects of labor supply on mental health across groups. In this section, I use data from the basic monthly Current Population Survey (CPS) from 1994-96 to describe occupations for this age group. While I restrict to the same years as my Add Health sample, the samples in the CPS and Add Health are not necessarily equivalent. For example, the occupation information in CPS only includes those who are older than 14 and information on whether an individual is in secondary school is only available for those older than 15, while my Add Health sample includes individuals who are 14 and 15 years old. However, this section provides insight on the broad compositional differences in occupation between different groups. Additional breakdowns of occupations by family income, race, and metropolitan status are available upon request.

Table A13. Major Occupation Categories: Males and Females Aged 16-18

	Males	Females
Executive, administrative, & managerial	0.01	0.01
Professional specialty	0.02	0.02
Technicians & related support	0.00	0.00
Sales	0.16	0.37
Administrative support, including clerical	0.04	0.11
Private household	0.00	0.04
Protective service	0.01	0.01
Service	0.37	0.37
Precision production, craft, & repair	0.03	0.00
Machine operators, assemblers, & inspectors	0.02	0.01
Transportation & material moving	0.02	0.00
Handlers, equipment cleaners, helpers, laborers	0.23	0.04
Farming, forestry, & fishing	0.09	0.02
N	22628	22282

Note: The table shows the composition of occupations held by male and female students aged 16 to 18 in the 1994-1996 basic monthly CPS. Occupations are based upon the reported major occupation codes.

Table A14. Ten Most Common Occupation by Gender

Males	Females
Food service (30%)	Sales, retail, & personal services (36%)
Freight, stock, & materials handlers (15%)	Food service (28%)
Sales, retail, & personal services (15%)	Other admin. support, including clerical* (8%)
Farm workers (7%)	Personal service (5%)
Other handlers & laborers† (6%)	Private household service (4%)
Cleaning and building services (5%)	Freight, stock, & materials handlers (3%)
Other admin. support, including clerical* (4%)	Cleaning and building services (2%)
Personal service (3%)	Secretaries, stenographers, & typists (2%)
Motor vehicle operators (2%)	Health service (2%)
Farm operators and managers (1%)	Teachers, except college & university (2%)
$N = 22,625$	$N = 22,271$

Note: The table shows the top ten reported occupation for each group in order from most to least common for 16- to 18-year-olds in the 1994-1996 basic monthly CPS. Occupations are based upon the reported detailed occupation codes.
* That is, not in one of the following: supervisors, administrative support; computer equipment operators; secretaries, stenographers, and typists; financial records processing; or mail and message distribution.
† That is, not in one of the following: Construction laborers; freight, stock and materials handlers

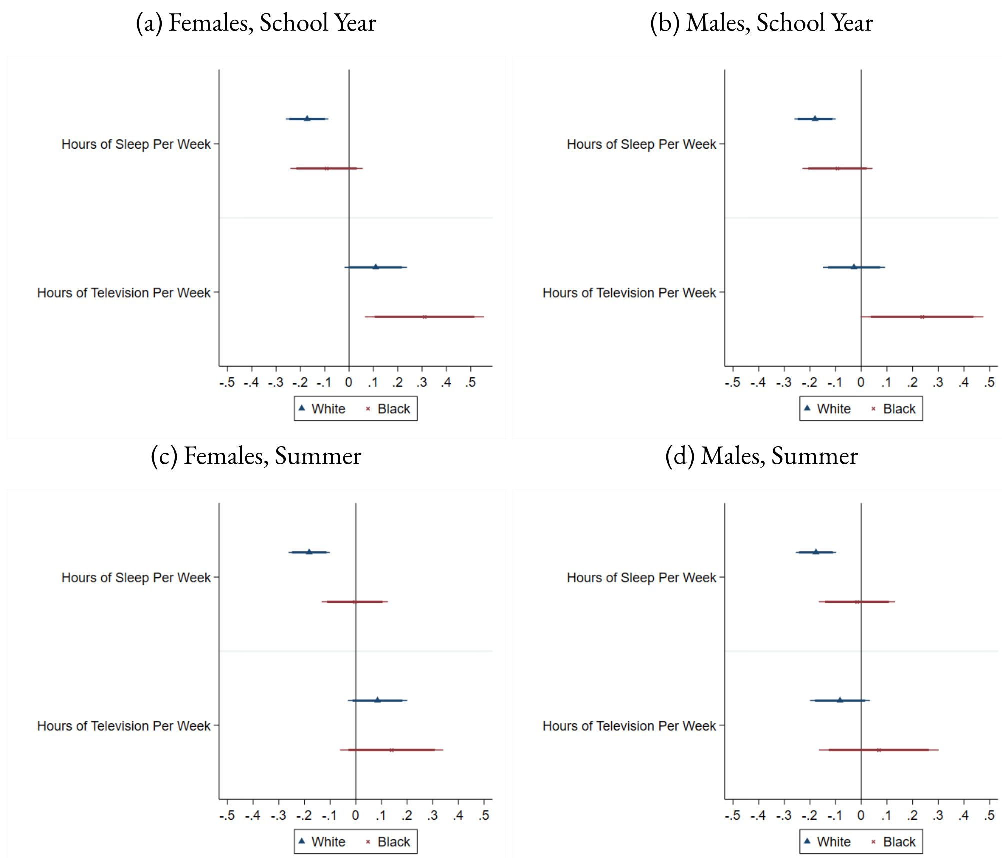
A.3 Racial Differences in the Effects of Hours Worked

In this section, I examine the racial heterogeneity present in Figure 1.7, which shows notable differences in how increased labor supply affects the mental health of White and Black adolescents. White females face harmful effects from working, while the estimated effects, though noisy, for Black females suggest possible benefits. Black males, especially during the summer, see clear benefits from increased working hours. Although the estimates are imprecise, White males appear to face harmful impacts from increased labor supply. The estimated effects are consistently statistically different between racial groups.

To better understand what drives these differences, I examine possible heterogeneity in the candidate mechanisms discussed in Section 1.4.5. Figures A10 to A13 show the effects of an additional hour worked per week on various candidate mechanisms, segmented by race. Overall, it does not appear that differences in time use substitution drive the effect heterogeneity. The most notable racial differences in time use substitution are during the school year, such that White males have larger decreases in the likelihood of socializing and exercising and White females have larger decreases in the likelihood of playing sports. However, there are stark racial differences in the effects on risky behaviors and delinquency. Increased hours worked per week result in increased delinquency for White females during the school year, as well as increases in the likelihood of drinking and binge drinking alcohol, smoking, and having friends who drink alcohol. Similarly, White males during the school year increase their binge drinking, smoking, and friendships with peers who drink alcohol. White males during the summer also increase their drinking behavior and have more friends who smoke, while White females increase their smoking and have more friends who drink alcohol. In contrast, Black adolescents during both the school year and the summer see no increases in their risky behaviors. Working introduces harmful behaviors and peer influences largely among White adolescents, while not detrimentally impacting the behavior and peer associations of Black adolescents. In fact, some estimates suggest that working may improve the risky behavior, peer group, and delinquency outcomes for Black adolescents.

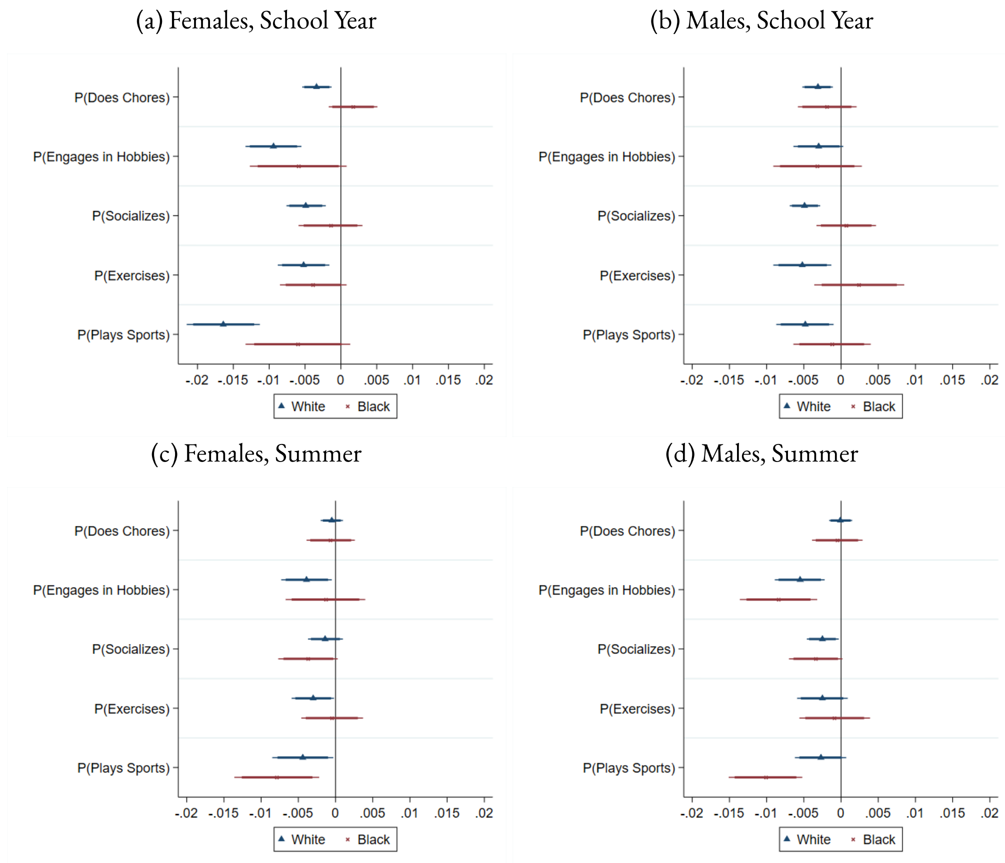
These findings suggest that working introduces White adolescents to risky behaviors and peer groups that are especially harmful. Although there are not large differences in changes to time use, those that do exist suggest possible changes in peer groups as White students have larger changes in how much they socialize, exercise, or engage in active sports, especially during the school year. These new behaviors and influences may outweigh the possible mental health benefits from employment. Meanwhile, Black adolescents may be better able to gain the mental health benefits from working, as they do not increase their engagement in risky behaviors, delinquency, or harmful peer groups.

Figure A10. The Effects of Working Hours on Time Use: Heterogeneity by Race



Note: The figure shows the effects of one additional hour of work per week on various measures of time use. Controls include dummies for age-in-quarters, school grade, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

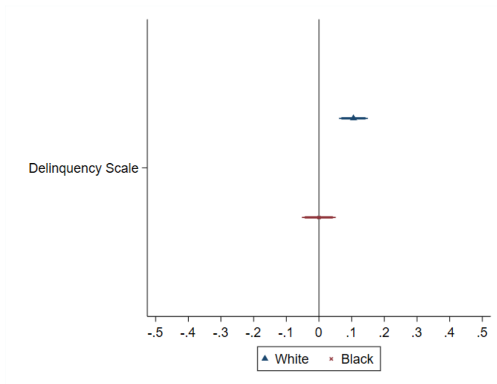
Figure A11. The Effects of Working Hours on Time Use: Heterogeneity by Race



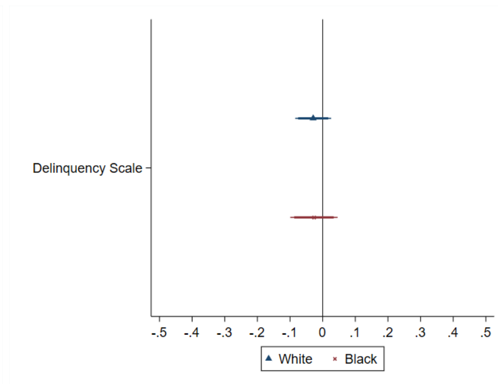
Note: The figure shows the effects of one additional hour of work per week on various measures of time use. Controls include dummies for age-in-quarters, school grade, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

Figure A12. The Effects of Working Hours on Delinquency: Heterogeneity by Race

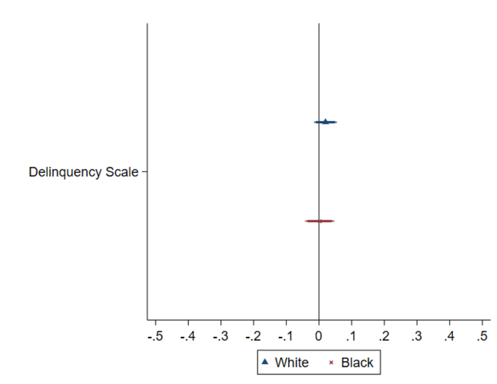
(a) Females, School Year



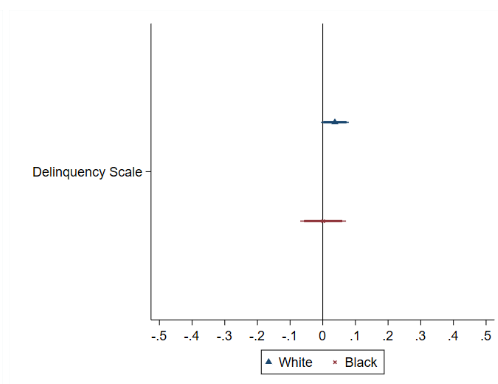
(b) Males, School Year



(c) Females, Summer

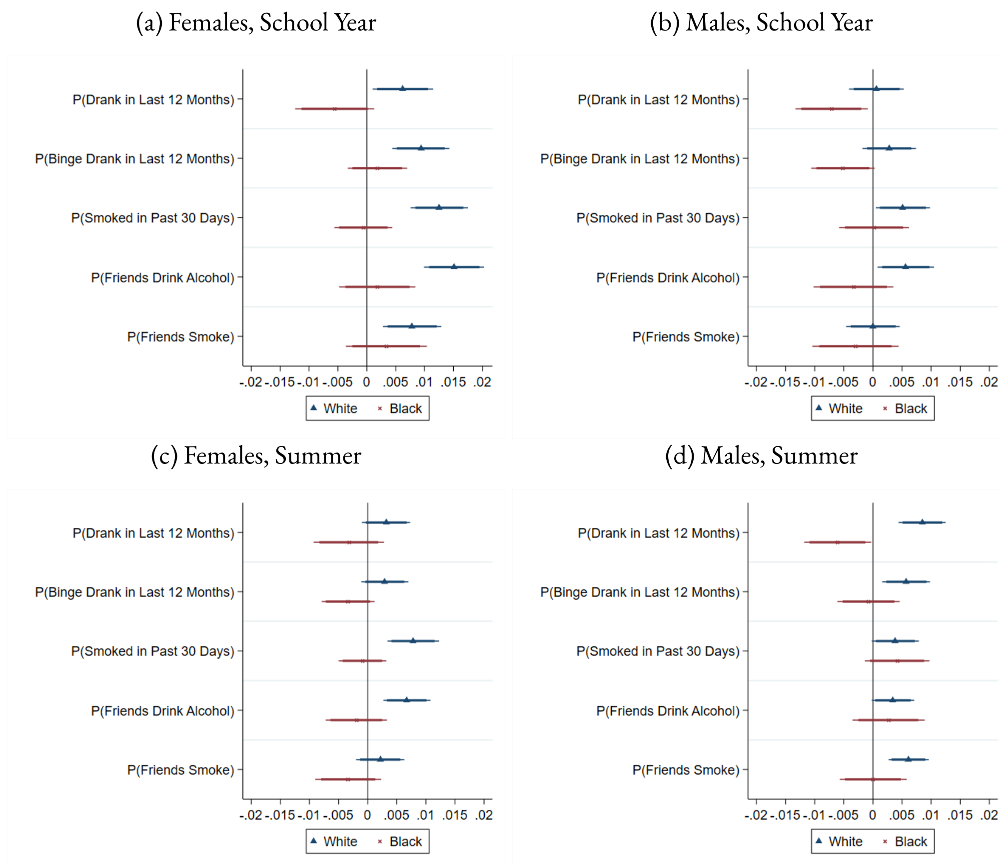


(d) Males, Summer



Note: The figure shows the effects of one additional hour of work per week on delinquency. Controls include dummies for age-in-quarters, school grade, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

Figure A13. The Effects of Working Hours on Risky Behaviors: Heterogeneity by Race



Note: The figure shows the effects of one additional hour of work per week on various measures of risky behaviors. Controls include dummies for age-in-quarters, school grade, maternal and paternal education, whether the individual has siblings, Hispanic origin, whether they attend a rural school, and wave. 90% and 95% confidence intervals, based on bootstrapped standard errors from 500 iterations, are shown.

Appendix B

Chapter 2 Appendix

B.1 Additional Tables and Figures

Table B1. Descriptive Statistics (Academic-Year Sample)

	Females		Males	
	Mean	SD	Mean	SD
Educational Outcomes				
Yearly GPA	2.73	0.87	2.43	0.91
Yearly P(Ever Fail Course)	0.26	0.44	0.35	0.48
Hours Worked & Bunching				
Works zero hours ($H = 0$)	0.45	0.5	0.41	0.49
Academic year hours worked ($H \geq 0$)	9.09	11.77	10.59	13.01
Academic year hours worked ($H > 0$)	16.59	11.33	17.9	12.45
Control Variables				
Age (years)	16.51	1.02	16.6	1.01
Hispanic	0.18	0.38	0.18	0.39
Race				
White	0.61	0.49	0.63	0.48
Black	0.22	0.41	0.19	0.39
Other	0.17	0.38	0.18	0.39
Maternal Education				
No degree	0.16	0.37	0.13	0.34
High school	0.31	0.46	0.31	0.46
Some college	0.20	0.40	0.18	0.38
College	0.26	0.44	0.28	0.45
Missing	0.08	0.27	0.19	0.30
Paternal Education				
No degree	0.11	0.31	0.11	0.31
High school	0.22	0.41	0.23	0.42
Some college	0.12	0.33	0.14	0.35
College	0.22	0.41	0.24	0.43
Missing	0.33	0.47	0.28	0.45
Grade				
9^{th} grade	0.13	0.34	0.15	0.35
10^{th} grade	0.30	0.46	0.30	0.46
11^{th} grade	0.31	0.46	0.31	0.46
12^{th} grade	0.26	0.44	0.24	0.43
Has siblings	0.78	0.42	0.78	0.41
Attends rural school	0.16	0.37	0.17	0.37
Observations	10641		10378	

Note: The table shows descriptive statistics for the sample of person-wave observations, based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample is used to study outcomes that are contemporaneous to a given school year, therefore includes all observations of an individual in which I observe them in high school.

Table B2. Descriptive Statistics (Cumulative Outcomes Sample)

	Females		Males	
	Mean	SD	Mean	SD
Educational Outcomes				
Cumulative GPA	2.71	0.78	2.43	0.81
P(Ever Fail Course)	0.45	0.5	0.57	0.49
P(HS Dropout)	0.05	0.21	0.07	0.26
P(Attend College)	0.73	0.44	0.64	0.48
Hours Worked & Bunching				
Works zero hours ($H = 0$)	0.42	0.49	0.38	0.48
Academic year hours worked ($H \geq 0$)	10.41	12.3	11.99	13.62
Academic year hours worked ($H > 0$)	17.88	11.24	19.26	12.56
Control Variables				
Age (years)	16.82	0.98	16.9	0.97
Hispanic	0.18	0.38	0.19	0.39
Race				
White	0.61	0.49	0.62	0.48
Black	0.22	0.41	0.19	0.40
Other	0.17	0.38	0.18	0.39
Maternal Education				
No degree	0.16	0.37	0.14	0.35
High school	0.31	0.46	0.31	0.46
Some college	0.19	0.4	0.18	0.38
College	0.26	0.44	0.28	0.45
Missing	0.09	0.28	0.10	0.30
Paternal Education				
No degree	0.11	0.32	0.11	0.31
High school	0.22	0.41	0.23	0.42
Some college	0.12	0.33	0.14	0.35
College	0.21	0.41	0.23	0.42
Missing	0.33	0.47	0.28	0.45
Grade				
9^{th} grade	0.06	0.24	0.08	0.27
10^{th} grade	0.25	0.43	0.25	0.43
11^{th} grade	0.29	0.46	0.31	0.46
12^{th} grade	0.40	0.49	0.36	0.48
Has siblings	0.76	0.43	0.77	0.42
Attends rural school	0.16	0.37	0.16	0.37
Observations	7052		6900	

Note: The table shows descriptive statistics based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample is used to study cumulative and long-term outcomes. I include individuals only once, when I last observe them in high school.

Table B3. Descriptive Statistics across Cumulative Samples (Females)

	Full Analytical Sample			Conditional on Being in Wave 4			Conditional on Transcript Data		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Educational Outcomes									
Cumulative GPA	2.71	0.78	4336	2.72	0.77	3866	2.71	0.78	4336
P(Ever Fail Course)	0.45	0.50	4336	0.45	0.50	3866	0.45	0.50	4336
P(HS Dropout)	0.05	0.21	5605	0.05	0.21	5605	0.03	0.17	3866
P(Attend College)	0.73	0.44	5605	0.73	0.44	5605	0.76	0.43	3866
Hours Worked & Bunching									
Works zero hours ($H = 0$)	0.42	0.49	7052	0.41	0.49	5605	0.4	0.49	4336
Academic year hours worked ($H \geq 0$)	10.41	12.3	7052	10.51	12.19	5605	10.44	12.02	4336
Academic year hours worked ($H > 0$)	17.88	11.24	4105	17.72	11.07	3325	17.28	10.99	2619
Control Variables									
Age (years)	16.82	0.98	7052	16.82	0.98	5605	16.81	0.98	4336
Hispanic	0.18	0.38	7052	0.17	0.38	5605	0.16	0.37	4336
Race									
White	0.61	0.49	7052	0.62	0.49	5605	0.63	0.48	4336
Black	0.22	0.41	7052	0.22	0.42	5605	0.21	0.41	4336
Other	0.17	0.38	7052	0.16	0.37	5605	0.16	0.37	4336
Maternal Education									
No degree	0.16	0.37	7052	0.16	0.36	5605	0.15	0.36	4336
High school	0.31	0.46	7052	0.31	0.46	5605	0.31	0.46	4336
Some college	0.19	0.4	7052	0.20	0.40	5605	0.20	0.40	4336
College	0.25	0.43	7052	0.25	0.43	5605	0.26	0.44	4336
Missing	0.09	0.28	7052	0.08	0.27	5605	0.08	0.27	4336
Paternal Education									
No degree	0.11	0.32	7052	0.11	0.32	5605	0.11	0.32	4336
High school	0.22	0.41	7052	0.22	0.42	5605	0.23	0.42	4336
Some college	0.12	0.33	7052	0.13	0.34	5605	0.14	0.34	4336
College	0.21	0.41	7052	0.21	0.41	5605	0.24	0.42	4336
Missing	0.33	0.47	7052	0.32	0.47	5605	0.29	0.45	4336
Grade									
9^{th} grade	0.06	0.24	7052	0.05	0.22	5605	0.04	0.2	4336
10^{th} grade	0.25	0.43	7052	0.25	0.44	5605	0.26	0.44	4336
11^{th} grade	0.29	0.46	7052	0.30	0.46	5605	0.30	0.46	4336
12^{th} grade	0.4	0.49	7052	0.40	0.49	5605	0.40	0.49	4336
Has siblings	0.76	0.43	7052	0.76	0.43	5605	0.77	0.42	4336
Attends rural school	0.16	0.37	7052	0.17	0.37	5605	0.18	0.38	4336

Note: The table shows descriptive statistics based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample is used to study cumulative and long-term outcomes. I include individuals only once, when I last observe them in high school. This sample includes only females.

Table B4. Descriptive Statistics across Cumulative Samples (Males)

	Full Analytical Sample			Conditional on Being in Wave 4			Conditional on Transcript Data		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Educational Outcomes									
Cumulative GPA	2.43	0.81	3944	2.46	0.81	3305	2.43	0.81	3944
P(Ever Fail Course)	0.57	0.49	3944	0.56	0.50	3305	0.57	0.49	3944
P(HS Dropout)	0.07	0.26	4936	0.07	0.26	4936	0.05	0.23	3305
P(Attend College)	0.64	0.48	4936	0.64	0.48	4936	0.67	0.47	3305
Hours Worked & Bunching									
Works zero hours ($H = 0$)	0.38	0.48	6900	0.36	0.48	4936	0.36	0.48	3944
Academic year hours worked ($H \geq 0$)	11.99	13.62	6900	12.26	13.57	4936	12.24	13.53	3944
Academic year hours worked ($H > 0$)	19.26	12.56	4297	19.26	12.42	3141	19.08	12.44	2531
Control Variables									
Age (years)	16.9	0.97	6900	16.92	0.96	4936	16.92	0.96	3944
Hispanic	0.19	0.39	6900	0.18	0.38	4936	0.18	0.38	3944
Race									
White	0.62	0.48	6900	0.64	0.48	4936	0.65	0.48	3944
Black	0.19	0.40	6900	0.19	0.39	4936	0.17	0.37	3944
Other	0.18	0.39	6900	0.17	0.38	4936	0.18	0.38	3944
Maternal Education									
No degree	0.14	0.35	6900	0.13	0.34	4936	0.13	0.34	3944
High school	0.31	0.46	6900	0.32	0.46	4936	0.32	0.47	3944
Some college	0.18	0.38	6900	0.18	0.38	4936	0.18	0.38	3944
College	0.27	0.44	6900	0.28	0.45	4936	0.29	0.45	3944
Missing	0.10	0.30	6900	0.09	0.29	4936	0.09	0.28	3944
Paternal Education									
No degree	0.11	0.31	6900	0.11	0.31	4936	0.10	0.31	3944
High school	0.23	0.42	6900	0.23	0.42	4936	0.23	0.42	3944
Some college	0.14	0.35	6900	0.15	0.35	4936	0.15	0.36	3944
College	0.23	0.42	6900	0.24	0.43	4936	0.26	0.44	3944
Missing	0.28	0.45	6900	0.27	0.44	4936	0.25	0.44	3944
Grade									
9^{th} grade	0.08	0.27	6900	0.06	0.24	4936	0.05	0.23	3944
10^{th} grade	0.25	0.43	6900	0.25	0.43	4936	0.25	0.43	3944
11^{th} grade	0.31	0.46	6900	0.32	0.46	4936	0.31	0.46	3944
12^{th} grade	0.36	0.48	6900	0.37	0.48	4936	0.39	0.49	3944
Has siblings	0.77	0.42	6900	0.77	0.42	4936	0.78	0.42	3944
Attends rural school	0.16	0.37	6900	0.18	0.38	4936	0.19	0.39	3944

Note: The table shows descriptive statistics based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample is used to study cumulative and long-term outcomes. I include individuals only once, when I last observe them in high school. This sample includes only males.

Table B5. Descriptive Statistics across Academic-Year Samples (Females)

	Full Analytical Sample			Conditional on Transcript Data		
	Mean	SD	N	Mean	SD	N
Educational Outcomes						
Yearly GPA	2.73	0.87	6590	2.73	0.87	6590
Yearly P(Ever Fail Course)	0.26	0.44	6590	0.26	0.44	6590
Hours Worked & Bunching						
Works zero hours ($H = 0$)	0.45	0.5	10641	0.44	0.5	6590
Academic year hours worked ($H \geq 0$)	9.09	11.77	10641	8.99	11.43	6590
Academic year hours worked ($H > 0$)	16.59	11.33	5830	15.93	11.00	3719
Control Variables						
Age (years)	16.51	1.02	10641	16.49	1.02	6590
Hispanic	0.18	0.38	10641	0.16	0.37	6590
Race						
White	0.61	0.49	10641	0.63	0.48	6590
Black	0.22	0.41	10641	0.21	0.41	6590
Other	0.17	0.38	10641	0.16	0.37	6590
Maternal Education						
No degree	0.16	0.37	10641	0.15	0.35	6590
High school	0.31	0.46	10641	0.31	0.46	6590
Some college	0.20	0.40	10641	0.20	0.40	6590
College	0.26	0.44	10641	0.27	0.45	6590
Missing	0.08	0.27	10641	0.07	0.26	6590
Paternal Education						
No degree	0.11	0.31	10641	0.11	0.32	6590
High school	0.22	0.41	10641	0.23	0.42	6590
Some college	0.12	0.33	10641	0.14	0.34	6590
College	0.22	0.41	10641	0.24	0.43	6590
Missing	0.33	0.47	10641	0.29	0.45	6590
Grade						
9^{th} grade	0.13	0.34	10641	0.12	0.33	6590
10^{th} grade	0.3	0.46	10641	0.31	0.46	6590
11^{th} grade	0.31	0.46	10641	0.31	0.46	6590
12^{th} grade	0.26	0.44	10641	0.26	0.44	6590
Has siblings	0.78	0.42	10641	0.79	0.40	6590
Attends rural school	0.16	0.37	10641	0.18	0.39	6590

Note: The table shows descriptive statistics for the sample of person-wave observations, based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample includes only females.

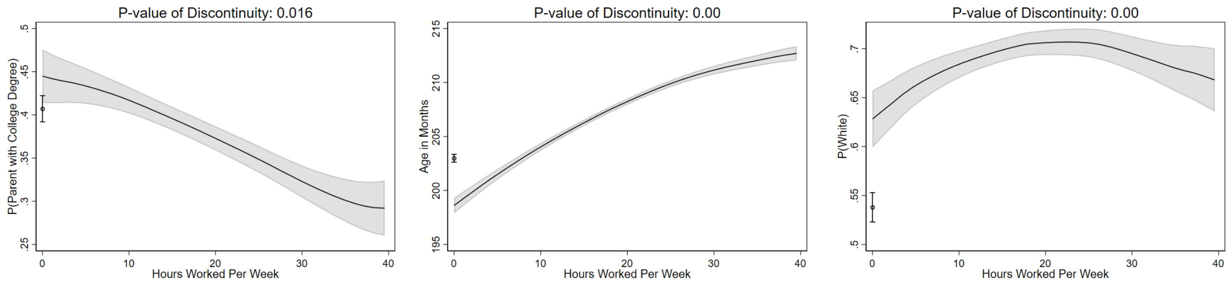
Table B6. Descriptive Statistics across Academic-Year Samples (Males)

	Full Analytical Sample			Conditional on Transcript Data		
	Mean	SD	N	Mean	SD	N
Educational Outcomes						
Yearly GPA	2.43	0.91	6032	2.43	0.91	6032
Yearly P(Ever Fail Course)	0.35	0.48	6032	0.35	0.48	6032
Hours Worked & Bunching						
Works zero hours ($H = 0$)	0.41	0.49	10378	0.4	0.49	6032
Academic year hours worked ($H \geq 0$)	10.59	13.01	10378	10.54	12.67	6032
Academic year hours worked ($H > 0$)	17.90	12.45	6138	17.44	12.06	3646
Control Variables						
Age (years)	16.6	1.01	10378	16.59	1.01	6032
Hispanic	0.18	0.39	10378	0.17	0.38	6032
Race						
White	0.63	0.48	10378	0.66	0.47	6032
Black	0.19	0.39	10378	0.16	0.37	6032
Other	0.18	0.39	10378	0.18	0.38	6032
Maternal Education						
No degree	0.13	0.34	10378	0.12	0.33	6032
High school	0.31	0.46	10378	0.32	0.47	6032
Some college	0.18	0.38	10378	0.18	0.38	6032
College	0.28	0.45	10378	0.30	0.46	6032
Missing	0.10	0.30	10378	0.09	0.28	6032
Paternal Education						
No degree	0.11	0.31	10378	0.10	0.30	6032
High school	0.23	0.42	10378	0.22	0.42	6032
Some college	0.14	0.35	10378	0.15	0.36	6032
College	0.24	0.43	10378	0.27	0.44	6032
Missing	0.28	0.45	10378	0.25	0.44	6032
Grade						
9^{th} grade	0.15	0.35	10378	0.13	0.34	6032
10^{th} grade	0.3	0.46	10378	0.30	0.46	6032
11^{th} grade	0.31	0.46	10378	0.32	0.47	6032
12^{th} grade	0.24	0.43	10378	0.25	0.43	6032
Has siblings	0.78	0.41	10378	0.79	0.41	6032
Attends rural school	0.17	0.37	10378	0.19	0.39	6032

Note: The table shows descriptive statistics for the sample of person-wave observations, based upon Add Health waves 1 and 2. Excluding age, control variables are indicators. This sample includes only males.

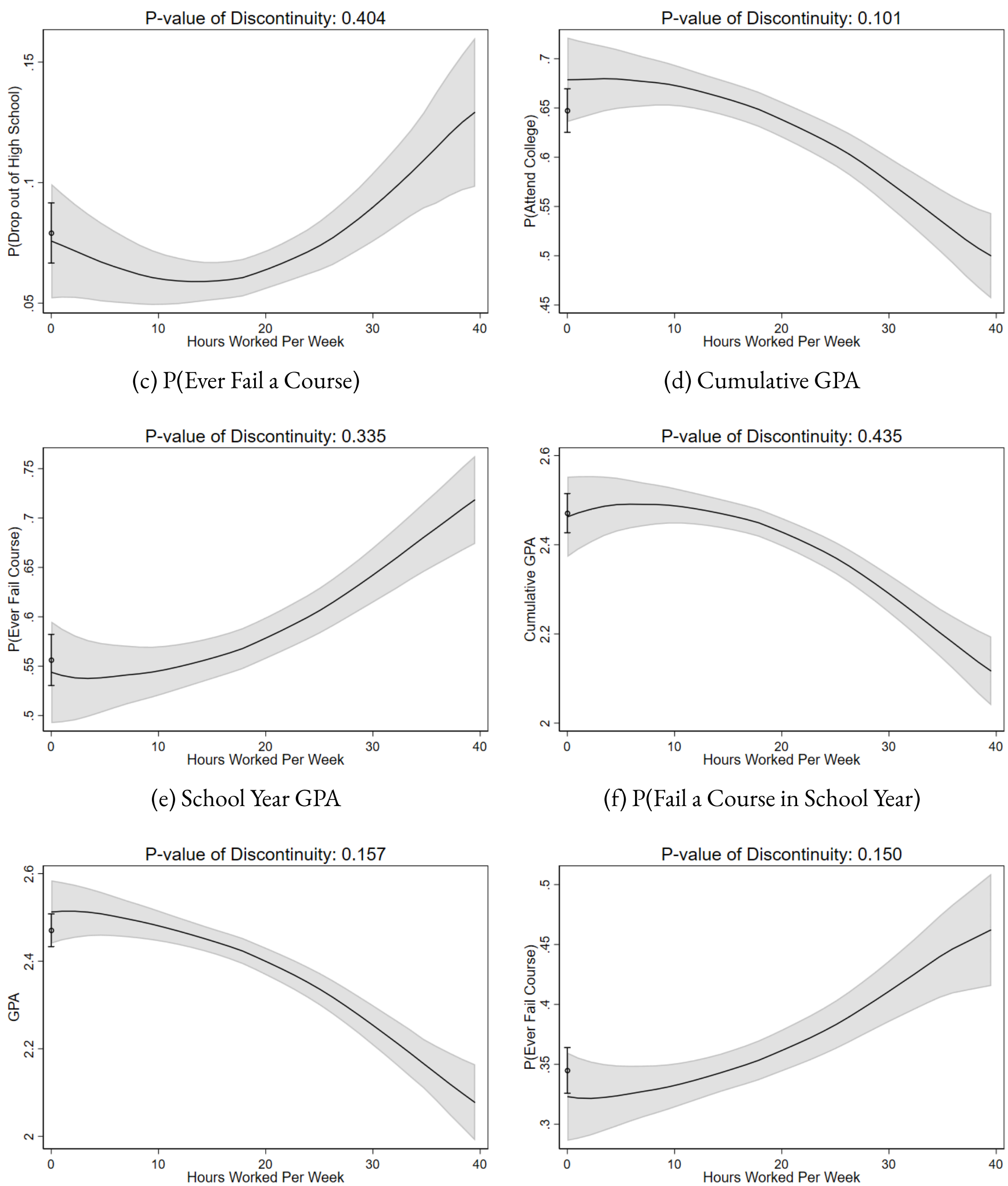
Figure B1. Evidence of $H^* < 0$ Types (Male Subsample)

(a) P(College-Educated Parent) (b) Age (in months) (c) P(White)



Note: Each panel shows the local linear estimate of the expected value of the variable conditional on H, as well as the expected value of the variable for those at $H = 0$. The p-value is for a test for whether there is a discontinuity at zero. 95% confidence intervals shown in gray. The sample includes only males and uses the academic-year sample, as defined in Section 2.2. Bandwidth=10.

Figure B2. Evidence of Selection at $H = 0$ (Male Subsample)



(a) P(High School Dropout)

(b) P(College Enrollment)

(c) P(Ever Fail a Course)

(d) Cumulative GPA

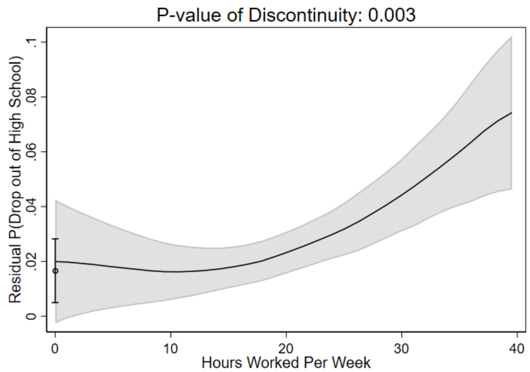
(e) School Year GPA

(f) P(Fail a Course in School Year)

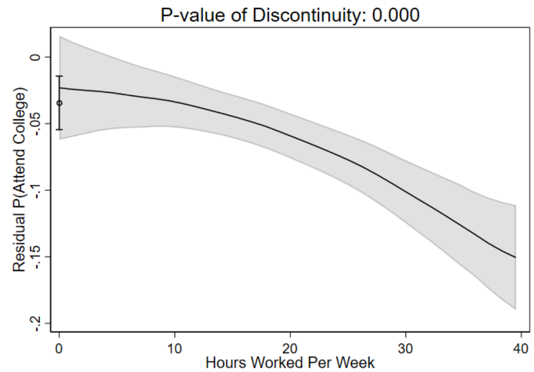
Note: The figure displays the local linear estimate of the expected value of educational outcomes conditional on the hours worked per week, estimated for $H > 0$, and the expected outcomes for those who do not work ($H = 0$). The p-value shown is for a test for whether there is a discontinuity at zero. 95% confidence intervals shown in gray. The sample includes only males. Bandwidth=10.

Figure B3. Evidence of Selection on Unobservables $H = 0$ (Male Subsample)

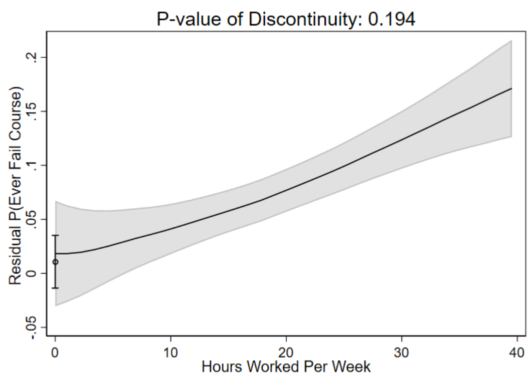
(a) P(High School Dropout)



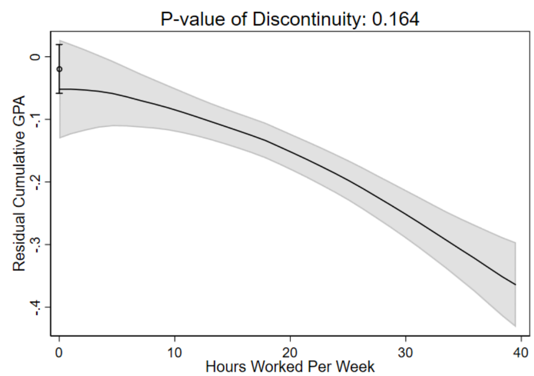
(b) P(College Enrollment)



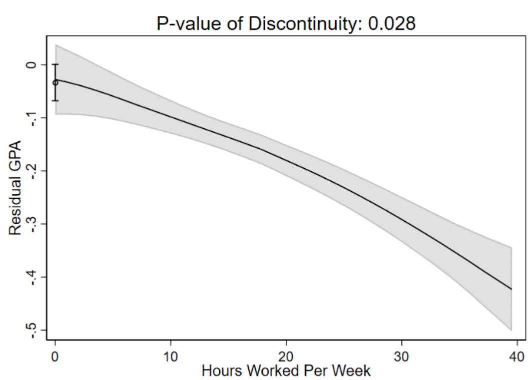
(c) P(Ever Fail a Course)



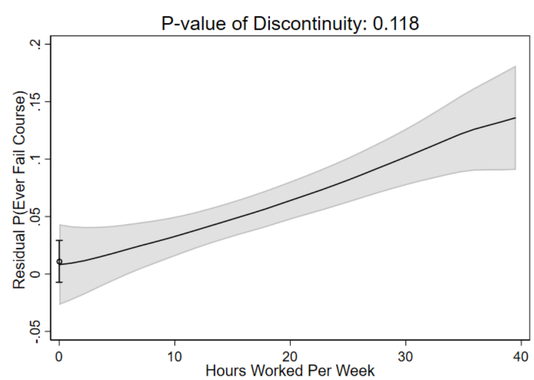
(d) Cumulative GPA



(e) School Year GPA



(f) P(Fail a Course in School Year)



Note: The figure shows the local linear estimate of the average value of the outcomes after controlling for observables (race, age, parental education, siblings, Hispanic origin, attending a rural school, and wave), estimated for $H > 0$. It also includes the residualized outcome for those who do not work ($H = 0$). The p-value shown is for a test for whether $\delta = 0$, which corresponds to a test for exogeneity. 95% confidence intervals shown in gray. The sample includes only males. Bandwidth=10.

Table B7. The Effect of Working Hours on Cumulative Educational Outcomes (First Time Observed in High School Sample)

	P(HS Dropout)	P(Enrolled in College)	P(Ever Failed Course)	GPA
	(1)	(2)	(3)	(4)
Panel A: Females				
β	0.0023**	-0.0052***	0.0104***	-0.0186***
	(0.0010)	(0.0018)	(0.0023)	(0.0033)
δ	-0.0017***	0.0036***	-0.0049***	0.0093***
	(0.0006)	(0.0011)	(0.0015)	(0.0022)
N	5595	5595	4323	4323
Mean	0.040	0.739	0.447	2.736
Panel B: Males				
β	0.0014	-0.0030*	0.0063***	-0.0083**
	(0.0011)	(0.0018)	(0.0022)	(0.0035)
δ	-0.0003	0.0004	-0.0020	0.0012
	(0.0007)	(0.0012)	(0.0015)	(0.0024)
N	4922	4922	3939	3939
Mean	0.066	0.653	0.568	2.457

Note: The table shows the effect of one additional hour of work per week on various cumulative educational outcomes. Controls include dummies for age-in-quarters, grade, race, maternal and paternal education, whether the individual has siblings, wave of interview, Hispanic origin, and urban vs. rural environment. The sample includes individuals the first time they are observed in high school. Bootstrapped standard errors in parentheses are based on 500 iterations. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table B8. The Effect of Working Hours on Educational Outcomes (Controlling for School Fixed Effects)

	Cumulative Outcomes				Academic-Year Outcomes	
	P(HS Dropout)	P(Enrolled in College)	P(Ever Failed Course)	GPA	GPA	P(Ever Failed Course)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Females						
β	0.0024***	-0.0053***	0.0110***	-0.0188***	-0.0188***	0.0061***
	(0.0009)	(0.0017)	(0.0021)	(0.0032)	(0.0031)	(0.0017)
δ	-0.0018***	0.0037***	-0.0062***	0.0113***	0.0103***	-0.0034***
	(0.0006)	(0.0012)	(0.0014)	(0.0023)	(0.0020)	(0.0011)
N	5600	5600	4332	4332	6581	6581
Mean	0.040	0.739	0.447	2.736	2.729	0.255
Panel B: Males						
β	0.0023**	-0.0060***	0.0050**	-0.0098***	-0.0113***	0.0037**
	(0.0011)	(0.0019)	(0.0023)	(0.0035)	(0.0032)	(0.0016)
δ	-0.0011	0.0029**	-0.0008	0.0016	0.0017	-0.0002
	(0.0008)	(0.0013)	(0.0017)	(0.0026)	(0.0022)	(0.0011)
N	4921	4921	3931	3931	6020	6020
Mean	0.066	0.653	0.568	2.457	2.433	0.352

Note: The table shows the effect of one additional hour of work per week on various educational outcomes. Bootstrapped standard errors in parentheses are based on 500 iterations. Controls include dummies for age-in-quarters, grade, race, maternal and paternal education, whether the individual has siblings, wave of interview, Hispanic origin, urban vs. rural environment, and school fixed effects.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

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