Essays on Student Employment

HARVARD UNIVERSITY
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Essays on Student Employment

presented by Monnica Chan

candidate for the degree of Doctor of Philosophy and hereby certify that it is worthy of acceptance.

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Essays on Student Employment

A Dissertation presented by

Monnica Chan

to

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Essays on Student Employment

Abstract

College tuition and fee rates have risen dramatically over the last twenty years. Grant aid dollars, however, have increased at a slower rate, especially at public four-year institutions, which were twice as expensive in AY2018-19 compared to AY1998-1999 (Ma, Baum, Pender & Libassi, 2018). How do students pay for college when grant aid is not enough? Two potential possibilities are through loans and work. A rich and growing body of literature has explored how much students borrow and what effects borrowing may have on students' short and long-term success. Whether and how much students are working as another strategy to pay for college, however, is still relatively unexplored. This dissertation contributes to the literature on student employment through two studies.

In the first study, I analyze data from four administrations of the National Postsecondary Student Aid Study (NPSAS:04, NPSAS:08; NPSAS:12; and NPSAS:16) using a combination of three decomposition methods: Oaxaca-Blinder, semiparametric reweighting, and recentered influence functions. These analytical strategies identify the patterns and factors affecting student employment over the last fifteen years. I find that the probability of a student working while in school has become more closely related to local unemployment rates over time, and less closely associated with college price. In the second part of the dissertation, I use data from the Kentucky Center for Statistics to estimate the causal effect of funding changes to the FWS Program. I find evidence that funding increases are often passed on to students in the form of additional FWS offers; decreases in funding appear to have no effect. There is little impact on students' academic outcomes. Combined, these studies shed light on the ways student employment intersects with college affordability and its implications for equitable student access and success.

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Acknowledgements

College affordability can be a crucial component of postsecondary student access and success. It is hard to go to school if it is unaffordable. I feel very fortunate to have enrolled in graduate school. The Harvard Graduate School of Education's (HGSE) Ph.D. program provided an opportunity to formally explore one facet of college affordability: student employment. The University and the Roy E. Larsen Doctoral Fellowship provided important material and financial support that enabled this work. I am particularly appreciative of Clara Lau and Eric Zeckman for guiding me through many program and dissertation deadlines.

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Introduction to the Dissertation

Over the last thirty years, college tuition and fee rates have risen faster than inflation and median family income. Between 1989 and 2019, median family income rose 26%, compared to a 278% increase in published tuition and fees at public four-year institutions and a 208% increase at public two-year institutions (Ma, Pender & Libassi, 2020). While grant aid is available to offset rising college costs for students, these resources have not kept pace. Even after accounting for grant aid, total costs of attendance have risen at public and private, two- and four-year institutions beyond the means of many families. In 2019, for example, the average net cost of attendance was $14,580 to attend a public two-year institution and $19,520 to attend a public four-year institution. These amounts are uncomfortably close to the average family income among the bottom quartile of families: $22,840 (Ma et al., 2020).

To pay for high college costs, many students turn to work during the school year (Baum, 2010; Goldrick-Rab, 2016). Over the last twenty years, anywhere from 49% to 62% of the undergraduates attending school in October also reported working (Flood, King, Rodgers, Ruggles & Warren, 2020). This number is estimated to be even higher when examining students across the entire academic year, which I discuss in more detail in the first dissertation paper.

Working while going to school, however, has not always been a rising trend. Rather, longitudinal patterns in the incidence and intensity of student employment tell a more a story of ups and downs. Between the 1970s and early 2000s, the share of 18-22 year old high school graduates who simultaneously worked and enrolled in college increased from 15% to 29% (Scott-Clayton, 2012). The average weekly hours worked among 18-22 year old undergraduates also increased between the 1970s and early 2000s (Scott-Clayton, 2012). Student employment rates and hours worked rose for other undergraduate populations as well, particularly among first-generation students attending a public four-year institution (Bound, Lovenheim & Turner, 2012; Weiss & Roksa, 2016).

These increases in student employment may be attributed to increased college enrollment rates (Weiss & Roksa, 2016), college costs and broader macroeconomic conditions (Scott-Clayton, 2012). Indeed, student demographic characteristics and state unemployment rates explain a large part of these historical trends (Scott-Clayton, 2012). The increase in the likelihood of work and number of hours worked between the 1970s and early 2000s were also positively correlated with trends in college pricing. In the lead up to the 2008 recession, student employment declined alongside decreases in net college price, providing further evidence of the link between student work and college costs (Scott-Clayton, 2012). The link between college costs and student employment decisions is also borne out by rich qualitative studies and surveys, which find many students working to pay for school (Baum, 2010; Goldrick-Rab, 2016).

After the 2008 recession, however, net college prices rebounded while student employment rates continued to fall. At public four-year institutions, the net cost of attendance increased 15% between 2008 and 2018, even after accounting for grant aid (Ma et al., 2020). At two-year public institutions, net cost of attendance increased 6% (Ma et al., 2020). Student employment among all undergraduates, on the other hand, fell from 77.6% to 63.9% between 2004 and 2016 (NCES, 2005; NCES, 2018). The average number of hours worked also fell during this time, from 29 to 28 hours per week (NCES, 2005; NCES, 2018).

These trends raise a series of questions at the foundation of this dissertation: What factors contribute to an undergraduate students' employment patterns? How have these factors changed over time? Do changing labor market conditions and trends in college prices relate to changes in student employment? What are the implications for student success?

In the first dissertation paper, I investigate the first three questions using decomposition methods. I use two measures student employment: the likelihood of working (e.g. an employment rate) and weekly hours worked. Decompositions methods identify how much of the recent decline in student employment incidence and intensity can be attributed to changes in factors such as student demographics, enrollment patterns, college costs, and local unemployment rates. Decomposition methods also identify how much of the decline is attributable to changes in how these factors predict the likelihood of student employment and weekly hours worked. I find evidence that student employment rates are increasingly tied to local unemployment rates. In addition, I find evidence that student employment rates are increasingly less associated with college costs, as measured by total costs of attendance, unmet need, and student borrowing behavior. These changes in how much local unemployment rates and college costs predict student employment explain much of the decline in overall student employment rates. These findings raise concerns that student employment may no longer be a consistent financial resource for students seeking to pay for college through work. Instead, students may have trouble finding a job, even if they want one.

What implications might these trends have on student success? Earlier quasi-experimental studies of student employment uniformly find that working while in school affects student earnings and time use. These studies present mixed evidence, however, as to the impacts of working on student grades and degree attainment (Stinebrickner & Stinebrickner, 2003; Scott-Clayton & Minaya, 2016; Scott-Clayton, 2011; Soliz & Long, 2016). One study, for example, found that students employed through the Federal Work Study (FWS) program experienced higher six-year Baccalaureate degree completion rates compared to students who otherwise would not have worked (Scott-Clayton & Minaya, 2016). Under another study, the same program in a rural setting appeared to have no statistically significant effect on degree completion (Scott-Clayton, 2011). Similarly, some studies find that working had a negative effect on students' grades (Stinebrickner & Stinebrickner, 2003; Scott-Clayton & Minaya, 2016), while studies in other contexts found no statistically significant effect on grades but a positive effect on credit accumulation rates (Scott-Clayton, 2011; Soliz & Long, 2016). Combined, these quasi-experimental studies suggest that the effects of student work varies across student subpopulations and local contexts.

The second paper of the dissertation complements the existing literature evaluating the impact of student employment. It does so by evaluating the impact of student employment from the supply side; I leverage exogenous changes in FWS funding to examine the impact of work study funding changes on institutional and student behavior in Kentucky. I find that increasing campus-based work study funding through the FWS program increases the number of work study offers and dollar amount offered to students. Decreases in funding had little impact on student financial aid offers. I also find that funding changes negligibly impacts student grades and credit completion, regardless of whether the change was an increase or decrease in funding. Given that student employment rates have declined in recent years, increasing FWS funding may increase the size and likelihood of a student being offered FWS through their financial aid package, with important affordability implications. Whether students take up these positions remains an area for future research.

As the United States again faces high unemployment rates due to the COVID-19 pandemic, understanding how student employment responds to economic downturns – and how campus-based work study programs may impact student employment rates and academic success – can inform current policy discussions. Combined, these papers raise important questions for policymakers to consider: how much do current financial aid policies expect students to work, and how accurately do those policies reflect current economic realities? More research is needed to understand how FWS positions function as a substitute for non-work study jobs. Nevertheless, this dissertation provides some evidence that institutions pass on funding increases to students and that the recent decline in student employment rates are attributable to increased influence from local unemployment rates. Offering FWS positions thus may help students access wages and work opportunities otherwise unavailable. As we – the postsecondary policy and research community – continue to build student-centered and student-focused educational systems, we must remember that students do not make their educational choices in a vacuum. Rather, local macroeconomic contexts and financial aid availability may profoundly affect the opportunities available to students and their ability to succeed.

1 Understanding Trends in Student Employment: How Determinants of Student Work Changed Before, During & After the 2008 Recession

1.1 Introduction

"Work to pay for school" is a common refrain. Paying for college through work earnings, however, has grown increasingly difficult due to rising college costs and stagnant wages. In 2020, the average cost of attendance – including room and board, books, and insurance – at a public four-year in-state institution was $19,490 (Ma, Pender & Libassi, 2020). Across all industries and all employees, the average gross hourly earnings was $28.74 (Bureau of Labor Statistics, 2021). At that rate, a student would need to work forty hours a week for seventeen weeks to pay the educational costs remaining after grant aid. Between taxes and the concentration of student employment in lower-paying industries such as trade and transportation, education and health services, and leisure and hospitality (Ruggles, *et al*., 2021), a student would likely need to work even more hours to cover college-related bills. Indeed, students often juggle courses, work schedules and other commitments to pay for school (Baum, 2010; Goldrick-Rab, 2016; Carnevale & Smith, 2018).

Longitudinal trends in college costs and student employment rates, however, suggest a more nuanced relationship between college costs and student employment. Although average levels of unmet financial need[1] have risen over the last twenty years, students' employment rates have declined. This is opposite of the historical, concurrent rise in college costs and student employment rates in the 1970s through the early 2000s. The near term decline is also contrary to

[1] Unmet financial need is equal to the amount of college costs remaining, after accounting for all sources of grant aid and a students' expected family contribution.

what one might expect if college-related expenses were a crucial determinant of students' employment decisions (Scott-Clayton, 2012). Students who do work are also working fewer hours per week, again going against the hypothesis that if college costs are rising, students might work more to meet those increased expenses. This paper describes these trends in student employment in more detail. I also identify how changes to factors that predict student employment – such as student demographic and enrollment patterns, college costs, and unemployment rates – help explain some, but not all, of the decline in student employment patterns over the last twenty years. By doing so, I shed light on why student employment trends have deviated from its historical relationship with college costs.

In this study, I use four waves of the National Postsecondary Student Aid Study (NPSAS) administered by the U.S. Department of Education between the 2003-2004 and 2015-2016 academic years. The NPSAS data provide student-level information on student employment, college enrollment, and financial aid. These data provide the most comprehensive, nationally representative snapshot of the educational costs students face, and how they pay for college (Radwin, *et al.*, 2018). I find evidence that how much local unemployment rates and college costs predict whether a student works has changed over time. Among students who do work, I find evidence that the determinants of hours worked for part-time student workers are fundamentally different from that of full-time workers.

These findings contribute to our understanding of student employment in multiple ways. First, they document reductions in the rate and intensity of student employment. Second, these results suggest that the relationship between student employment and its determinants has changed over time. Students may be relying less on work to pay for college than in the past. This leads to the third contribution: areas for future research. For example, I find that changes in student borrowing have little relation to changes in student employment rates. If students are working less and not necessarily borrowing more, even in the face of rising college costs, how are they paying for college? Future research might investigate how students use other financial products, such as credit cards, to pay for college. The relationships documented below between student demographics, enrollment patterns, costs, and student employment over time and across the distribution of student work intensity may help inform work in that area.

In the next section, I provide additional detail and background on recent changes to student employment rates and hours worked. Then, I describe the mean and quantile decomposition methods used to investigate whether and how much the declines in student employment are attributable to changes in college costs, student enrollment patterns and/or the local economy. I close by discussing my results and their implications for future research, financial aid policy, and educational practice.

1.2 Background

Between the 1970s and mid-2000s, student employment was on the rise (Scott-Clayton, 2012; Bound, Lovenheim & Turner, 2012). Scott-Clayton (2012) documents this trend for 18-22 year old full-time undergraduate students using October Current Population Survey (CPS) data. She finds that in 1970, 33% of 18-22 year old, full-time college students worked while enrolled; by 2000, the employment rate had risen to 52% (Scott-Clayton, 2012). Student workers in 2000 also spent more time at work than their 1970 peers. In 2000, 18-22 year old full-time undergraduates worked 22 hours per week, compared to 18 hours in 1970 (Scott-Clayton, 2012).

This trend reversed in the mid-to-late 2000s. Between 2005 and 2009, employment rates among 18-22 year old, full-time college students fell from 48% to 40% (Scott-Clayton, 2012).

Student workers also worked slightly less; by 2009, average hours worked per week had fallen from 22 hours at the beginning of the decade to 21 hours (Scott-Clayton, 2012). I extend these findings beyond 2009 in Figure 1.1. The figure presents the change in employment rates and hours worked for the same sample as Scott-Clayton (2012): 18-22 year old, full-time undergraduates with a high school credential. The figure also includes employment trends for the all undergraduates, regardless of age, enrollment intensity, and high school completion status. Student employment rates and hours worked declined during the 2008 recession for both groups, and has since leveled off. What might explain this more recent trend in student employment rates and hours worked?

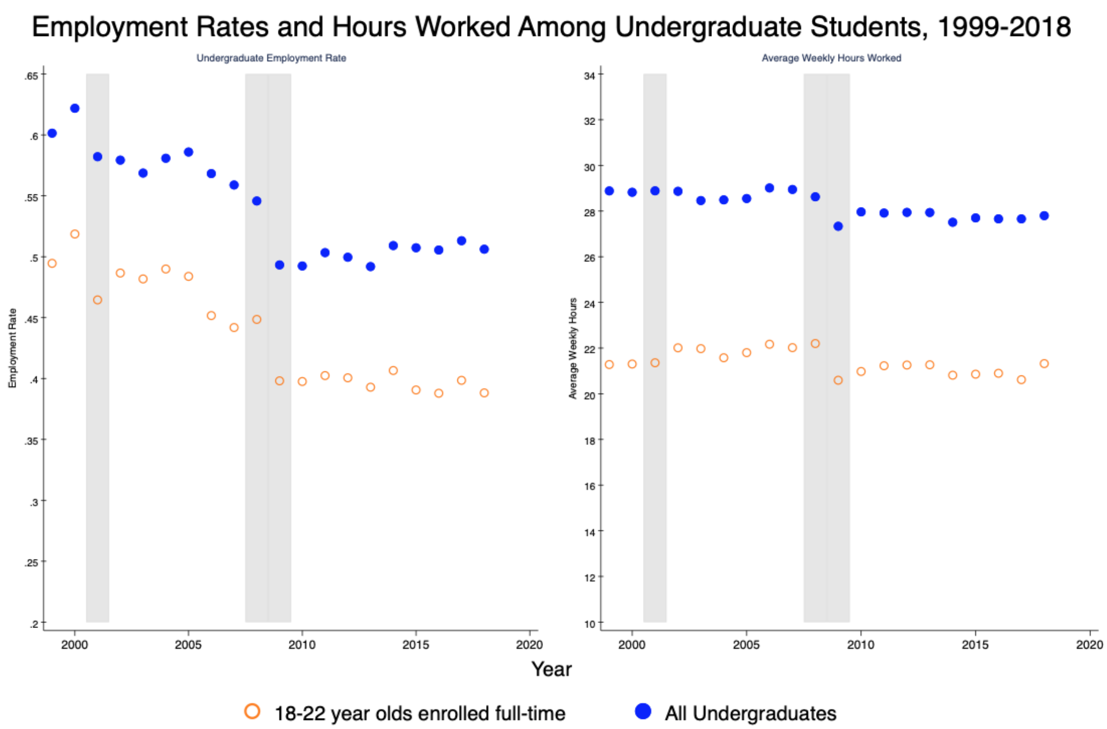

FIGURE 1.1: EMPLOYMENT RATES AND HOURS WORKED AMONG UNDERGRADUATES, 1999-2018

Notes: This figure shows the estimated undergraduate employment rate and average weekly hours worked using the October IPUMS-CPS database (Flood, *et al.*, 2020). Proportions are population-weighted averages.

Using regression methods, Scott-Clayton (2012) finds that student demographics and state unemployment rates explain 29% of the increase in weekly hours worked between 1970 and 2000. These measures are even more predictive of the decline in hours worked between 2000 and 2009 (Scott-Clayton, 2012). That demographic characteristics and broader unemployment rates can predict historical student employment rates aligns with other research. Older students are more likely to work than younger students and financially independent students are more likely to work than financial dependents (Baum, 2010; Scott-Clayton, 2012). Lower-income student workers are more likely to be female and non-white compared to higher-income student workers (Carnevale & Smith, 2018). Unemployment rates may also predict whether students work: sharp declines in the student employment rate occurred during both of the last economic recessions, when unemployment rates increased sharply (Figure 1.1).

The predictive power of demographic characteristics and macroeconomic conditions also applies to the employment patterns for youth and entry-level workers. This is relevant because the student labor market likely overlaps with the labor markets for entry-level workers and youth: undergraduate students are in the process of increasing their human capital through additional education. Student workers thus may only qualify for entry-level, lower-skilled jobs – even if not all undergraduates are young. The factors that predict the employment rate of youth and young adults include age, gender, parental education, and race (Lee & Staff, 2007; Hout, 2018). Economic contractions also often disproportionately affect younger and less-educated workers (Rothstein, 2017; Kalleberg & von Wachter, 2017).

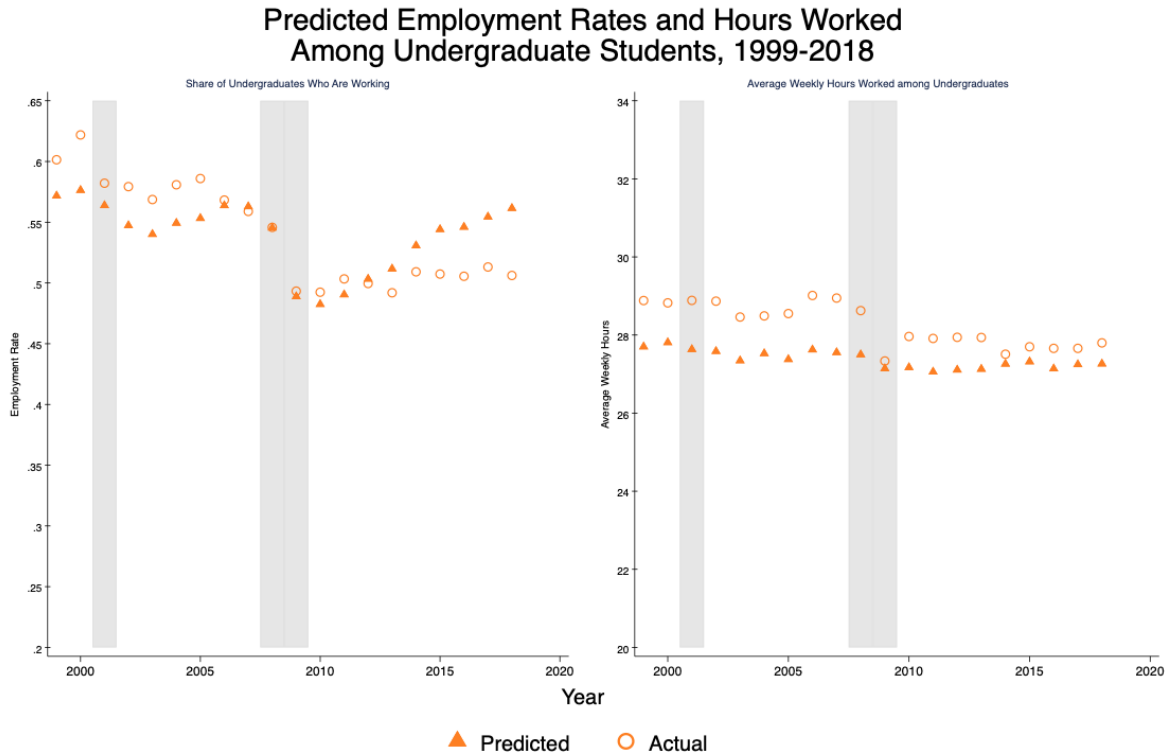

FIGURE 1.2: PREDICTED AND ACTUAL STUDENT EMPLOYMENT, 1999-2018

Notes: This figure shows predicted and actual student employment rates and weekly hours worked using the IPUMS-CPS database (Flood, *et al.*, 2020) for all undergraduate students. Proportions are population-weighted averages. See Appendix A for details.

Although Scott-Clayton's (2012) finds that changes to student demographic characteristics and state unemployment rates explain some of the decline through 2009, Figure 1.1 shows that both student employment rates and weekly hours worked had not recovered to pre-2008 levels by 2018 for both 18-22 year old full time undergraduates and the undergraduate population as a whole. Using IPUMS-CPS data (Flood, King, Rodgers, Ruggles & Warren, 2020), I extend Scott-Clayton's (2012) regression analysis to examine the predictive power of demographic characteristics and unemployment rates in more recent years. I predict student employment rates and weekly hours worked in the 1999-2018 time period for the entire undergraduate population and present those estimates in Figure 1.2 (see Appendix A for a discussion of how these predicted values were generated). Student demographics and state-year unemployment rates are strong predictors of actual employment rates particularly between 2006 and 2012. There is a growing divergence, however, in predicted and actual student employment rates after 2012. Demographic and state-year unemployment rates increasingly overpredict student employment rates between 2012 and 2018. Similarly, demographic characteristics and state-year unemployment rates also predict a decline in weekly hours worked, although these characteristics generally underpredict how much students are working and how large the decline in average hours worked has been over the last twenty years.

Given the differences in predicted and actual measures of student employment, might other factors, such as college enrollment patterns and college costs play a role? Because the CPS does not collect information on where students enroll and how much they pay for college, I use NPSAS data to examine how changes to student demographics, enrollment patterns, college costs and unemployment rates relate to student employment. The NPSAS provides a comprehensive, nationally representative snapshot of student-level data on college enrollment and financial aid receipt (Radwin, *et al.*, 2018). Specifically designed to better understand how students pay for college, the survey is well-suited to studying how student demographics, enrollment decisions and college costs are related to student employment decisions.

From the NPSAS data, it is clear that many of these factors have changed between the AY2003-04 and AY2015-16 survey administrations. Table 1.1 presents changes in student demographic characteristics and local unemployment rates across the four NPSAS survey waves used in this analysis. Between 2004 and 2016, undergraduate students are more likely to identify as non-White; this is also the case among undergraduate workers. Undergraduate students are also increasingly younger, with fewer financial resources on average than their earlier counterparts. Given that older students are more likely to work, the shift towards a slightly younger undergraduate population may explain some of the decline in unemployment. On the other hand, the decline in students' financial resources could lead to increases in student employment, because low- and middle-income students are more likely to work while in school than their higher income peers (Baum, 2010).

Changes to college enrollment patterns may also relate to student employment decisions. For example, balancing full-time college enrollment with long work hours and/or variable work schedules can be very difficult (Goldrick-Rab, 2016). This may explain some of the larger patterns seen across the country: students enrolled exclusively part-time during an academic year are much more likely to work 35 or more hours per week; and, students enrolled exclusively full-time worked at lower rates than their peers who enrolled exclusively part-time or mixed part-time and full-time enrollment (Baum, 2010). Across the time frame in this study, students were more likely to mix part-time and full-time enrollment over the course of AY2015-16 compared to AY2003-04; the share of students who ever enrolled full-time did not change dramatically over the 12 year period (Table 1.2). Across all survey years, working students were less likely to enroll exclusively full-time, compared to all undergraduate students as a whole (Table 1.2). By itself, one might expect the decline in exclusive full-time enrollment to predict increases in student employment. Other enrollment trends, however, suggest otherwise. For example, students were more likely to attend college out-of-state in 2016 compared to undergraduates in 2004 (Table 1.2). Because working college students are less likely to have out-of-state residency than undergraduate students overall, one might expect the rise in out-of-state enrollment to predict decreases in student employment.

TABLE 1-1 DEMOGRAPHIC CHARACTERISTICS, ANALYTIC SAMPLES BY NPSAS YEAR

	All Students (Workers & Non Workers)				Student Workers			
	2004	**2008**	**2012**	**2016**	**2004**	**2008**	**2012**	**2016**
Nonwhite (%)	0.322	0.337	0.379	0.431	0.319	0.333	0.357	0.408
	(0.007)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)	(0.007)	(0.005)
White (%)	0.645	0.636	0.591	0.535	0.648	0.641	0.614	0.559
	(0.007)	(0.004)	(0.007)	(0.004)	(0.007)	(0.004)	(0.007)	(0.005)
Black (%)	0.14	0.138	0.157	0.154	0.14	0.139	0.148	0.152
	(0.006)	(0.003)	(0.005)	(0.003)	(0.006)	(0.003)	(0.005)	(0.003)
Latino (%)	0.117	0.127	0.155	0.193	0.121	0.13	0.152	0.191
	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)	(0.006)	(0.004)
Asian American or Native Hawaiian Pacific Islander (%)	0.056	0.064	0.058	0.075	0.05	0.056	0.049	0.059
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Male (%)	0.427	0.435	0.435	0.439	0.425	0.432	0.429	0.418
	(0.003)	(0.003)	(0.005)	(0.001)	(0.004)	(0.003)	(0.005)	(0.003)
First-generation college student (%)	0.352	0.338	0.339	0.241	0.356	0.344	0.332	0.243
	(0.003)	(0.002)	(0.005)	(0.003)	(0.003)	(0.003)	(0.005)	(0.003)
Dependent college student (%)	0.508	0.534	0.488	0.504	0.493	0.515	0.47	0.465
	(0.005)	(0.003)	(0.012)	(0.003)	(0.005)	(0.004)	(0.012)	(0.004)
Independent with dependent(s) (%)	0.224	0.214	0.236	0.257	0.232	0.224	0.248	0.271
	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Independent without dependent(s) (%)	0.268	0.252	0.275	0.24	0.274	0.261	0.283	0.264
	(0.003)	(0.003)	(0.010)	(0.003)	(0.004)	(0.003)	(0.011)	(0.003)
Age (years)	26.299	25.847	26.414	25.777	26.396	26.015	26.587	26.181
	(0.085)	(0.061)	(0.190)	(0.057)	(0.091)	(0.069)	(0.200)	(0.070)
Income (2016 $)	66,600	65,700	56,200	59,100	65,600	63,600	56,500	57,800
	(470)	(300)	(820)	(450)	(490)	(310)	(760)	(580)
Expected Family Contribution (2016 $)	12,200	11,600	8,500	10,100	12,100	11,000	8,400	9,600
	(130)	(80)	(170)	(160)	(150)	(70)	(170)	(190)
Local unemployment rate (%)	6.029	4.617	8.887	5.25	6.027	4.612	8.828	5.219
	(0.036)	(0.026)	(0.055)	(0.019)	(0.037)	(0.027)	(0.057)	(0.018)
Number of Observations	65,960	91,110	78,830	71,500	48,900	71,490	46,910	46,470
Population Size	16,999,140	18,283,420	20,317,520	17,240,160	13,197,030	14,456,410	13,435,010	11,011,090

Notes: Analysis of NPSAS:04, NPSAS:08, NPSAS:12, and NPSAS:16 data. Sample includes all NPSAS survey respondents with complete demographic information who reported working any job (including work study) during the academic year. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000.

TABLE 1-2: ENROLLMENT CHARACTERISTICS, ANALYTIC SAMPLE BY NPSAS YEAR

	All Students (Workers & Non Workers)				**Student Workers**			
	2004	**2008**	**2012**	**2016**	**2004**	**2008**	**2012**	**2016**
Attended a 2-year non-profit institution in NPSAS year (%)	0.452	0.455	0.457	0.455	0.466	0.483	0.477	0.484
	(0.003)	(0.003)	(0.014)	(0.002)	(0.004)	(0.004)	(0.015)	(0.003)
Attended a 4-year non-profit institution in NPSAS year (%)	0.46	0.445	0.4	0.441	0.448	0.423	0.39	0.412
	(0.003)	(0.003)	(0.012)	(0.002)	(0.004)	(0.003)	(0.013)	(0.003)
Attended a for-profit institution in NPSAS year (%)	0.079	0.092	0.137	0.094	0.077	0.087	0.127	0.096
	(0.001)	(0.001)	(0.020)	(0.003)	(0.001)	(0.001)	(0.022)	(0.003)
Attended college in-state in NPSAS year (%)	0.887	0.866	0.821	0.818	0.9	0.879	0.839	0.839
	(0.003)	(0.004)	(0.018)	(0.003)	(0.004)	(0.004)	(0.020)	(0.004)
Lived on campus in NPSAS year (%)	0.157	0.159	0.135	0.158	0.132	0.132	0.105	0.126
	(0.004)	(0.002)	(0.005)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)
Ever enrolled full-time in NPSAS year (%)	0.646	0.638	0.676	0.654	0.619	0.602	0.638	0.613
	(0.005)	(0.003)	(0.009)	(0.003)	(0.005)	(0.004)	(0.011)	(0.004)
Only enrolled full-time in NPSAS year (%)	0.493	0.483	0.519	0.449	0.463	0.443	0.48	0.407
	(0.006)	(0.004)	(0.012)	(0.003)	(0.006)	(0.004)	(0.014)	(0.004)
Number of Observations	65,960	91,110	78,830	71,500	48,900	71,490	46,910	46,470
Population Size	16,999,140	18,283,420	20,317,520	17,240,160	13,197,030	14,456,410	13,435,010	11,011,090

Notes: Analysis of NPSAS:04, NPSAS:08, NPSAS:12, and NPSAS:16 data. Sample includes all NPSAS survey respondents with complete demographic information who reported working any job (including work study) during the academic year. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000.

In addition to student demographic changes and enrollment changes, changes in college costs may also relate to the decline in student employment rates and hours worked. Scott-Clayton (2012) draws on human capital theory to describe how credit constraints may be an important factor in determining student employment. Indeed, wages from employment can help students pay for books, living expenses, and other costs (Baum, 2010; Goldrick-Rab, 2016). Rich qualitive studies have also found that students may feel so credit constrained that they sacrifice their academic progress in order to work (Goldrick-Rab, 2016; Armstrong & Hamilton, 2013). Between 2004 and 2016, the average cost of college increased (Table 1.3). Students were more likely to borrow and borrowed slightly more, on average, in 2016 than in 2004 (Table 1.3). The average increase in student loans between 2004 and 2016, however, is smaller than the increase in unmet need after grant aid for the overall student population and for student workers. If students are working to make up the difference, one might expect changes in these measures of college cost (e.g. cost of attendance, unmet need, and borrowing amounts) to relate to changes in student employment.

TABLE 1-3: FINANCIAL CHARACTERISTICS, ANALYTIC SAMPLE BY NPSAS YEAR

	All Students (Workers & Non Workers)				Student Workers			
	2004	**2008**	**2012**	**2016**	**2004**	**2008**	**2012**	**2016**
Enrollment adjusted budget (2016 $)	14,100	15,700	17,300	18,600	13,600	15,000	16,500	17,500
	(100)	(100)	(200)	(100)	(100)	(100)	(200)	(100)
Unmet need after grant aid (2016 $)	5,500	6,400	8,100	8,800	5,300	6,200	7,700	9,300
	(100)	(100)	(200)	(100)	(100)	(100)	(200)	(100)
Total loans in NPSAS year (incl. PLUS loans, 2016 $)	2,700	3,500	3,700	3,500	2,700	3,400	3,600	3,300
	0	0	(100)	0	0	0	(100)	0
Ever borrowed a student loan (%)	0.338	0.382	0.414	0.377	0.338	0.376	0.41	0.373
	(0.002)	(0.002)	(0.010)	(0.002)	(0.002)	(0.002)	(0.011)	(0.003)
Ever borrowed a federal student loan (%)	0.323	0.342	0.397	0.354	0.323	0.336	0.391	0.352
	(0.002)	(0.001)	(0.010)	(0.002)	(0.002)	(0.002)	(0.010)	(0.003)
Ever borrowed a PLUS loan (%)	0.036	0.038	0.045	0.042	0.032	0.033	0.041	0.035
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Number of Observations	65,960	91,110	78,830	71,500	48,900	71,490	46,910	46,470
Population Size	16,999,140	18,283,420	20,317,520	17,240,160	13,197,030	14,456,410	13,435,010	11,011,090

Notes: Analysis of NPSAS:04, NPSAS:08, NPSAS:12, and NPSAS:16 data. Sample includes all NPSAS survey respondents with complete demographic information who reported working any job (including work study) during the academic year. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000.

1.3 Motivation

A more direct investigation that accounts for how changes in demographic trends, college enrollment patterns, college costs, and macroeconomic factors relate to student employment decisions is important given the ways student employment may reflect broader social and economic inequality. Students who work many hours, for example, have less time for schoolwork and may do poorly in school (Goldrick-Rab, 2016; Mayhew, *et al.*, 2016; Stinebrickner & Stinebrickner, 2003). The format of work (e.g., overnight shifts) and type of work (e.g., cleaning the bathrooms of peers) can also negatively affect students' social and academic outcomes by preventing equal immersion in the college community and limiting the amount of time available to spend on schoolwork (Jack, 2019; Goldrick-Rab, 2016; Armstrong & Hamilton, 2013). More affluent students, on the other hand, may view work as a place for socializing and community, especially in work places branded as "cool" or "high end" (Besen-Cassino, 2014). In addition, higher-income students are more likely to work in jobs and industries related to their career interests, through internships and apprenticeships, than their more affluent peers (Rivera, 2015; Carnevale & Smith, 2018; Armstrong & Hamilton, 2013). Student employment thus may serve as an avenue for "effectively maintained inequality" (Lucas, 2001), where some low-income students have access to higher education and work, but encounter a decidedly different experience than their well-resourced peers (Roksa & Velez, 2010; Weiss & Roksa, 2016).

Although student employment manifests social and education inequities, working while in school can also play important educational roles. For example, student employment can help students' cognitive & intellectual development (Mayhew, et al., 2016). Working can also provide students with potential career pathways and provide critical opportunities for workplace skill development (Hoffman & Collins, 2020).

Combined, the body of literature on student employment suggests that where students work, the nature of work, and how much students work can both manifest and mitigate broader socio-economic inequities. In this way, student employment embodies the broader, contradictory purposes of education by serving both sorting and educational functions (Labaree, 1997). Decomposition analyses are one framework for studying the sorting and educational functions of student employment. This is because the method identifies how much of the recent decline in student employment is attributable to changing relationships between student employment and its predictors and how much is attributable to changing levels of a given predictor variable. For example, this decomposition analysis finds that the relationship between local unemployment rates and student employment rates has grown stronger over time. It also finds that this strengthening relationship explains a large share of the decline in student employment rates between 2004 and 2016. In other words, student employment outcomes are increasingly related to broader macroeconomic factors; students may have trouble finding jobs, in spite of wanting one. This may signal that student employment's potential as an educational experience is waning – rather, who works and how much they work may be increasingly tied to broader social and economic conditions. This paper thus serves as a backdrop for understanding whether declining student employment trends is a sign of "effectively maintained" inequality, or increasing inequality.

1.4 Data

This paper analyzes data from the NPSAS, a nationally representative survey administered by the U.S. Department of Education (DOE). The DOE administers the NPSAS every four years. I focus on the four most recent administrations of the survey, conducted in the 2003-2004, 2007-2008, 2011-2012, and 2015-2016 academic years. Although some survey questions change over time, the eligibility requirements for both institutional and student sampling frames have remained generally consistent, allowing for comparison across survey waves (Wine, Siegel, & Stollberg, 2018).

Data collected through NPSAS include student enrollment records, educational experiences (e.g., high school academic experiences, major of study), financial aid receipt, employment history, income and expenses, and demographic background. Information contained in the NPSAS surveys are collected from student interviews, institutional reporting, and other administrative data sources such as the National Student Clearinghouse and the National Student Loan Data System (NSLDS). The NPSAS surveys utilize a two-stage sampling design. To create the sample, NPSAS statisticians first selected Title IV-eligible postsecondary institutions across strata based on institution level, control, and other characteristics. The sampling frame includes public, not-for-profit and for-profit institutions as well as four-year, two-year and less-than two-year institutions. Undergraduate and graduate students enrolled at these institutions were then sampled across strata based on their class standing and degree program. The NPSAS student records are thus meant to nationally represent all college students.

To compare employment rates and hours worked across surveys, I utilize information provided in interview protocols and codebooks to re-code items such that they are consistent across years (see Appendix Table 1.B for a description of variables). I adjust for inflation by converting students' expected family contribution, income, estimated costs of attendance, and financial aid amounts to 2016 dollars using the Consumer Price Index (CPI-U) from the U.S. Department of Labor. I also incorporate institutional Carnegie Classifications from the Integrated Postsecondary Education Data System (IPEDS) to account for changes in institutional classifications across NPSAS survey years (Radwin, Wine, Siegel, & Bryan, 2013).

Given prior research on the importance of unemployment rates in predicting student employment trends (Scott-Clayton, 2012), I use estimates of labor force participation provided at the county-level by the Local Area Unemployment Statistics (LAUS) program from the U.S. Bureau of Labor Statistics. LAUS provides information on county-level unemployment rates and labor force size. I aggregate this information to the commuting zone and state-level by summing across component counties (U.S. Bureau of Labor Statistics, 2020). I use these measures of the annual unemployment rate within the commuting zone (or state, if commuting zone-level rates are unavailable) of each NPSAS institution as a measure of labor market participation and activity. These aggregate measures of unemployment rates are subject to uncertainty due to differences in commuting zone definitions and accuracy of the underlying county estimates (Foote, Kutzbach & Vilhuber, 2017). This may result in an overestimate of the precision of my results (Foote, Kutzbach & Vilhuber, 2017).

When using survey data, it is also important to consider the representativeness of the sample. Across all decompositions, I use NCES-provided analytic weights to account for each observation's probability of being sampled. These weights are used to recover national estimates of the undergraduate population.[2] To confirm whether I used the analytic weights appropriately, I

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[2] Across all four survey years, I use the WTA000 analytic weight variable. In NPSAS:04, another weight variable (STUDYWT) is available. STUDYWT is nearly perfectly correlated with WTA000 ($\rho = 0.95$). I use WTA000 because it is recommended by NCES as the final analytic weight (Cominole, Siegel, Dudley, Roe & Gilligan, 2006).

compare population estimates and employment rates to other nationally representative surveys often used in population-based studies: the American Community Survey (ACS) and the CPS. I document these comparisons visually in Figures 1.3 and 1.4.

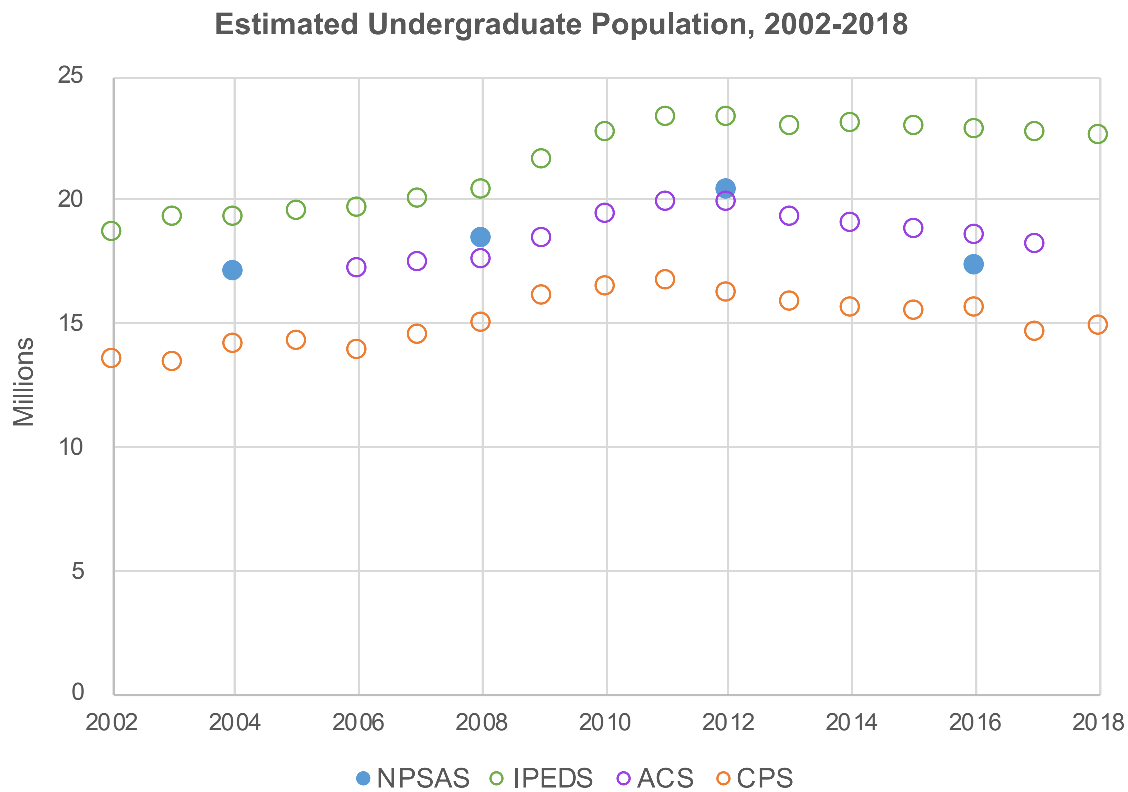

FIGURE 1.3: UNDERGRADUATE POPULATION ESTIMATES OVER TIME, ACROSS DATA SOURCES

Notes: This figure shows the estimated undergraduate population across data sources and years. Estimates were generated from the NPSAS:04, NPSAS:08, NPSAS:12, and NPSAS:16 surveys; the Integrated Postsecondary Education Data System (IPEDS); the IPUMS-ACS database (Ruggles, *et al.*, 2021); and the IPUMS-CPS database (Flood, *et al.*, 2020). Proportions are population-weighted averages. IPEDS estimates are based on 12-month undergraduate headcounts; because these counts may include students without a high school credential, the NPSAS, IPUMS-ACS and IPUMS-CPS estimates do not condition on high school degree completion.

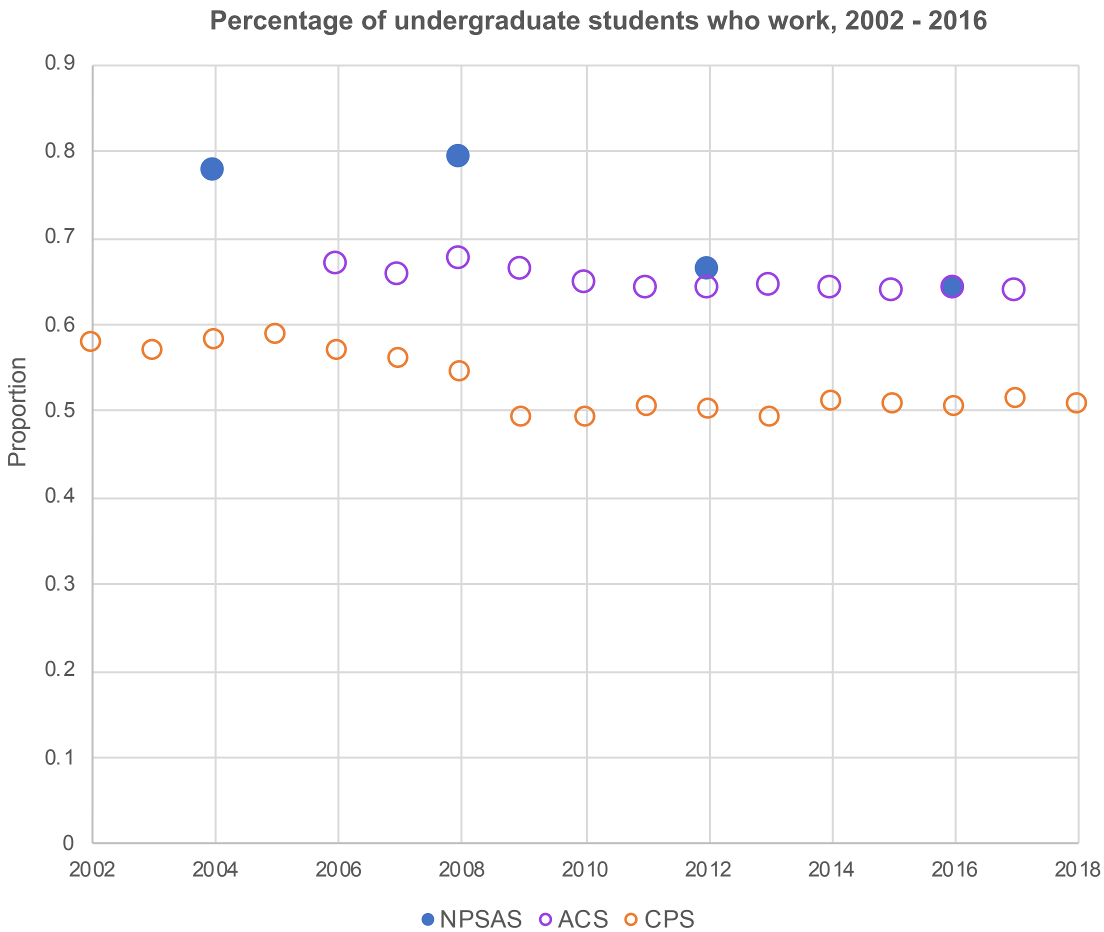

FIGURE 1.4: UNDERGRADUATE EMPLOYMENT RATES OVER TIME, ACROSS DATA SOURCES

Notes: This figure shows the estimated share of undergraduate population that has a job across data sources and years. Estimates were generated from the NPSAS:04, NPSAS:08, NPSAS:12, and NPSAS:16 surveys; the IPUMS-CPS database (Flood, *et al.*, 2020); and the IPUMS-ACS database (Ruggles, *et al.*, 2021). Proportions are population-weighted averages.

Figure 1.3 presents estimates of the undergraduate population size across multiple data sources. Both the NPSAS and IPUMS-ACS (Ruggles, *et al.*, 2021) estimates are slightly smaller than 12-month undergraduate headcounts reported by the U.S. Department of Education. The discrepancy seems reasonable because the 12-month undergraduate headcount is reported by institutions and may include students enrolled at multiple institutions. The NPSAS estimated population sizes and ACS estimated population sizes are both slightly larger than estimates from the IPUMS-CPS (Flood, *et al.*, 2020). The IPUMS-CPS data does not sample individuals living in institutional settings and is limited to respondents surveyed in October of the survey year. It thus may be expected that the CPS estimates of undergraduate population size are smaller than both the IPEDS 12-month enrollment counts and survey estimates from NPSAS and ACS, which represent counts across multiple months. Importantly, the trends across all data sources are similar.

Figure 1.4 compares survey-weighted trends in student employment rates from the NPSAS and the ACS and CPS (Figure 1.4). As with the overall sample estimations, the level of student employment is a better match between the NPSAS and ACS surveys, particularly in 2012 and 2016. Between 2006 and 2016, however, the ACS[3] estimates a much smaller decline in student employment rates (2.9 pp) compared to the NPSAS (13.8 pp) and CPS data (6.1 pp). Although the ACS has a larger sampling frame than the CPS, the CPS is designed to measure trends in local labor markets and is used by the Bureau of Labor Statistics to calculate local unemployment and employment rates. The CPS estimates do suggest a lower share of students are working compared to the NPSAS; nevertheless, the trends are similar. Furthermore, some of this difference may again be attributable to differences in the survey population. As with the estimates of the total undergraduate population, CPS data focuses on students enrolled in October, the month of the survey. The NPSAS data covers the entire academic year. These sample differences likely would result in some differences in student employment rates because employment rates vary over the course of a year (Geremew & Gourio, 2018). Given the general consistency in trends across the NPSAS and CPS, and explainable differences in levels, I interpret the findings presented below as nationally representative. As a robustness check I

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[3] Prior to 2006, the ACS survey sample excluded group quarters.

present unweighted estimates using the NPSAS data and compare my results using the NPSAS survey data to results from a similar analysis using the CPS[4].

The primary results analyze two subsamples of NPSAS student records. The first analytic sample includes undergraduate student records with complete demographic information, attending a postsecondary institution with available local unemployment rates. Across all four NPSAS survey waves, this encompasses 331,860 student records[5]. This sample represents the subpopulation of undergraduate students who enrolled at only one institution during the NPSAS survey years with complete demographic information. The second analytic sample further restricts the first analytic sample to undergraduate students who reported working more than zero hours a week, including work-study positions. This analytic sample is comprised of 246,200 student records across four survey administrations.

1.5 Methodological Approach

I use decomposition methods to estimate how changes in demographic characteristics, college costs, local unemployment rates, and enrollment patterns, relate to changes in student employment rates and hours worked per week. Specifically, I use one of the most popular decomposition methods, Kitagawa-Oaxaca-Blinder (KOB)[6], and two extensions of KOB decompositions. The first extension uses semi-parametric reweighting to calculate the comparison group used in the decomposition (DiNardo, Fortin, & Lemieux, 1996). This extension relaxes the linearity assumption undergirding KOB decompositions. It has been used by other education researchers to understand increases in students' time to degree (Bound,

[4] Decomposition results using ACS data are available upon request.
[5] Sample counts are rounded in accordance with NCES disclosure guidelines.
[6] Also known as Oaxaca-Blinder decomposition.

Lovenheim, & Turner, 2012) and student borrowing trends over time (Hershbein & Hollenbeck, 2015). In addition to reweighting, this paper also uses recentered influence functions (RIFs) to decompose changes in hours worked at the 25th and 75th percentile of the distribution. This is necessary because KOB decompositions are limited to the mean. RIFs estimate how changes in predictor variables affect statistical parameters such as means, medians, and other τth quantiles of an outcome variable (Firpo, Fortin & Lemieux, 2009).[7]

The basic KOB decomposition relies on a linear regression model:

$$(1)\ Y_{i,j} = \beta_0 + \beta_1 \overrightarrow{Demographics} + \beta_2 \overrightarrow{CollegeCosts} + \beta_3 \overrightarrow{UR} + \beta_4 \overrightarrow{Enrollment} + \epsilon_i$$

$$\text{for } j = A, B$$

$Y_{i,j}$ represents two measures of student employment: (1) the likelihood of working for student, i, in year , j, and (2) the weekly number of hours worked, excluding non-workers. Students' race, gender, age, financial dependency status, expected family contribution (logged), income (logged), and parental education level comprise the demographic characteristics in the model.[8] I use inflation-adjusted measures of estimated cost of attendance (logged), federal student loan amounts,[9] and unmet financial need after grant aid as measures of the college costs students face. I also proxy for local labor market health with local unemployment rates. Student

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[7] RIFs are related to influence functions, a commonly used statistic in econometrics; the influence function is "re-centered" by the addition of q_τ in Equation (3).
[8] To relax functional form assumptions, I divide students into five age categories: 24 or younger, 25-24, 35-44, 45-54 years old, and 55 and older.
[9] I include the amount of Parent PLUS loans borrowed and the total amount of federal undergraduate student loans borrowed as separate predictor variables. Across NPSAS years, the eligibility requirements (including borrowing limits) changed for these loan programs; differential changes across loan programs may have a direct impact on student employment.

enrollment patterns are captured by whether the student enrolled at a two- or four-year institution; whether the institution was for-profit; whether the student enrolled in-state; whether the student enrolled full-time; and whether the student lived on campus.

Unlike Scott-Clayton (2012), and the predicted trends presented in Figure 1.2, KOB decompositions identify how changes to student employment relate to both changes in observable characteristics and changes in the relationship between observable characteristics and employment rates (e.g. $\beta_1, \beta_2, \beta_3, \beta_4$ in Equation (1) above). In contrast, the predictions generated in Figure 1.2 and Scott-Clayton (2012) rely on a single regression model estimated on survey responses pooled together over time. This single regression model forces the relationship between student employment rates and student demographic characteristics to be the same, across all years in the analysis. While the diversion in predicted and observed student employment rates in the CPS may be due to other factors, like college costs, it is also possible that the diversion results from changing relationships between student demographic characteristics, unemployment rates, and student employment rates. KOB decompositions help to account for changes in the relationship between different predictors of the regression model and the outcome of interest.

In the following sub-sections, I first describe traditional KOB decompositions in more detail. Then, I briefly discuss the reweighting process and RIFs. Across all estimations, I bootstrap for statistical inference by using the NCES-provided bootstrap replicate weights and estimating the decomposition results 200 times.[10] These decomposition methods and

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[10] Each bootstrap sample is created based on the NCES-provided bootstrap weights, WTA001-WTA200. In addition, some sampling strata in the NPSAS:12 sample only contained a single sampling unit. For these strata, I use the average of the variances from other strata with multiple sampling units, a slightly more conservative variance estimation calculation than treating the strata with single sampling units as certainty units and/or centering the sampling unit at the overall population mean (Appendix Table 1.C).

bootstrapping procedures follow Hershbein & Hollenbeck (2015), who used these methods and multiple NPSAS survey waves to examine changes in student borrowing over time.

1.5.1 Differences at the Means: Kitagawa-Oaxaca-Blinder Decomposition

The KOB decomposition uses ordinary least squares (OLS) regressions to estimate the relationship between observable characteristics and outcomes such as the likelihood of working while enrolled and the number of hours worked per week. In a traditional KOB decomposition, the estimated coefficients, β, from the regression in equation (1) are used to calculate:

$$
\begin{aligned}
(2)\ E[Y_B | j = B] - E[Y_A |\ j =\ A] &= \{E[X_B] - E[X_A]\}\beta_A + \{E[X_B](\beta_B - \beta_A)\} \\
&= (\bar{X}_B - \bar{X}_A)\hat{\beta}_A + \bar{X}_B\big(\hat{\beta}_B - \hat{\beta}_A\big) \\
&= \widehat{\Delta}_X + \widehat{\Delta}_S\ ,
\end{aligned}
$$

where Y_A and Y_B represent the outcome for two different cohorts (e.g., the average hours worked per week for first-time undergraduates surveyed in either the NPSAS:04 cohort or the NPSAS:16 cohort). The difference between Y_A and Y_B is "decomposed" into two components: (1) changes in the average sample characteristics of Group A compared to Group B, $\widehat{\Delta}_X$, often referred to as a composition effect; and (2) changes in the relationship between various observable characteristics and the outcome, $\widehat{\Delta}_S$, also known as a structural effect. Both of these components rely on the linearity assumptions of OLS and the choice of the reference group in equation (1). Equation (2) illustrates how the estimated $\hat{\beta}$s from equation (1) determine the composition ($\widehat{\Delta}_X$) and structural effects ($\widehat{\Delta}_S$).

Implicit in the decomposition method is the assumption that equation (1) accurately captures the relationship between the variables of interest and the outcome. If equation (1) inaccurately describes the relationship between students' demographic characteristics, college costs, local unemployment rates, and enrollment patterns with their decision to work, then the composition and structural effect estimates will be inaccurate.

To check whether equation (1) inappropriately captures the relationship between these factors and either the likelihood of student employment or the weekly hours worked among student workers, I present multiple specifications after the main results. First, I examine the stability of my results to using the reweighted counterfactual reference group instead of the original comparison groups (Fortin, Lemieux, & Firpo, 2011; DiNardo, Fortin, & Lemieux, 1996). Students enrolled in AY2003-04, for example, would no longer be compared directly to AY2015-16 students, but rather a counterfactual 2016 cohort, where AY2015-16 students are weighted based on their similarity to the 2004 cohort. Reweighting the distribution of students in AY2015-16 to match those in AY2003-04 loosens the linearity assumptions in the KOB decomposition (see Appendix C for additional detail). Then, I examine how my estimates change across alternative specifications of my predictor variables (e.g., a continuous measure of age, untransformed cost variables, etc).

Traditional KOB decompositions are also limited to examining changes at the mean. The range of student employment patterns, however, underscores the importance for examining changes at other parts of the student employment distribution. Some students, for example, work very few hours a week while others may be working full-time (Baum, 2010). This is clear from the distribution of hours worked in the NPSAS survey: at the 25th percentile, students work approximately 20 hours per week; at the 75th percentile, students work approximately 40 hours per week. Decomposing these parts of the distribution separately allows for heterogeneity in the relationships between observable characteristics and work intensity. In the next section, I describe the remaining extension of KOB methods: RIFs.

1.5.2 Decompositions Beyond the Mean

In this section, I describe how I use RIFs to decompose distributional differences (Firpo, Fortin & Lemieux, 2018). Estimating this relationship independently across high (e.g., 75th percentile) and low (e.g., 25th percentile) levels of work intensity is important because changes in the patterns and predictors of work intensity may vary across part-time and full-time workers. In education, RIFs have been used by Hershbein and Hollenbeck (2015) to decompose distributional differences in student borrowing, using NPSAS data. Equation (3) defines the RIF:

$$(3)\ RIF(Y; q_\tau, F_Y) = q_\tau + \frac{\tau - \mathbf{1}\{Y \leq q_\tau\}}{f_Y(q_\tau)},$$

where q_τ is the unconditional quantile of the outcome Y, and $f_Y(q_\tau)$ is the density of Y at q_τ. The indicator function indicates whether the value of the outcome variable is above or below q_τ. I estimate the RIF value for each observation using equation (1), at the quantile of interest (Firpo, Fortin & Lemieux, 2018). These estimated RIF values represent the new outcome variable in a KOB-style decomposition; if the statistical parameter of interest is set to the mean, the results match those from the Oaxaca-Blinder decomposition. In practice, I estimate traditional KOB decompositions, reweighted decompositions, RIF values at the distributional statistic of interest, and distributional decompositions using Rios-Avila's (2020) Stata package RIF.

1.6 Results

Below, I discuss how changes to demographic characteristics, enrollment patterns, local unemployment rates, and college costs over time relate to the decline in student employment rates and weekly hours worked. These factors can explain changes in student employment in two ways. First, measurable changes to who enrolls in college; where they enroll; whether jobs are available; and how much college costs could change observed student employment rates and hours worked, even if the relationship between these factors and student employment remained constant over time. Alternatively, the relationship between these factors and student employment rates and intensity could change over time. This might occur due to changing policies; shifts in students' preferences for work; and/or changes to employer preferences for hiring students, among other things. Often, both of these changes – in the level of a factor, and in its relationship to student employment rates and intensity – occur. First, I focus on the decomposition of student employment rates between 2004 and 2016 and intervening years. Then, I present decomposition results for average weekly hours worked, followed by a discussion of my results across the distribution of weekly hours worked.

1.6.1 Changes in Student Employment Rates

While the overall story of student employment is one of decline, I find that the share of students working increased slightly between 2004 and 2008, from 77.6% to 79.1%. This increase was eliminated between 2008 and 2012, when the share of students working fell dramatically from 79.1% of undergraduate students to 66.1%. The decline in student employment rates continued between 2012 and 2016 (Table 1.4). Because results from the subperiod vary from the overall 12-year period, I first discuss changes to student employment for the overall 2004-2016 period, followed by each individual subperiod.

TABLE 1-4: TRADITIONAL KITAGAWA-OAXACA-BLINDER DECOMPOSITION,
CHANGES IN THE LIKELIHOOD OF WORKING OVER TIME, 2004-2016 AND INTERVENING SUBPERIODS

	2004-2016	**2004-2008**	**2008-2012**	**2012-2016**
Likelihood of Working (%, Year 2)	0.639***	0.791***	0.661***	0.639***
	(0.003)	(0.002)	(0.003)	(0.003)
Likelihood of Working (%, Year 1)	0.776***	0.776***	0.791***	0.661***
	(0.002)	(0.002)	(0.002)	(0.003)
Simple Difference (Year 2 – Year 1)	-0.138***	0.014***	-0.129***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.004)
Total Compositional Effect	-0.012***	0.010***	-0.054***	0.053***
	(0.003)	(0.003)	(0.006)	(0.009)
Total Structural Effect	-0.125***	0.005	-0.076***	-0.076***
	(0.005)	(0.004)	(0.006)	(0.009)
Sample Size, Year 1	65,960	65,960	91,110	78,830
Sample Size, Year 2	71,500	91,110	78,830	71,500

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

Between 2004 and 2016, 90% of the decline is attributable to structural changes, particularly to enrollment patterns and local unemployment rates. This suggests that the relationship between student employment and its determinants changed over the last two decades. For example, in both 2004 and 2016, unemployment rates are negatively associated with the likelihood of working. In 2016, however, a one percentage point change in the unemployment rate predicts nearly three times the change in student employment rates than in 2004.[11] Mechanically, this helps explain the large structural effect attributed to unemployment

[11] Regression results available upon request.

rates in Table 1.5. Substantively, this result suggests that the relationship between local unemployment rates and student employment rates has shifted. The larger predictive power of local unemployment rates in 2016 may reflect some of the lingering economic effects and uneven recovery of the 2008 recession. The decline in student employment thus may be attributable to students being more reliant on broader macroeconomic conditions than in 2004.

TABLE 1-5: DETAILED DECOMPOSITION OF THE LIKELIHOOD OF WORKING OVER TIME, 2004-2016

	2004-2016	**2004-2008**	**2008-2012**	**2012-2016**
Simple Difference (Year 2 – Year 1)	-0.138***	0.014***	-0.129***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.004)
Compositional Effects				
Demographic Changes	-0.020***	-0.002*	-0.013***	-0.004***
	(0.002)	(0.001)	(0.002)	(0.001)
Financial Changes	0.000	0.003***	0	0.000
	(0.002)	(0.001)	(0.001)	(0.000)
Unemployment Rate	0.013***	0.010***	-0.037***	0.060***
	(0.002)	(0.003)	(0.006)	(0.008)
Enrollment Changes	-0.005***	-0.001	-0.003**	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Total Compositional Effects	-0.012***	0.010***	-0.054***	0.053***
	(0.003)	(0.003)	(0.006)	(0.009)
Structural Effects				
Demographic Changes	-0.003	0.029	-0.026	-0.007
	(0.026)	(0.026)	(0.023)	(0.022)
Financial Changes	0.122~	-0.101	0.215*	0.007
	(0.070)	(0.071)	(0.086)	(0.091)
Unemployment Rate	-0.057**	0	-0.008	-0.069**
	(0.018)	(0.016)	(0.010)	(0.023)
Enrollment Changes	-0.052**	0	0.018	-0.069***
	(0.017)	(0.015)	(0.016)	(0.017)
Intercept Changes	-0.136~	0.076	-0.275**	0.062
	(0.071)	(0.072)	(0.088)	(0.088)
Total Structural Effects	-0.125***	0.005	-0.076***	-0.076***
	(0.005)	(0.004)	(0.006)	(0.009)
Sample Size, Year 1	65,960	65,960	91,110	78,830
Sample Size, Year 2	71,500	91,110	78,830	71,500

Notes: ~ $p<0.1$; * $p<0.05$; ** $p<0.01$; *** $p<0.001$. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

Results from the subperiods illustrates this in more detail (Table 1.5). For example, between 2004 and 2008, the 1.4 pp ($p<0.001$) increase in student employment rates is nearly entirely attributable to the decline in local average unemployment rates, which fell from 6.0% for AY2003-04 students to 4.6% for AY2007-08 students. Between 2008 and 2012, rising local average unemployment rates explained 3.7 pp ($p<0.001$) of the 12.9 pp ($p<0.001$) decline, or 28% of the change. An additional 1.3 pp ($p<0.001$) decline is predicted by changes to student demographic characteristics, such as falling average income and a rise in financially independent students. Between 2012 and 2016, student employment rates continued to fall; however, the model does not differentiate between structural and compositional effects.

To relax the linearity assumption in the OLS model, I compare these results to a weighted counterfactual. These results compare the likelihood of student employment in a prior year, such as 2004, to the likelihood of student employment in a later year, such as 2016, where student records are weighted based on their similarity to the 2004 cohort. The closer the calculated 2016 counterfactual is to the actual 2004 employment rate, the larger role observable changes in determinants play in explaining the 2004-2016 change. Table 1.6 presents these results. In the 2004-2016 decomposition, the reweighted 2016 counterfactual employment rate (63.7%, $p<0.001$) is indistinguishable from the observed 2016 employment rate (63.9%, $p<0.001$). This suggests that the majority of the change is due to structural changes in the student labor market; substantively, this is similar to the traditional KOB estimates, which found that a majority of the 2004 to 2016 decline in student employment rates resulted from structural effects.

TABLE 1-6: REWEIGHTED DECOMPOSITION OF HOURS WORKED PER WEEK

	2004-2016	2004-2008	2008-2012	2012-2016
Likelihood of Working (%, Year 2)	0.639	0.791	0.661	0.639
	(0.003)	(0.002)	(0.003)	(0.003)
Likelihood of Working (%, Counterfactual Year 2)	0.637	0.788	0.719	0.575
	(0.007)	(0.025)	(0.013)	(0.020)
Likelihood of Working (%, Year 1)	0.776	0.776	0.791	0.661
	(0.002)	(0.002)	(0.002)	(0.003)
Simple Difference (Year 2 – Year 1)	-0.138***	0.014***	-0.129***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.004)
Total Compositional Effect	0.001	0.002	-0.058***	0.064**
	(0.007)	(0.025)	(0.012)	(0.020)
Total Structural Effect	-0.139***	0.012	-0.072***	-0.086***
	(0.007)	(0.025)	(0.013)	(0.020)
Sample Size, Year 1	65,960	65,960	91,110	78,830
Sample Size, Year 2	71,500	91,110	78,830	71,500

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200). The counterfactual estimate of hours worked is generated by reweighting Year 2 records by their similarity to Year 1 using a logit model similar to Equation (1) in the text.

Between 2004 and 2008, the reweighted counterfactual is again more similar to the reweighted year than the base year (2004). Unlike the traditional KOB estimates, the magnitude of these estimates suggest that a majority of the change is due to structural impacts, although the estimate is imprecise. Examining the determinants in more detail, unemployment rate changes between 2004 and 2008 predict a similar-in-magnitude change in employment rates (7.8 pp, p<0.01) as in the traditional KOB. Under the reweighted 2008 counterfactual, however, the overall compositional impacts become insignificant (Table 1.6). The differences between the traditional and reweighted KOB decompositions suggest that the OLS models for predicting student employment in 2004 and 2008 may be inaccurate and I interpret the traditional KOB results for this subperiod with caution.

TABLE 1-7: DETAILED REWEIGHTED DECOMPOSITION OF LIKELIHOOD OF WORKING OVER TIME

	2004-2016	2004-2008	2008-2012	2012-2016
Simple Difference (Year 2 – Year 1)	-0.138***	0.014***	-0.129***	-0.023***
	(0.004)	(0.003)	(0.003)	(0.004)
Compositional Effects				
Demographic Changes	-0.020***	0.018	-0.015***	-0.004
	(0.003)	(0.012)	(0.004)	(0.007)
Financial Changes	0.000	0.001	-0.001	0.000
	(0.002)	(0.007)	(0.002)	(0.002)
Unemployment Rate	0.023***	0.078**	-0.035***	0.076***
	(0.004)	(0.024)	(0.005)	(0.010)
Enrollment Changes	-0.008***	-0.057**	0.011~	-0.025***
	(0.002)	(0.020)	(0.006)	(0.005)
Structural Effects				
Demographic Changes	-0.042	0.095	-0.161~	-0.089
	(0.056)	(0.258)	(0.085)	(0.135)
Financial Changes	0.252~	-0.187	1.432**	0.552
	(0.144)	(1.535)	(0.457)	(0.519)
Unemployment Rate	-0.090**	0.038	-0.031	0.011
	(0.033)	(0.072)	(0.049)	(0.096)
Enrollment Changes	0.000	0.230	-0.167~	0.112
	(0.044)	(0.201)	(0.086)	(0.191)
Intercept Changes	-0.215	-0.175	-1.132***	-0.669
	(0.142)	(1.002)	(0.340)	(0.482)
Sample Size, Year 1	65,960	65,960	91,110	78,830
Sample Size, Year 2	71,500	91,110	78,830	71,500

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

For the 2008-2012 subperiod, the compositional effects estimated by the reweighted decomposition echoes the traditional decomposition. Changes to observable demographic, cost, enrollment, and macroeconomic factors explain 5.8 pp (p<0.001) of the 12.9 percentage point decline (Table 1.6). The remainder of the decline (7.2 percentage points, p<0.001) is explained by structural effects; these estimates from the reweighted decomposition are nearly identical to those from the traditional decomposition (5.4 pp and 7.6 pp, respectively). As with the traditional decomposition, rising unemployment rates in this period explain 3.5 pp ($p<0.001$) of the decline in student employment (Table 1.7). Demographic changes also play a role, predicting 1.5pp of the decline ($p<0.001$). Changes to family income played the largest role; between 2008 and 2012, average family income fell $8,500 (Table 1.1). Because there was a slightly positive relationship between family income and the likelihood of work, this resulted in an overall, negative compositional effect. Under the reweighted counterfactual, the impact of enrollment changes becomes marginally significant (1.1pp, $p<0.1$).

The aggregate structural effects are nearly identical across the reweighted and traditional decompositions for the 2008-2012 period. Examining this in detail, both specifications suggest a large change in the relationship between college costs and the likelihood of student employment. The reweighted specification predicts a much larger structural effect than the traditional KOB decomposition. In the traditional KOB decomposition, changes in the relationship between college costs and student employment predicted a 21.5 pp ($p<0.05$, Table 1.5), which was offset by intercept changes of negative 27.5 pp ($p<0.01$). When comparing 2008 employment rates to a re-weighted 2012 cohort, changes in the relationship between college costs and student employment predict a 143.2 pp change, offset by large changes in the intercept and marginally significant impacts in the role of demographic and enrollment patterns (Table 1.7). Although very different in magnitude, the large structural effects of intercept changes and college cost changes across both specifications suggest important changes to the relationship between college costs and student employment rates, as evidenced by changes in the coefficients of the linear regression model undergirding the KOB decomposition (Equation 1). In 2008, a 1% change in enrollment-adjusted costs of attendance predicted a 3.2 pp ($p<0.001$) decline in the likelihood of employment; by 2012, this relationship was smaller (0.7) and insignificant.[12] The changing magnitude and significance of the association between college costs and whether a student works suggests a number of hypotheses: perhaps student preferences for work have changed; college costs play a less important role in predicting employment decisions; and/or employers have changed their hiring preferences, breaking the relationship between college costs and employment, even for students who wish to work.

During the next sub-period, from 2012-2016, the reweighted decomposition again overpredicts the amount of change attributable to compositional and structural changes. This overprediction could stem from an inaccurate model specification and/or substantial changes in the student labor market. Inaccurate model specifications may include an improperly specified functional form and/or omitted variables. To test this, I run the same reweighted decomposition with various alternative specifications for the underlying model.[13] For example, I run the model with either a log-transformed or continuous age variable, in place of age categories. In addition, I run alternative specifications where I change the reweighting formula to allow for interactions between college costs and enrollment (e.g., students attending full-time or living on campus pay more for college on average); and financial dependency status and income. None of these alternative specifications correct for the off-setting between the estimated compositional and structural effects; the magnitudes of both effects are similar across specifications. To test for omitted variables, I examine how the re-weighted decomposition changes if I exclude students who only work work-study positions, in case there is an omitted variable that predicts work-

[12] Regression results available by request.
[13] Results available by request.

study positions[14]. Again, the model's estimates for structural and compositional effects off-set each other. Combined with the documented slow growth after the 2008 recession (Yagan, 2019), it is likely that the relationship between the determinants of student employment and the likelihood of working changed between 2012 and 2016 as the economy continued to recover. This hypothesis supports the large role of structural changes previously discussed for the larger 2004-2016 time period.

1.6.2 Implications of Survey Design on Employment Rate Results

Given the discrepancy in student employment rates between NPSAS data and the CPS and ACS, I further examine the 2004-2016 findings delineated above in two ways: (1) I examine how the decomposition results change when I treat the NPSAS survey responses as non-representative; and (2) I compare my survey-weighted and unweighted results with a similar decomposition using CPS data.

The discrepancy in student employment rates between the NPSAS data and other data sources raises the possibility that my subpopulation estimates of employment rates among undergraduates who attended only one institution in the NPSAS survey year are inaccurate. Treating the survey responses as non-representative relaxes the assumption that each student respondent represents students other than themselves. To do this, I run the decomposition analysis without analytic weights. Because this precludes using the bootstrap replicate weights, I calculate robust, clustered standard errors at the institution-level, the primary sampling unit (Cameron & Miller, 2015). These results show a slightly attenuated decline in student

[14] In AY2011-2012, NPSAS estimates suggest that 3.8%, 95% CI [3.5, 4.1] of undergraduate students held only a work-study job; 62.3%, 95% CI [61.6, 63.1] held a non-work-study job. In AY2015-16, 2.7%, 95% CI [2.5, 2.9] of undergraduate students held only a work-study job; 61.2%, 95% CI [60.6, 61.8] held a non-work-study job.

employment rates: in 2004, 74.1% ($p<0.001$) of the NPSAS survey sample reported working during the academic year, compared to 65.0% ($p<0.001$) of students in 2016. This 9.1 ($p<0.001$) decline in student employment rates is less than the estimated 13.8 pp ($p<0.001$) decline resulting from using the survey weights. The decomposition results, however, are substantively similar with and without a reweighted counterfactual. As before, structural changes explain most of the decline in student employment rates during this period (Table 1.8).

TABLE 1-8: COMPARISON OF NPSAS AND CPS DECOMPOSITION OF STUDENT EMPLOYMENT, 2004-2016

	Unweighted NPSAS Data		**CPS Oct. Supplement**	
	Trad. KOB	**Re-weighted Counterfactual**	**Trad. KOB**	**Re-weighted Counterfactual**
Likelihood of Working (%, 2016)	0.650***		0.507***	
	(0.003)		(0.008)	
Likelihood of Working (%, 2004)	0.741***		0.569***	
	(0.003)		(0.007)	
Simple Difference (2016-2004)	-0.091***		-0.061***	
	(0.004)		(0.011)	
Likelihood of Working (%, Counterfactual 2016)		0.651***		0.505***
		(0.005)		(0.010)
Total Compositional Effect	-0.008*	-0.001	-0.004	0.003
	(0.003)	(0.004)	(0.008)	(0.009)
Total Structural Effect	-0.084***	-0.090***	-0.057***	-0.064***
	(0.004)	(0.006)	(0.013)	(0.013)
Sample Size, 2004	65,960		5,985	
Sample Size, 2016	71,500		5,592	

Notes: ~ $p<0.1$; * $p<0.05$; ** $p<0.01$; *** $p<0.001$. Across both data sources, financial variables are adjusted for inflation to 2016 dollars using the CPI. The NPSAS data includes all NPSAS survey respondents with complete demographic information. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200). The IPUMS-CPS data includes all CPS respondents to the October Supplement, who reported being enrolled in an undergraduate program that month (Flood *et al.*, 2020). CPS estimates are are weighted using IPUMS-provided analytic weight EDSUPPWT.

A similar decomposition using the CPS data echoes this pattern (Table 1.8). Appendix B describes the sample and alternative determinants used in this model. There are slight differences because the CPS and NPSAS surveys collect different types of information. Between 2004 and 2016, CPS-estimated student employment rates among undergraduate students dropped 6.1 pp ($p<0.001$), from 56.9 pp to 50.7 pp. As with the NPSAS decompositions, most of the decline in student employment rates during this period are attributable to structural changes.

Combined, these estimates suggest that how the determinants of student employment (e.g., student demographics, college costs, enrollment patterns, and local unemployment) relate to students' likelihood of employment has changed over time. Both the traditional and reweighted KOB decomposition estimates identify that local unemployment rates play an important role in explaining the recent patterns in student unemployment rates. I also find evidence that college costs become less predictive of whether a student works over time. In addition, the consistency of results across the full 2004-2016 period and 2008-2012 subperiod suggests that a linear probability model can consistently predict student employment rates under certain conditions. The difference in traditional and reweighted decomposition results for the 2004-2008 and 2012-2016 subperiods highlight a particular need to understand what makes these time periods unique. Both the 2004-2008 and 2008-2012 subperiods were preceded by recessions. As the economy grew in both periods, economic growth in the broader labor market may not translate equally to growth in the student labor market. This would be consistent with the interpretation that structural changes – in how the student labor market relates to the broader economy – account for nearly all of the decline in student employment rates between 2004-2016. Under certain economic conditions, students may be unable to find jobs, even when they need them. Portraying student employment as an accessible financial resource may thus oversimplify the financial constraints facing students. Future research should further examine students' job search processes. Investigating how students find jobs, and how students respond to limited employment opportunities, will help elucidate the ways student employment reifies other social, economic, and educational inequities, particularly in regards to college affordability.

1.6.3 Changes in Average Hours Worked Per Week

Students were not only less likely to work between 2004 and 2016; those who worked, worked fewer hours in 2016 than in 2004. This decline is less dramatic than the change in overall employment: between 2004 and 2016, the average hours worked per week fell by one hour ($p<0.001$) from 29.33 hours per week to 28.35 hours. Table 1.9 shows how the overall decline in work intensity masks a changing trend in the average amount of time students spent working. In 2008, student workers reported working 18 minutes more per week (0.30 hours, $p<0.05$) than students workers in 2004. Between 2008 and 2012, this trend reversed, with students working 47 minutes less per week (-0.79 hours, $p<0.001$) in 2012 than in 2008. The decline in hours worked continued between 2012 and 2016, with student workers reporting working 30 minutes less on average in 2016 than in 2012 (-0.50 hours, $p<0.001$).

Results from the traditional KOB decomposition suggest that between 2004 and 2016, approximately one third (-0.30, $p<0.05$) of the decline in hours worked is attributable to measurable changes in the determinants of work. Across the other subperiods, the traditional KOB model is unable to precisely estimate compositional and structural effects. As with the decomposition of student employment rates, I examine whether and how the decomposition estimates change when I reweight the distribution of student employment in the later year to better match the earlier year (Table 1.9).

Under the reweighted model, there is little change to the structural and compositional results. For the 2004-2016 period, total compositional effects explain more than half (-0.56, p<0.01) of the decline in hours worked per week for the overall time period. In the subperiods, the reweighted decomposition estimates remain imprecise. Nevertheless, model fit statistics for the reweighted decomposition suggest that the reweighting factor is inconsistently estimated.[15] Before placing too much weight on these decompositions of hours worked, I thus first examine the decompositions at the 25th and 75th percentile of the distribution.

TABLE 1-9: TRADITIONAL & REWEIGHTED KOB DECOMPOSITION OF AVERAGE HOURS WORKED PER WEEK

	2004-2016	**2004-2008**	**2008-2012**	**2012-2016**
Average Hours Worked Per Week				
Hours Worked (Year 2)	28.35***	29.64***	28.85***	28.35***
	(0.10)	(0.10)	(0.11)	(0.10)
Hours Worked (Year 1)	29.33***	29.33***	29.64***	28.85***
	(0.11)	(0.11)	(0.10)	(0.11)
Simple Difference (Year 2 – Year 1)	-0.98***	0.30*	-0.79***	-0.50***
	(0.14)	(0.15)	(0.14)	(0.15)
Traditional KOB Decomposition				
Total Compositional Effect	-0.30*	0.11	-0.36	-0.3
	(0.12)	(0.13)	(0.22)	(0.28)
Total Structural Effect	-0.68***	0.19	-0.42~	-0.2
	(0.14)	(0.14)	(0.24)	(0.28)
Reweighted KOB Decomposition				
Hours Worked (Counterfactual Year 2)	28.91***	29.96***	28.27***	29.74***
	(0.19)	(0.88)	(0.82)	(0.60)
Total Compositional Effect	-0.56**	-0.32	0.58	-1.39
	(0.18)	(0.90)	(0.83)	(0.61)
Total Structural Effect	-0.43*	0.63	-1.37	0.89
	(0.18)	(0.88)	(0.83)	(0.61)
Sample Size, Year 1	48,900	48,900	71,490	46,910
Sample Size, Year 2	46,470	71,490	46,910	46,470

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information who reported working any job (including work study) during the academic year. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200). The counterfactual estimate of hours worked is generated by reweighting Year 2 records by their similarity to Year 1 using a logit model similar to Equation (1) in the text.

[15] Model fit statistics also suggest poor fit when I use a continuous version of age and when I use an interacted reweighting factor. Only the estimates excluding work study students provided consistent estimates; in that case, the entire decline in hours worked is attributed to compositional effects (results available upon request).

TABLE 1-10: CHANGES & DECOMPOSITIONS FOR PART-TIME & FULL-TIME STUDENT WORKERS, 2004-2016

	25th Percentile (Part-Time Workers)				75th Percentile (Full-Time Workers)			
	2004-2016	**2004-2008**	**2008-2012**	**2012-2016**	**2004-2016**	**2004-2008**	**2008-2012**	**2012-2016**
Hours Worked Per Week								
Hours Worked (Year 2)	19.11***	20.30***	20.34***	19.11***	41.21***	42.01***	41.44***	41.21***
	(0.56)	(0.10)	(0.11)	(0.56)	(0.06)	(0.06)	(0.06)	(0.06)
Hours Worked (Year 1)	20.60***	20.60***	20.31***	20.34***	41.85***	41.83***	42.07***	41.41***
	(0.11)	(0.11)	(0.10)	(0.11)	(0.05)	(0.05)	(0.06)	(0.05)
Simple Difference (Year 2 – Year 1)	-1.49**	-0.29*	0.03	-1.22*	-0.64***	0.18*	-0.63***	-0.20**
	(0.57)	(0.14)	(0.14)	(0.58)	(0.07)	(0.07)	(0.08)	(0.08)
Traditional KOB Decomposition								
Total Compositional Effect	-0.27*	0.11	-0.43~	-0.43	-0.19*	0.03	-0.12	-0.03
	(0.13)	(0.13)	(0.26)	(0.32)	(0.08)	(0.10)	(0.16)	(0.19)
Total Structural Effect	-1.22*	-0.40*	0.46~	-0.79	-0.45***	0.15~	-0.50**	-0.18
	(0.58)	(0.17)	(0.26)	(0.65)	(0.08)	(0.09)	(0.16)	(0.19)
Reweighted KOB Decomposition								
Hours Worked (Counterfactual Year 2)	20.66***	20.25***	18.04***	21.64***	41.44***	41.66***	41.34***	41.64***
	(0.36)	(1.55)	(1.93)	(0.80)	(0.10)	(0.38)	(0.57)	(0.22)
Total Compositional Effect	-1.55**	0.05	2.31	-2.53**	-0.23*	0.35	0.10	-0.43~
	(0.54)	(1.56)	(1.91)	(0.94)	(0.09)	(0.38)	(0.58)	(0.23)
Total Structural Effect	0.06	-0.34	-2.27	1.30	-0.41***	-0.17	-0.73	0.23
	(0.37)	(1.55)	(1.93)	(0.82)	(0.09)	(0.38)	(0.57)	(0.23)
Sample Size, Year 1	48,900	48,900	71,490	46,910	48,900	48,900	71,490	46,910
Sample Size, Year 2	46,470	71,490	46,910	46,470	46,470	71,490	46,910	46,470

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

1.6.4 Distributional Changes: Part-time & Full-time Work

Table 1.10 presents changes at the 25th and 75th percentile of hours worked. Distributional analyses are especially important if demographic characteristics, enrollment rates, college costs, and unemployment rates relate to hours worked differently at the 25th percentile than at the 75th. At the 25th percentile, student workers reported working approximately 20 hours a week across all the NPSAS surveys; I refer to these workers as part-time workers. At the 75th percentile, student workers worked approximately 40 hours a week; I refer to these workers as full-time workers. Importantly, there are not large shifts in these distributional statistics. However, at each quantile, students do report working slightly fewer hours in 2016 than in 2004. Part-time workers, for example, reported working -1.49 hours ($p<0.01$) less in 2016 than 2004, suggesting a rise in the relative number of student workers who work less than 20 hours a week. Full-time workers also reported working less (-0.64 hours, $p<0.001$) in 2016 than 2004, although this decline is smaller. When the decline occurred also varies. Among part-time workers, nearly all of the decline in hours worked per week occurred between 2012 and 2016. Full-time student workers, on the other hand, experienced the largest decline in hours worked between 2008 and 2012.

The distributional decomposition also shows how the relationship between work intensity and students' demographics, college costs, local unemployment rates, and enrollment patterns may vary across different types of student workers. In particular, the decomposition models appear more suited to estimating changes among full-time workers rather than part-time workers. This might explain the model's poor fit with average hours worked.

For example, among part-time workers, the traditional RIF-KOB decomposition model and reweighted RIF-KOB decomposition model present disparate results. Between 2004 and

2016, the traditional RIF-KOB model estimates that a majority of the decline in hours worked among part-time workers can be attributed to structural effects. The reweighted RIF-KOB, on the other hand, suggests that a majority of the decline is attributable to compositional effects. Across the subperiods, the traditional RIF-KOB decomposition finds significant structural impacts for both part-time and full-time workers. The reweighted decomposition, however, either identifies insignificant compositional and structural effects or overpredicts the impact of compositional changes across the subperiods (Table 1.10). The disagreement across the traditional and reweighted decomposition models for the overall 2004 to 2016 time period and four sub-periods suggests that the linear model predicting weekly hours worked may be inappropriate for this part of the distribution. Even across different specifications of the reweighted decomposition, model diagnostics suggest potential model misspecification in equation (1) or the reweighting function (Appendix Table 1.D).

Among full-time student workers, the traditional and reweighted decomposition results are more consistent, particularly for the 2004 to 2016 time period. The right panel of Table 1.10 presents these results for the traditional RIF-KOB and reweighted decompositions. In both specifications, changes to observables account for a third (-0.19 hours, $p<0.05$; -0.23, $p<0.05$) of the 38 minute decline in hours worked between 2004 and 2016. This is similar across different alternative specifications of the reweighted decomposition (Appendix Table 1.E). Across both models, changes to student demographics and college costs are significant explanatory factors for the observed decline in hours worked (Table 1.11). Under the traditional specification, enrollment changes also play a role; however, the significance of these determinants attenuate under the reweighted decomposition. Both the traditional and reweighted decompositions attribute over 50% of the change in hours worked to structural factors. The only structural determinants significant across both specifications was whether a student enrolled in-state or lived on-campus.

TABLE 1-11: DETAILED DECOMPOSITIONS FOR FULL-TIME STUDENT WORKERS

	Traditional	Reweighted
Compositional Effects		
Total Compositional Effect	-0.19*	-0.23*
	(0.08)	(0.09)
Demographic Changes	-0.25***	-0.26***
	(0.05)	(0.07)
Financial Changes	-0.09*	-0.14*
	(0.03)	(0.06)
Unemployment Rate	0.03	0.06
	(0.04)	(0.07)
Enrollment Changes	0.11***	0.06
	(0.02)	(0.07)
Structural Effects		
Total Structural Effect	-0.45***	-0.41***
	(0.08)	(0.09)
Demographic Changes	0.33	-0.10
	(0.54)	(1.20)
Financial Changes	0.96	3.05
	(1.70)	(2.48)
Unemployment Rate	0.16	0.71
	(0.43)	(0.58)
Enrollment Changes	0.50	0.33
	(0.35)	(0.67)
Intercept Changes	-2.40	-4.52
	(1.79)	(2.79)
Sample Size, 2004	48,900	
Sample Size, 2016	46,470	

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

Although identifying an appropriate model to estimate hours worked for part-time workers is needed, the reweighted KOB decompositions do shed some light on changes to weekly hours worked among students working full-time (~40 hours per week). For students at this part of the distribution, changes to student demographic characteristics and college costs were clear contributors to the decline. Notably, these results are similar to the results for the decomposition of average hours worked, in spite of the misspecification. At both the mean and 75th percentile, declining shares of first-generation college students, falling incomes, and rising costs of attendance partly explain the decline in hours worked. These are notable findings because the relationship between college costs and student employment is opposite of what might be expected given other research documenting the ways student employment relieves students' credit constraints (Scott-Clayton, 2012; Goldrick-Rab, 2016). Instead, these results suggest that students attending more expensive colleges tend to work fewer hours, even after controlling for student enrollment patterns and demographic characteristics.

The structural results are also worth noting: in both the mean and 75th percentile decompositions, whether a student enrolled in-state also appeared to play a role, although aggregate enrollment effects were insignificant. In 2004, in-state students worked, on average, four hours more per week compared to their out-of-state peers; by 2016, in-state and out-of-state students worked the same number of hours. The relationship between a student's work load and their state residency thus changed over the twelve year period. Combined, these findings raise additional questions about the relationship that students have with their employers. The change in the relationship between in-state status and work intensity, for example, may be capturing other structural changes not represented in the model, such as changes to employer staffing schedules. Below I discuss this potential concern along with other limitations of these decomposition methods.

1.7 Limitations & Considerations

In this section, I describe some of the key assumptions and limitations of the KOB-based decomposition methods used in this paper. In particular, the accounting of compositional and structural changes is limited by the regression models used in the decomposition. One classic limitation is omitted variable bias. For example, KOB decompositions assume that the undergraduate population in one year is comparable to another year, conditional on the covariates in the model. If unobserved factors change from one period to the next, even after controlling for covariates, then the structural effects attributed to various factors will be biased. I tested for this potential bias by using re-weighted counterfactuals across each estimation (see Appendix C for details). In addition, I also examined how my estimates change across different specifications of the foundational OLS model (see Equation 1).

Through the specification checks, I found that omitted variable bias and model misspecification is an unlikely concern for the decomposition of student employment rates over time. If it were, I would expect the decomposition to overpredict compositional and structural effects for both the overall time period and the subperiods. This is not the case. Because the decomposition of student employment rates in the overall 2004-2016 period and 2008-2012 subperiod are well estimated, it is more likely that macroeconomic forces influence student employment rates in important ways, in both the cross-section and longitudinally.

The presence of omitted variables is more likely in the decomposition of hours worked, particularly among average and part-time workers. This is evidenced by the inconsistency in the reweighting factor for the decomposition of average hours worked and the differences in results across reweighted and traditional KOB decomposition results for part-time workers. The student profile of part-time workers has likely changed over time, beyond the covariates included in the model. A changing student profile is especially likely given the large decline in student employment rates. Future research should investigate the role of other covariates to better understand who works part-time while going to school, and how that varies from students who work full-time.

KOB-based decompositions are also limited in their ability to test specific mechanisms. Rather, the results are limited to identifying potential hypotheses for future study. For example, the change in the relationship between in-state students and weekly hours worked mechanically explains some of the decline in average hours worked. Substantively, however, the result may reflect broader economic conditions given that the 2008 recession occurred in the middle of my analytic time period. The effects of this recession were long lasting, with many workers working fewer hours than desired and young, less-experienced workers experiencing larger negative impacts on their wages and employment rates (Kalleberg & von Wachter, 2017). If employers employing in-state students were disproportionately impacted by the recession, compared to those employing out-of-state students, this might explain some of observed structural effects. Thus, although the structural effect suggests that the relationship between hours worked and state residency changed between 2004 and 2016, the method itself is unable to identify definitively the source of the relationship change. Rather, the result only points to hypotheses for future research.

1.8 Implications & Conclusion

In this paper, I examined changes to student employment over the last two decades. Using nationally representative data, I find that the share of students who worked fell between

2004 and 2016. Weekly hours worked have also fallen during this time period. Given that student employment may help students pay for college, and college costs increased during this time period, the declines in student employment rates and intensity seem at odds with these trends. I utilized predictors of student employment, as identified by economists, demographers, and sociologists in past research, to untangle potential hypotheses that might explain these trends in both student employment rates and hours worked.

I find evidence that although the number of students enrolling in college has increased, student employment has declined and the relationship between local unemployment rates and student employment is stronger now than in the past. Understanding how macroeconomic forces affect students' employment preferences for working, and access to jobs, is an important area for future research. Additionally, I find the relationship between college costs and whether a student works has changed over the last two decades; costs of attendance is no longer a significant predictor of whether students work. Future research should examine why this is the case. Combined, these findings suggest that students may be working less because they are having trouble finding jobs.

The decomposition rules out large roles for demographic and enrollment changes – declining employment is thus not attributable to measurable changes in who enrolls in college and where students are enrolling. Because these results suggest that student employment may no longer offset college costs in the same way historically conceptualized by financial aid practitioners and researchers, educational policymakers and practitioners should reconsider whether student employment is a viable financial resource for educational costs. In particular, this highlights a particular need to investigate additional research questions, such as: How do students find a job and/or decide to work? If students are unable to find a job, what do they do - what other financial products are these students using to pay for college?

In addition to the results on the employment margin, I find that the amount of hours students work per week has also declined. I find evidence that changes to student demographic characteristics and college costs play a significant role in explaining the declines in hours worked, particularly for the full-time worker. In addition, I find evidence that the same decomposition model does not apply to both full-time and part-time student workers. This reflects longer-term trends specific to the labor market for part-time jobs (Kalleberg, 2011). This finding may also complements prior work conceptualizing different student-worker identities, where some individuals view themselves as workers going to school, while others identify themselves as students who work (Perna, 2010). Just as others have suggested tailoring student services to different student-worker profiles, conducting additional analyses of how these trends – and determinants – vary across student worker subpopulations will be important for future research (Perna, 2010). These findings also support financial aid eligibility requirements that recognize the spectrum of student employment and enrollment intensity. For example, aid programs that serve both full- and part-time students may help support those working full- and part-time in important ways.

A third set of findings from this paper suggests that the basic linear model's applicability varies across subperiods. Variation over time in the relationship between various factors and student employment rates and hours worked suggests another area for future research. Because the analytical subperiods overlap with periods before, during, and after the 2008 recession, these findings raise additional questions regarding how broader economic conditions impact the relationship between college costs and the likelihood of employment. These questions include, how do students' job-seeking and work behaviors change in times of economic downturns? How do these behaviors change during periods of growth? How does the relationship between student employment, financial aid, and college cost vary in times of economic growth? The answers to these questions – and their intersection with student enrollment and borrowing decisions – can help inform policy and program development at the institution and system levels by elucidating whether and how governments should consider funding models for student financial aid and higher education that are countercyclical to broader economic trends.

2 The Impact of Fluctuating Federal Work Study Dollars in Kentucky

2.1 Introduction

Each year, the U.S. government spends approximately $150 billions of dollars on student financial aid (Ma, Pender & Libassi, 2020). These dollars go towards grant aid, loan aid, tax benefits, and work study programs to support students enrolled in postsecondary education. Approximately one billion dollars are expended through the Federal Work Study (FWS) program. As a campus-based, need-based aid program, FWS dollars are distributed to institutions, which in turn use the funds to subsidize work study positions for students. Much research has shown that other financial aid programs positively impact student enrollment, persistence, and degree completion across contexts (Page & Scott-Clayton, 2016). Less is understood, however, about the impact of work study dollars.

This paper utilizes a difference-in-differences framework to estimate how federal funding changes to the Federal Work Study (FWS) program affect students' academic and financial outcomes. I use student-level data from the state of Kentucky to compare FWS eligible students and ineligible students across institutions that experienced different changes to their federal work study budgets between AY2011-11 to AY2014-15. Past evaluations of the program in West Virginia and Ohio also used a difference-in-differences framework (Scott-Clayton, 2012; Soliz & Long, 2016). These earlier studies leveraged FWS funding differences across institutions for undergraduate students enrolled in the early to mid-2000s. For those students, FWS had small, positive impacts on credit completion and suggestive negative effects on student grades (Scott-Clayton, 2012; Soliz & Long, 2016). This paper builds on these two evaluations of the FWS program by evaluating how funding changes within the same institution affects student outcomes. It helps to answer the question, “how are students affected by institution-level changes in work study resources?”

My results show that the answer is complicated; changes to FWS funding levels can influence both institutional and student behavior. First, changing the amount of FWS funds available to institutions affected how institutions awarded these funds to students through financial aid offers. I find evidence that impacts from a decrease in funding are not equal and opposite to a positive change in funding. This suggests that institutions may respond differently to funding gains than to funding losses. Second, I find little impact of increased FWS funds on student academic outcomes. For example, I find imprecise and small impacts on credit accumulation and first-to-second year persistence, consistent with prior research that estimates the effect of FWS participation on first-year credit completion in West Virginia and Ohio (Scott-Clayton, 2011; Soliz & Long, 2016). However, unlike these earlier evaluations, I find suggestive evidence that increases in FWS funding may positively impact students’ grades. Finally, this paper rules out other financial aid sources as a potential mechanism for these results. Increasing federal work study budgets had no significant impact on other grant and loan aid awarded by the institution.

Before discussing these results and implications in more detail, I provide additional detail on the FWS program, Kentucky public four-year institutions, the data, and the identification strategy below.

2.2 Background

2.2.1 The Federal Work Study Program

This paper estimates the impact of changes in institutional funding provided through the FWS program, a federally-funded student aid program. The FWS program subsidizes student employment through a campus-based allocation formula. Institutions use these funds to offset up to 75% of wages earned through a student work-study position. FWS jobs are typically on-campus employment opportunities, although some off-campus employers are also eligible for the subsidy (Scott-Clayton, 2011). The program thus supports institutions in providing work study opportunities to students.

FWS dollars are awarded to students through federal and institutional financial aid processes. In other words, students must first apply for federal financial aid. Then, institutions package a federal work study award into students' award letters based on the student's financial need and the institution's FWS budget. Unlike grant and loan aid, however, a student does not immediately receive the FWS dollars offered in their aid award letter. Rather, the student must find a job and earn up to the amount listed in the award through their wages. Wages in FWS jobs must meet federal, state, and local minimum wage requirements; in Kentucky, FWS students earned at minimum $7.25 per hour during the time period covered in this analysis. Students' earnings are distributed like a normal paycheck. The FWS award in a students' award letter is thus the maximum federally-subsidized earnings a student can earn in a FWS position. Some students earn more than the amount listed under the work study award if the institution (or employer) has the funds to pay the student's full wages once their FWS award is exhausted. The opportunity to earn more than the FWS award amount, however, is not guaranteed and often varies across employers/departments, even within the same institution. These characteristics of the FWS program help explain why some students who are offered FWS aid do not take it (e.g. students may be unable to find a FWS eligible position, or they may choose to take a higher paying position elsewhere). In 2012, approximately 5% of undergraduates held a FWS position compared to 62% of undergraduates who worked at a non-work study position. Overall 66% of undergraduates worked at either a work-study or non-work study position (NCES, 2013.).

Recent quasi-experimental evaluations of the FWS program suggest mixed impacts on student outcomes. Two studies, using a difference-in-differences design, find mixed impacts on credit accumulation and student grades (GPAs). In Ohio, Soliz and Long (2016) find that FWS funds increased credit accumulation among first-time, full-time students and may decrease student grades, depending on the specification. In West Virginia, the program had a negative impact on first-year student grades and no significant impacts on credit accumulation (Scott-Clayton, 2011). In a third study, Scott-Clayton and Minaya (2016) analyze national data from the 1995-1996 and 2003-2004 administrations of the Beginning Postsecondary Student Study (BPS). The authors demonstrate how the counterfactual comparison group may matter when estimating FWS impacts using propensity score methods. Specifically, the authors find that FWS's positive academic effects were concentrated among students who otherwise would have worked another job in the absence of FWS. When compared against non-working students, the FWS program had a negative effect on student grades and no impact on four-year degree completion (Scott-Clayton & Minaya, 2016). Scott-Clayton and Minaya (2016) do not report estimates on credit completion. The findings in Scott-Clayton and Minaya (2016), however, echoes the negative impact on student grades in Scott-Clayton (2011) and the suggestive negative impact on grades in Soliz and Long (2016). Combined, these results suggest that FWS may increase credit completion and decrease grades, depending on the context of the evaluation. In other words, estimated work study impacts likely depend on whether students participate in outside employment.

This paper complements these prior studies by examining the impact of the FWS program in Kentucky. Similar to Scott-Clayton (2011) and Soliz and Long (2016), I use a difference-in-differences strategy. Rather than focusing on the differences in total FWS funds across institutions, however, I leverage annual changes in program funding. This is necessary because public four-year institutions in Kentucky vary in their financial aid practices. Specifically, Kentucky public four-year institutions which have consistently higher amounts of FWS funding also award much higher forms of student grant aid. In this case, isolating the impact of FWS dollars by comparing students across institutions with systematically different aid practices is difficult.[16] Focusing on within-institution funding changes thus isolates the comparison to students attending the same institution. An additional benefit of this approach is that it accounts for how institutions respond to changes to funding levels. Other research in higher education has documented the importance of funding, which I document below.

2.2.2 Funding Matters

Government funding to institutions can play an important role in supporting student success. Bound and Turner (2007), for example, suggest that limited public resources prevent institutions from meeting increased student demand, resulting in lower degree attainment rates. Deming and Walters (2017) build on this by causally connecting institutional spending with

[16] In practice, student financial aid award letters contain grant aid awards and FWS offers. An unbiased estimate of FWS impacts, under the quasi-experimental framework, would require that students who are in the "treated" group receive grant aid awards that are indistinguishable from the "untreated" comparison group. This is because grant aid positively impacts student persistence and completion (see Page & Scott-Clayton, 2016 for a review); if "treated" students were also more likely to receive grant aid awards and/or likely to receive more grant aid dollars than the comparison group, then the FWS estimates would be biased upwards.

degree attainment. Chakrabarti, Gorton, and Lovenheim (2019) also use a causal identification strategy to demonstrate how state higher education appropriations affect students' degree attainment and longer term outcomes such as debt burden and home ownership.

Funding to institutions is not the only type of funding that matters. Student financial aid can also boost student success rates. Grant aid supports increased student enrollment, persistence and degree attainment across multiple settings and research designs (Nguyen, Kramer, & Evans, 2019; Page & Scott-Clayton, 2016). Randomized control trials and quasi-experimental studies of student borrowing also demonstrate that student loans can increase student credit accumulation and degree attainment when used to relieve credit constraints (Marx & Turner, 2019; Wiederspan, 2016).

Funding for FWS programs thus may support student success as both an institutional resource and source of student financial aid. As an institutional resource, increasing FWS dollars could encourage institutions to spend additional dollars on students through the creation or extension of work study-funded positions. This in turn could increase opportunities for campus engagement (Perna, 2010). As a form of student financial aid, FWS funds may play an important role in relieving students' financial constraints through work. Given the positive impacts of grant and loan aid in other settings, understanding how FWS awards compare to and interact with other sources of financial aid may help inform institutional aid packaging practices. Below, I present preliminary evidence that institutions respond to funding increases by distributing additional aid to students, while minimizing the impact of funding cuts. I find little evidence that institutions substitute FWS dollars with grant and loan aid in student aid packages. Prior to presenting results, I discuss the context and data for this study. I also present my estimation strategy, which exploits exogenous sources of variation in both government FWS funding formulas and institutional aid packaging practices.

2.3 Study Context and Data

This paper utilizes data on undergraduate students attending any Kentucky public four-year institution between AY2010-11 and AY2014-15. Public four-year institutions are spread throughout the state. These institutions include research universities (e.g. University of Kentucky and University of Louisville), Master's-level institutions, and a Baccalaureate-level college (Kentucky State University). Each institution participated in the FWS program for all the years included in this study.

2.3.1 Variation in FWS availability across institutions and years

The U.S. government annually distributes institutional FWS dollars using an allocation formula. The formula has two parts: one, a base-guarantee based on historical funding levels and participation, and two, a fair-share formula used to allocate funds based on the average financial need of an institution's student population (Fountain, 2017). About two-thirds of program funds are allocated to higher education institutions through a base-guarantee (Fountain, 2017; Smole, 2005).[17] The remainder of the funds are allocated through a fair-share formula that defines institutional need for additional FWS dollars as a function of total cost of attendance and the average expected family contribution among FWS eligible students attending the institution (Smole, 2005). In addition, the U.S. Secretary of Education has the discretion to redistribute up to 10% of funds in excess of $700 million to institutions that graduate or transfer more than half

[17] In some states, federal financial aid is allocated to a higher education system office, for distribution within member institutions. In Kentucky, individual institutions have agreements with the Federal Student Aid office to participate in the FWS program. FWS funds are therefore allocated directly to the institution in this context.

of their Pell Grant recipients (Fountain, 2018). Summarizing the overall distribution pattern, critics note that the formula favors institutions with longer participation in the program and institutions with larger costs of attendance, rather than the level of financial need of the institution's current students (Soliz & Long, 2016; Fountain, 2017).

From a research perspective, the various elements of the formula introduce potential sources of exogenous variation in the total amount of FWS dollars allocated to an institution from year to year. First, total program dollars are dependent on Congressional appropriations; if insufficient funds are appropriated, institutional FWS awards are proportionately reduced based on their base guarantee (Fountain, 2018). Congressional appropriations are part of a federal budget process that only just begins in the spring (American Council on Education, n.d.), i.e., when financial aid offers are sent to students. Second, the fair-share formula is a proportional distribution formula; the additional funds an institution receives on top of their base guarantee is a function of how their institutional need measures against other institutions across the country. Because individual institutions have little control over how other institutions set tuition and fee rates and enroll low-income students, an institution is likely unable to fully manipulate their fair-share portion of the FWS allocation from year to year[18]. Additionally, institutions are allowed to carry forward and backward up to 10% of their FWS allocation from year to year (Fountain, 2018). The carry-forward, carry-back provision could potentially mitigate against the need to manipulate any given year's allocation.

[18] In Kentucky, tuition and fees are set by the Kentucky Council on Postsecondary Education (Kentucky Revised Statutes Chapter 164, 1997). Even if individual public four-year institutions may influence their tuition rate through the Kentucky Council on Postsecondary Education, these institutions have little authority over the tuition rates of other institutions, especially those outside the state.

In any year during the analytical timeframe (AY2011-AY2015), up to four of eight Kentucky public four-year institutions saw no annual change in their FWS allocation. Two institutions never experienced a change to their FWS allocation across the entire five year period. Four institutions experienced a change to their FWS allocation every year between AY2011 and AY2015 (Table 2.1). These changes included increases and decreases from the prior year. At times, the funding changes offset prior changes. For example, between AY2012 and AY2013 Kentucky State University experienced an decline of $80,000; re-gained the $80,000 in AY2014; then lost the $80,000 again in AY2015. This pattern of funding fluctuations seems consistent with the base- and fair-share allocation formulas described previously. Overall, annual changes in funding ranged from -30% to 18% of prior year funding (Figure 2.1). When scaled by an institution's number of FWS participants in a given year, this was equivalent to a -$1,057 to $500 change per student.

TABLE 2-1: SUMMARY OF FWS PROGRAM AT KENTUCKY PUBLIC 4-YEAR INSTITUTIONS, AY2011-15

	FWS Allocation ($)	**Average Change in FWS Allocation ($)**	**Number of FWS Recipients**	**12-month Undergraduate Enrollment**	**Number of Years with Change in FWS Allocation**
Eastern Kentucky University	863,729.00	0.00	576	16,169	0
Kentucky State University	502,523.22	-851.71	241	3,013	3
Morehead State University	945,221.24	70.85	538	10,502	5
Murray State University	470,124.00	0.00	359	10,015	0
University of Kentucky	1,170,690.15	-79,846.73	544	22,350	5
University of Louisville	1,068,760.75	-78,454.55	441	18,267	5
Western Kentucky University	739,962.00	-7,850.39	835	20,865	1
Northern Kentucky University	422,888.26	-28,439.38	257	14,829	5

Source: U.S. Department of Education.

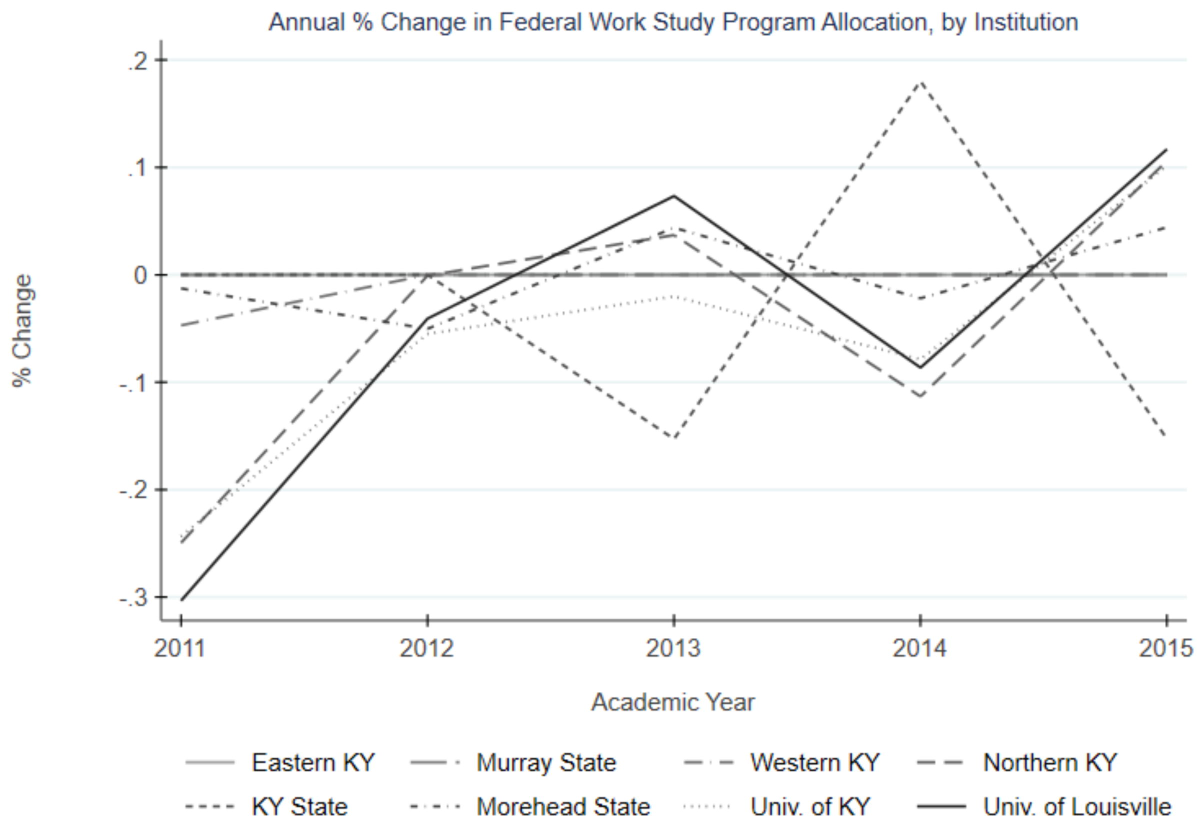

FIGURE 2.1: ANNUAL PERCENT CHANGE IN FWS APPROPRIATIONS BY INSTITUTION AND YEAR

Notes: This figure shows the percent change in FWS appropriations relative to the previous year for Kentucky public four-year institutions. In 2011, most institutions experienced a decline from the previous year as stimulus funding from the 2009 American Recovery and Reinvestment Act (ARRA) expired.

2.3.2 Variation in FWS eligibility

In addition to institution-level variation in FWS funding, students also vary in the amount of FWS dollars offered in their financial aid packages. Institutional financial aid administrators determine whether and how much FWS dollars are offered to each student.[19] Nationally,

[19] There are no statutory requirements for award size, although students must demonstrate financial need; e.g. their EFC must be less than the institution's cost of attendance.

approximately 5% of undergraduates received a FWS award; these students earned $2,213 through the program in AY2011-12 (Fountain, 2017). In this sample, 3.0% of students received a FWS award offer; recipients on average were awarded $1,635. Given the small numbers of students receiving a FWS offer and the funding idiosyncrasies that create institutional variation in available funds, it is unlikely that students are able to influence their likelihood of receiving FWS funds through institutional choice.

Furthermore, it appears that institutions often set a threshold for FWS eligibility based on a student's EFC. In West Virginia, for example, the vast majority of FWS students attending a public postsecondary institution had EFCs below $7,000 (Scott-Clayton, 2011); in Ohio, this threshold was $10,000 (Soliz & Long, 2016). In this sample of Kentucky undergraduates, 91.6% (1,300 of 1,420) of FWS awardees had an EFC below $10,000. Because a student's EFC is calculated through one of six formulas based on prior year tax information and subject to federal verification, the EFC is often thought of as a hard-to-manipulate measure of student financial need. Researchers have exploited the EFC as a proxy for random assignment in both regression discontinuity designs (e.g. Castleman & Long, 2016; Park & Scott-Clayton, 2019) and difference-in-differences analyses (e.g. Scott-Clayton, 2011; Soliz & Long, 2016). For the purposes of analyzing the impact of FWS funding changes in this context, I thus treat the $10,000 EFC threshold as a *de facto* eligibility threshold for FWS eligibility.

2.3.3 Data

I use student-level data provided by the Kentucky Center for Statistics, a state agency that collects administrative records from Kentucky college and universities for research purposes. These data include the college enrollment, degree conferral, and demographic information for students attending a public postsecondary institution between AY2009-AY2018. I focus on undergraduate students enrolled during AY2011-AY2015 who attended a public four-year institution in Kentucky (n=223,003) in order to observe degree completion within four academic years.

I match these enrollment records to students' financial aid awards, also provided by the Kentucky Center for Statistics. The financial aid awards include information on students' expected family contribution, financial dependency status, and financial aid awards offered to the student from an institution in a given year. In this analysis, I focus on students with observed financial aid awards because the FWS program is a student financial aid program; students who did not apply for financial aid may be very different from the students who applied for, and received, a FWS offer.

I further restrict my sample to students with first-year status, did not have a prior postsecondary enrollment record in AY2009 or AY2010, and graduated high school within two years of their first enrollment record. I focus on first-time students to account for the censoring of student records prior to AY2011 and eliminate the possibility of prior year financial aid awards impacting students in the current year. This restriction also allows for better comparison with prior FWS evaluations (e.g. Scott-Clayton, 2011; Soliz & Long, 2016), which also focused on a traditional-age, first-time undergraduate population. I focus on students that enrolled at only one institution within the same academic year, thus excluding students who were concurrently enrolled and students who transferred within the same year. This exclusion is necessary because these students often received financial aid awards across different institutions. I am unable to differentiate how multiple financial aid awards interacted with each other across institutions. I also focus on students with complete demographic information to control for well-documented heterogeneity in students' college experiences across dimensions of race, gender, parental education, and financial background (Mayhew, et al., 2016). The final analytic sample consists of 45,281 undergraduate students who are younger and more racially and financially diverse than the overall Kentucky undergraduate population (Table 2.2). For example, 97% of students in the analytic sample were considered financial dependents for the purposes of financial aid, and the average expected family contribution was $14,531 compared to $10,742 in the overall sample. A much higher share of students in the analytic sample, however, were offered a Pell Grant (42% vs. 29%) and a student loan (69% vs. 43%) compared to the overall set of student records.

TABLE 2-2: DESCRIPTIVE STATISTICS OF KENTUCKY UNDERGRADUATE STUDENTS, AY2011-15

	Full Sample		**Analytic Sample**	
	Means	**St. Dev.**	**Means**	**St. Dev.**
% Male	0.46	0.50	0.45	0.50
% First Generation	0.36	0.49	0.30	0.46
% Asian	0.02	0.12	0.01	0.12
% Black	0.10	0.30	0.15	0.35
% Latino	0.02	0.14	0.03	0.17
% White	0.80	0.39	0.78	0.41
% financial dependent	0.74	0.45	0.97	0.17
Average Age	23	8	19	1
% Enrolled in Fall Semester	0.85	0.35	1.00	0.00
Average Expected Family Contribution	$10,742.22	$15,606.30	$14,530.99	$21,778.65
% with any work study	0.06	0.25	0.08	0.26
Average Work Study Award (all sources, incl. $0)	$150.37	$900.79	$101.34	$606.31
% with Federal Work Study	0.02	0.16	0.03	0.17
Average Federal Work Study Award (incl. 0's)	$53.45	$427.01	$51.97	$345.66
% with grant aid	0.69	0.46	0.92	0.27
Average grant aid received (including 0's)	$5,619.88	$5,962.25	$7,077.67	$6,649.54
% with Pell	0.29	0.46	0.42	0.49
% borrowing	0.43	0.50	0.69	0.46
Average student loans (including 0's)	$4,683.72	$5,718.82	$6,120.58	$6,714.50
Estimated Enrollment Adjusted Tuition & Fees	$8,721.84	$5,081.99	$11,392.90	$5,500.80
Number of Institutions	8		8	
Number of students	223,003		45,281	

Notes: Summary of student records provided by Kentucky Center for Statistics. Analytic sample includes records of first-year, recent high school graduates who applied for aid and began enrollment in AY2011-2015.

I augment the student-level data with publicly available, institution-level data from the U.S. Department of Education (DOE). Each year, institutions report information on college price and enrollment to the Integrated Postsecondary Education Data System (IPEDS). I use data on 12-month undergraduate enrollment and tuition and fee rates in my analysis. I also use annual program reports on the FWS program from the DOE Federal Student Aid office. These reports contain information on the total amount of FWS funds allocated and disbursed through the program by institution and year. I use this information to calculate annual funding changes.

2.4 Empirical Strategy

This analysis uses a difference-in-differences (DD) framework to estimate the impact of FWS offers on student outcomes. A simple comparison of FWS recipients with non-recipients would be biased if the factors determining whether an institution offers a student FWS funds also predicts student outcomes. In addition, comparing the outcomes of FWS students attending institutions with large FWS allocations to the outcomes of FWS students attending institutions with little FWS allocations may conflate institutional-differences with any potential impact from the program. A DD estimator addresses these concerns of bias. My DD analysis focuses on the two plausibly exogenous sources of variation described above: (1) annual changes in an institution's FWS funding allocation and (2) a measure of a students' *de facto* FWS eligibility, based on their EFC. These DD estimates will be biased if something differentially affects FWS eligible students attending an institution that experiences changes to their FWS allocation, compared to FWS-ineligible students. I delve more into this potential source of bias below. First, I describe my estimation approach in more detail.

2.4.1 Estimating Impacts

There are multiple ways to measure changes in FWS funding: as a percentage change, amount change scaled per student, untransformed changes, logged-changes, etc. In my primary specification, I measure the change in FWS funding as the dollar amount change per 12 month undergraduate. I also include an alternative specification where the dollar amount change is scaled by the size of the financial aid cohort in the same year. A *de facto* FWS eligible student is considered “treated” if they attend an institution which experienced a change in their FWS allocation from the prior year. This means that FWS eligible students who begin their college enrollment at an institution with no change in FWS funds in that year are part of the untreated comparison group. The specification is thus a cross-sectional comparison between FWS eligible and ineligible students across institutions that experienced some, or no, change in FWS allocations and within institutions, across years, if institutions experienced differential changes in their FWS allocations.

The canonical DD compares two groups over two periods (Wing, Simon, Bellow-Gomez, 2018). Instead of two time periods, I focus on comparing FWS eligible and ineligible students attending institutions under different funding schemes. Figure 2.2 depicts this comparison at the institution-level. For example, between 2012 and 2013, the federal government increased the FWS allocation for three institutions (Figure 2.2, top right panel). This increase ranged from $15,757 to $75,460 or a 4-7% increase from the prior year. Two institutions experienced declines in their FWS allocation ranging from $23,803 to $80,000, equivalent to a 2% and 15% decline in funding compared to the previous year.

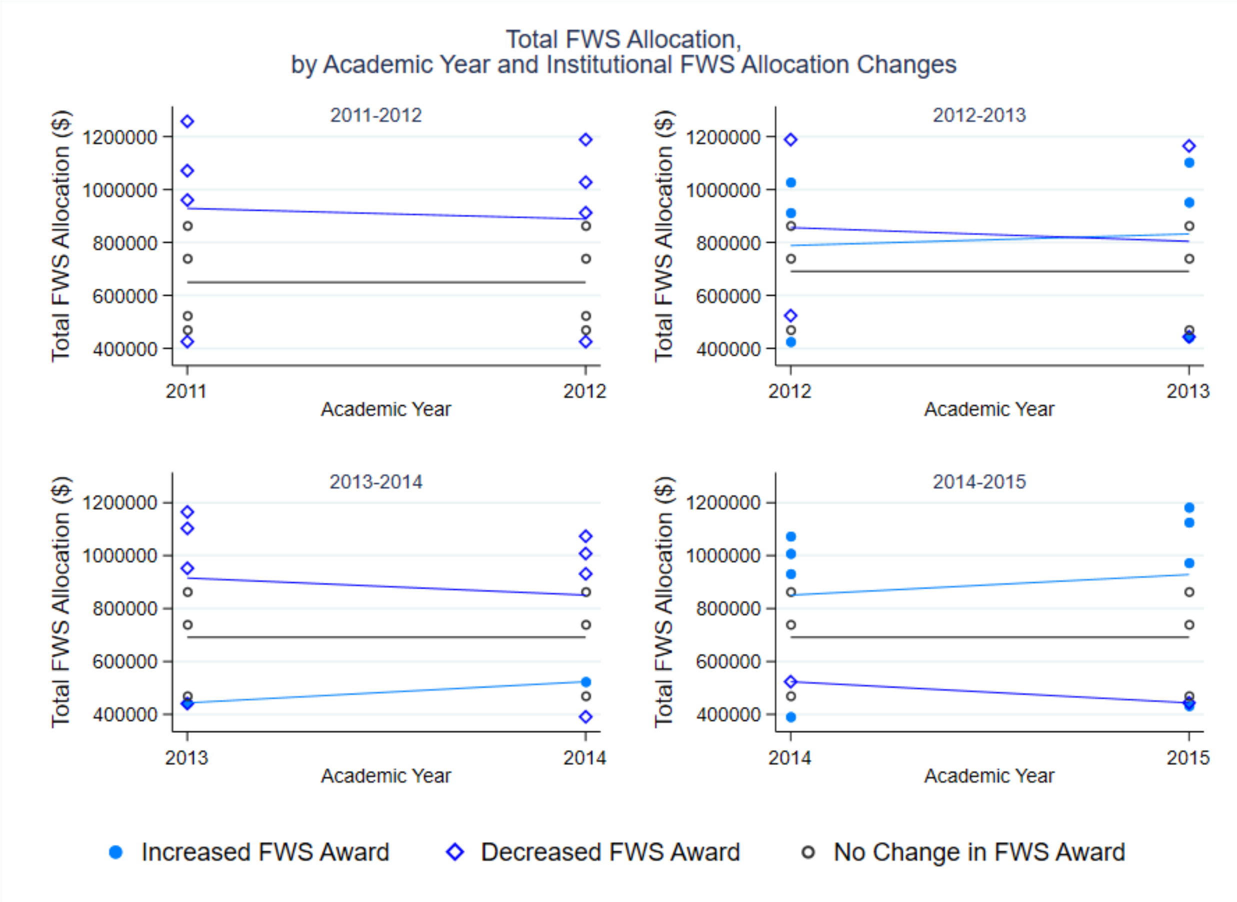

FIGURE 2.2: TOTAL FWS ALLOCATION BY YEAR AND TYPE OF FWS ALLOCATION CHANGE

Notes: This figure shows annual changes in the total FWS allocation to Kentucky public four-year institutions. Each data point represents the total FWS allocation. Solid, light blue circles represent institutions that experienced increases in its FWS allocation in the second year relative to the previous year. Hollow, blue diamonds represent institutions that experienced decreases in its FWS allocation in the second year relative to the first year.

To estimate the effect of these funding changes, I use the following model:

$$(1)\ \widehat{Y_i} = \beta_1\left(NegChange_{jt} * FWSElig_i * AllocationChange\right) + \beta_2\left(AllocationChange_{jt} * FWSElig_i\right) + \beta_3 NegChange * FWSElig_{jt} + \beta_4 AllocationChange * NegChange_{jt} + \beta_5 AllocationChange_{jt} + \beta_6 FWSElig_i + \beta_7 NegChange_{jt} + \beta_8 EFC_i + \delta_j + \gamma_t + X_i\phi + \mu_i\ ,$$

where Y_i represents student outcomes such as credits earned, persistence and graduation. β_1 and β_2 are the estimands of interest. The interaction terms $NegChange_{jt} * AllocationChange_{jt} * FWSElig_i$ and $AllocationChange_{jt} * FWSElig_i$, are difference-in-differences estimators, where $AllocationChange_{jt}$ measures the change in FWS dollars allocated to an institution *j* per undergraduate student in year *t*. I calculated this change by dividing the annual change in FWS funding by the 12-month undergraduate enrollment the institution reported to IPEDS in that academic year to account for differences in institutional size. Differences in institutional size likely affect how much year-to-year funding changes impact students; for example, an $80,000 decline in funding may have larger impacts at a smaller institution than a larger institution. $NegChange_{jt}$ is an indicator variable for whether or not an institution lost funding relative to the previous academic year. Omitting this indicator variable would constrain the estimated impact of funding changes to be the same for funding gains and losses. In other words, the triple interaction allows the impact of a funding loss to vary from a funding gain. β_1 and β_2 thus capture the effect of an additional FWS dollar lost or gained per student.

$FWSElig_i$ is a binary measure of a students' *de facto* FWS eligibility in their first academic year, as described above. β_1 and β_2 thus mechanically estimates the effect of an additional FWS dollar lost or gained per *de facto* eligible FWS students, relative to ineligible students within institutions that experienced a change in FWS funding. EFC represents a students' expected family contribution, a federally calculated measure of a student's ability to pay for college. [20] This model also includes institution fixed effects (δ_j), cohort fixed effects (γ_t)

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[20] Due to the larger number of students with EFC = 0, I test both a linear and binned specification. The binned EFC specification uses EFC quartiles with the following breakdown: first quartile are EFC=0 (24.96% of sample); second quartile are EFC [$1:$5,019] (25.04% of sample); third quartile are EFC [$5,020-$15,635] (25% of sample); fourth

and student level covariates (ϕ). The institution fixed effects account for potential systematic variation across institutions and isolates the comparison made in the interaction terms to students attending the same institution, across cohorts. In other words, the institution fixed effects assume that the difference in outcomes between FWS eligible and ineligible students is the same, across institutions. Cohort fixed effects account for systemic variation across cohorts of students. An institution-by-year fixed effect would be collinear with the main effect of the instrument, $AllocationChange_{jt}$, so is not included.

I also include student covariates in the model. The covariates are gender, race, age, dependency status, first-generation, enrollment-adjusted tuition and fee rates, the number of credits attempted in the first semester, and Pell Grant status. I control for the number of credits a student attempts in their first year because financial aid is often prorated based on a students' enrollment intensity. I include a student's Pell Grant status because the Pell Grant is a first-dollar scholarship[21], and eligibility for the Pell Grant is often synonymous with eligibility for other forms of state and institutional aid (Goldrick-Rab, 2016). Although I do not observe the tuition and fee charged to each individual student, I estimate enrollment-adjusted tuition and fee rates using IPEDS data on tuition and fees for in-state and out-of-state students and the number of credits students attempt each semester. These estimates would be inaccurate if a student took courses that incurred additional fees. I use robust standard errors clustered by institution because standard errors are likely correlated within institutions due to institution-specific financial aid practices and student supports (Cameron & Miller, 2015; Abadie, Athey, Imbens & Wooldridge,

quartile are EFC>$15,636 (25% of sample). The Pell grant threshold during the 2011 – 2015 academic years ranged from $4,995 - $5,273.

[21] First-dollar scholarships are awarded first in a student's financial aid package. Subsequent aid amounts (such as FWS awards) are determined based on a student's remaining financial need, after any first-dollar grant funds are applied.

2017). This results in more conservative standard errors than unclustered, robust standard errors; however, given that there are only 8 institutions, these errors may still be biased too small. If that were the case, then my results would be overstated in their statistical significance.

2.4.2 Identifying Assumptions

The DD method operates under the assumption that absent FWS funding changes, the difference in observed outcomes between FWS eligible and ineligible students, across institutions with and without funding changes, would remain the same. In other words, the estimates would be biased if FWS students at institutions with changing program funds were somehow affected by a policy or program change that did not affect FWS ineligible students. These might include changes to other student aid program budgets or changes to how institutions allocate work study funds. Changes to the availability of other state or federal aid programs (e.g. changes to Pell Grant eligibility) are unlikely to bias these results because changes to state and federal aid programs equally affect students regardless of whether their college experienced any FWS funding changes.

Changes to how institutions allocate work student funds, however, are a concern because these estimates would be biased if institutional aid officers changed the financial aid awards for *de facto* FWS eligible <u>and</u> ineligible students in response to federal funding changes. Institutional financial aid officers, for example, may shift state and institutional work study dollars to *de facto* FWS ineligible students who, under less federal funding, would have received no aid due to budget constraints. To a student, the source of work study funds is likely immaterial. In this case, the "treatment" of additional (less) FWS dollars would affect both eligible and ineligible students, resulting in potentially weak or null effects on related work study outcomes such as the total dollars awarded through state, institutional, and federal work study programs. I explore this possibility in two ways.

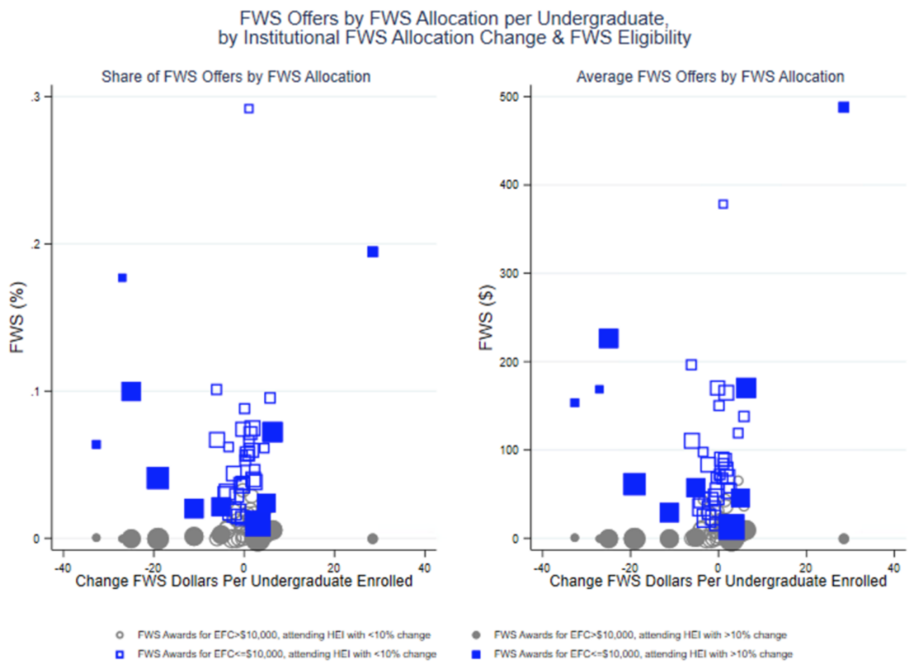

FIGURE 2.3: FWS OFFERS BY FWS ELIGIBILITY STATUS AND FWS FUNDING CHANGES

Notes: This figure shows FWS offers across students' FWS eligibility status and changes to institutional FWS allocations. On the left panel, each data point represents the share of FWS (in)eligible students that received a FWS offer in their financial aid award. On the right, each data point represents the average FWS offer an institution awarded its students in a given year, by *de facto* FWS eligibility status. Blue data points represent FWS offers for students with an expected family contribution less than or equal to $10,000 (*de facto* FWS eligible). The size of the datapoints reflect the size of the analytic sample enrolled attending the institution in that particular year.

First, I visually examine whether *de facto* ineligible students received FWS awards in Figure 2.3. The left panel of the figure depicts the share of students who received a FWS offer across institutions, by institutional funding changes and students' *de facto* eligibility status. The right panel of the figure depicts the average FWS offer awarded to students by FWS eligibility. In both figures, *de facto* FWS ineligible students receive fewer (smaller) FWS offers than their eligible peers, regardless of institutional funding changes. This figure also provides some assurance that the share and amount of FWS aid allocated to ineligible students does not broadly change across institutional funding changes.

To explore this visual pattern in more detail, I use Equation (1) to estimate the change in work study-related outcomes (Table 2.3). If institutions were awarding additional FWS dollars exclusively to *de facto* ineligible students, I might expect the impact of additional FWS funds to have a negative, or null, impact on *de facto* eligible students. Instead, I find that an additional $1 in FWS funding per undergraduate student increased the likelihood of a FWS offer to eligible students by 0.5 pp ($p<0.05$). One dollar of additional funding per undergraduate student also increased the size of a FWS offer to eligible first-year students by $14.25. In contrast, funding losses affected students' FWS offers in statistically distinguishable ways. For example, for each dollar cut from an institution's FWS allocation, eligible first-year students had a lower likelihood of a FWS offer (-0.6 pp, $p<0.05$) and smaller FWS (-18.07, $p<0.05$), relative to their peers who happened to begin their college enrollment in a year with a FWS funding gain. The differential impacts of funding gains and losses suggest that a dollar less in FWS funding has only a small impact on the likelihood and size of a FWS offer (-0.2 pp and -$3.82), supporting the specification in Equation (1) to separately estimate the impacts of funding gains and losses.

Institutional funding changes also appear unrelated to the FWS awards of *de facto* ineligible students. In a simplified version of Equation (1), where I regress additional FWS funds on the likelihood and size of a FWS offer only among students with an EFC larger than $10,000, additional FWS funds at the institution level are barely and insignificantly associated with the likelihood of receiving a FWS award (0.002 pp, p>0.1) and the size of a FWS award ($0.17, p>0.1)[22]. Later in the paper, I also examine whether and how my results change under a different *de facto* FWS eligibility threshold. If institutions responded to additional FWS dollars by allocating funds to more affluent students, raising the *de facto* FWS eligibility threshold should account for a more lenient financial aid award process.

TABLE 2-3: IMPACT OF FWS FUNDING CHANGES ON STUDENT WORK STUDY AWARDS

	Any Federal Work Study (%)	**Federal Work Study ($)**	**Non-Federal Work Study ($)**	**Any Work Study (%)**	**Any Work Study ($)**
Change measured in dollars per undergraduate					
Negative Change in Allocation X Eligibility	-0.00632*	-18.07*	-3.886	-0.00227	-8.145
	(0.00205)	(5.360)	(3.050)	(0.00254)	(5.420)
Change in Allocation X Eligibility	0.00469*	14.25*	1.187	-0.000766	1.075
	(0.00174)	(4.110)	(2.368)	(0.00210)	(3.924)
Negative Dollar Change in Allocation	0.00454	5.686	-0.521	0.00423	7.063
	(0.00235)	(4.307)	(2.432)	(0.00271)	(3.922)
Positive Dollar Change per UG enrolled	-0.00344	-4.086	3.373	-0.00160	-1.575
	(0.00162)	(3.267)	(2.454)	(0.00217)	(3.261)
FWS Eligible Students, attending Institutions that Lost Aid	-0.00181	4.187	-3.987	-0.00715	-12.00
	(0.00756)	(14.52)	(19.96)	(0.0104)	(18.49)
FWS Eligible Students, attending Institutions that did not Lose Aid	0.00907	0.732	-12.87	0.00152	-7.090
	(0.0106)	(24.20)	(12.07)	(0.0120)	(18.94)
R-squared	0.040	0.035	0.025	0.062	0.029
N	45281	45281	45281	45281	45281

Notes: * p<0.05, ** p<0.01, *** p<0.001. Each column represents a separate regression with year and institution fixed effects, as described in Equations (1) in the text. Clustered standard errors in parentheses. "Non-Federal Work Study" awards include state and institution work study dollars. The outcome "Any Work Study" includes all sources of work study dollars in a student's financial aid package (federal, state, and institutional).

[22] Detailed regression results available upon request.

Another assumption undergirding the comparison between eligible and ineligible students is that the populations of *de facto* eligible and ineligible students remained similarly comparable across funding changes. This assumption is violated if institutions experiencing changes in their FWS funding levels somehow changed who enrolled at their institution differentially across the *de facto* FWS eligible and ineligible population. Traditional parallel trends assumptions are impossible to test in the simple DD case (Wing, Simon, Bellow-Gomez, 2018) and more difficult under changing treatment conditions (Goodman-Bacon, 2019). Even so, it is important to confirm that differences between FWS eligible and ineligible students did not change as institutional funding schemes changed. To examine whether and how differences in FWS eligible and ineligible students changed across FWS eligibility and FWS funding changes, I regress a simpler version of Equation (1) on student demographic characteristics:

$$(2)\ \ \widehat{Y_i} = \beta_1\left(AllocationChange_{jt} * FWSElig_i\right) + \beta_2 AllocationChange_{jt} + \beta_3 FWSElig_i + \delta_j + \gamma_c + \mu_i$$

These balance checks suggest small and insignificant changes in student characteristics when institutions gained FWS dollars (Table 2.4). These characteristics include gender, race, age, and financial background, as measured by students' expected family contribution and Pell receipt. Changes to the difference in student characteristics across *de facto* eligible and ineligible students at institutions that lost funding were indistinguishable from the insignificant changes across students at institutions gaining FWS dollars. This suggests that differences in the composition of FWS eligible and ineligible students did not change as FWS funding changed across institutions, in spite of significant and substantial racial and financial differences across FWS eligible and ineligible students.

To explore the mechanisms behind the observed academic outcomes, I also use Equation (1) to estimate the impact of FWS funding on other forms of financial aid, such as grant aid and student loans. Below, I discuss these results in more detail.

TABLE 2-4: STUDENT DEMOGRAPHIC CHANGES ACROSS DD SPECIFICATIONS

	Male	Asian	Black	Latino	White	First Generation	Financial Dependent	Age	Average EFC	Pell (%)
Change measured in dollars per undergraduate										
Negative Change in Allocation X Eligibility	0.000998	-0.000916	0.00260	-0.00105	-0.00256	0.00323	-0.00234	0.00782	526.1	0.00832
	(0.00475)	(0.000650)	(0.00579)	(0.000871)	(0.00564)	(0.00367)	(0.00210)	(0.00751)	(536.6)	(0.00673)
Change in Allocation X Eligibility	-0.00150	0.000650	-0.00140	0.000968	0.00134	-0.00210	0.00188	-0.00918	-562.3	-0.00803
	(0.00391)	(0.000734)	(0.00554)	(0.000813)	(0.00559)	(0.00337)	(0.00192)	(0.00592)	(552.1)	(0.00575)
Negative Dollar Change in Allocation	-0.00153	0.000817	-0.00136	0.00125	0.00135	-0.00228	0.00261	-0.00745	-402.3	-0.00515
	(0.00265)	(0.000710)	(0.00494)	(0.00102)	(0.00534)	(0.00327)	(0.00187)	(0.00557)	(508.5)	(0.00534)
Positive Dollar Change per UG enrolled	0.000959	-0.000844	0.000385	-0.000903	-0.000537	0.00314	-0.00202	0.00843	461.4	0.00476
	(0.00297)	(0.000632)	(0.00438)	(0.000815)	(0.00473)	(0.00299)	(0.00168)	(0.00505)	(508.7)	(0.00484)
FWS Eligible Students, attending Institutions that Lost Aid	-0.0259	0.00688	-0.00851	-0.000730	0.00563	-0.0121	0.0108	-0.0491*	-1368.9	-0.0535*
	(0.0159)	(0.00353)	(0.0321)	(0.00528)	(0.0356)	(0.0104)	(0.0105)	(0.0155)	(3121.2)	(0.0175)
FWS Eligible Students, attending Institutions that did not Lose Aid	0.000409	0.00263	0.135**	0.00675*	-0.159**	0.240***	-0.0563***	0.0608***	-25846.4***	0.753***
	(0.0168)	(0.00143)	(0.0356)	(0.00204)	(0.0371)	(0.00991)	(0.00526)	(0.00986)	(1274.5)	(0.0151)
R-squared	0.004	0.008	0.159	0.004	0.124	0.090	0.027	0.005	0.406	0.548
N	45281	45281	45281	45281	45281	45281	45281	45281	45281	45281

Notes: * p<0.05, ** p<0.01, *** p<0.001. Each column represents a separate regression with year and institution fixed effects, as described in Equation (1) in the text. Clustered standard errors in parentheses.

TABLE 2-5: ACADEMIC IMPACTS OF FWS FUNDING CHANGES

	First Year Outcomes				Longer Term Outcomes	
	Credits Earned, Fall Yr 1	**Credits Attempted, Yr 1**	**Total College Credits Earned, Yr 1**	**GPA, Yr 1**	**Persisted, Yr 2**	**Earned Degree, by Yr 4**
Change measured in dollars per undergraduate						
Negative Change in Allocation X Eligibility	-0.0619	0.0248	-0.0678	-0.0163	-0.00240	0.00469
	(0.0465)	(0.0461)	(0.102)	(0.00695)	(0.00346)	(0.00387)
Change in Allocation X Eligibility	0.0685	-0.0204	0.0804	0.0150*	0.00188	-0.00587
	(0.0446)	(0.0482)	(0.0998)	(0.00626)	(0.00357)	(0.00405)
Negative Dollar Change in Allocation	0.0110	0.0865*	0.0697	0.0171**	0.00705*	-0.00777*
	(0.0360)	(0.0305)	(0.0715)	(0.00360)	(0.00202)	(0.00305)
Positive Dollar Change per UG enrolled	-0.0335	-0.0697	-0.100	-0.0175**	-0.00632*	0.00821*
	(0.0346)	(0.0313)	(0.0685)	(0.00353)	(0.00220)	(0.00314)
FWS Eligible Students, attending Institutions that Lost Aid	0.361	0.197	0.524	0.114**	0.0348	-0.0489*
	(0.154)	(0.284)	(0.409)	(0.0251)	(0.0170)	(0.0166)
FWS Eligible Students, attending Institutions that did not Lose Aid	-0.444**	-0.554**	-1.009**	-0.157**	-0.0615**	0.0357
	(0.105)	(0.141)	(0.220)	(0.0347)	(0.0130)	(0.0171)
R-squared	0.407	0.353	0.334	0.197	0.070	0.016
N	45281	45281	45281	44678	45281	45281

Notes: * p<0.05, ** p<0.01, *** p<0.001. Each column represents a separate regression with year and institution fixed effects, as described in Equation (1) in the text. Clustered standard errors in parentheses. GPA outcomes are only estimated for students with grades in college-level courses.

2.5 Results

2.5.1 Academic Outcomes

Table 2.5 presents estimated impacts of FWS on student outcomes such as credit completion, grades, persistence, and graduation. Overall, a $1 increase in FWS funding per undergraduate student has little impact on students' academic outcomes; this is consistent across multiple specifications. Specifically, there was no impact on credit accumulation, first-to-second year persistence or degree completion. A \$1 increase in FWS funds per undergraduate students appears to increase FWS *de facto* eligible students' GPAs by 0.015 ($p<0.05$) points. Importantly, the impact of FWS funding losses on student grades were statistically indistinguishable from funding gains, although the estimate is nearly equal in magnitude and opposite in sign.

Table 2.6 presents estimated impacts of FWS funding changes on academic outcomes using alternative specifications of Equation (1). First, I consider measuring changes to an institution's FWS allocation by the size of the undergraduate financial aid cohort.[23] This may be a more accurate measure of how changes in FWS funding affect students because some undergraduates who enroll in college either do not apply or do not qualify for aid (Table 2.6, top panel). I also run an alternative specification of Equation (1) that does not control for initial credits attempted because financial aid offers may affect a student's enrollment intensity (Table 2.6, bottom panel). As with the main specification, a \$1 change in FWS funds per financial aid recipient has little impact on students' academic outcomes. Under this specification, there is also no significant impact on student grades, although the pattern of the estimates are similar in direction and magnitude to the primary specification in Table 2.5.

I consider the implications of these results in three ways: first, I consider raising the *de facto* FWS eligibility threshold as a robustness check. Then, I compare my estimates with other evaluations of the FWS program. Lastly, I discuss other potential explanations for these results, such as student grant aid receipt and borrowing, as well as limitations of the identification strategy.

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[23] I calculate this using IPEDS data on financial aid cohort sizes.

TABLE 2-6: ALTERNATIVE SPECIFICATIONS

	First Year Outcomes				Longer Term Outcomes	
	Credits Earned, Fall Yr 1	**Credits Attempted, Yr 1**	**Total College Credits Earned, Yr 1**	**GPA, Yr 1**	**Persisted, Yr 2**	**Earned Degree, by Yr 4**
Change measured in dollars per undergraduate financial aid recipient						
Negative Change in Allocation X Eligibility	-0.0326	0.0224	-0.0280	-0.00948	-0.00123	0.00257
	(0.0286)	(0.0255)	(0.0614)	(0.00434)	(0.00217)	(0.00250)
Change in Allocation X Eligibility	0.0359	-0.0194	0.0355	0.00847	0.000838	-0.00321
	(0.0267)	(0.0255)	(0.0576)	(0.00392)	(0.00217)	(0.00250)
R-squared	0.407	0.353	0.334	0.197	0.070	0.016
N	45281	45281	45281	44678	45281	45281
Change measured in dollars per undergraduate, controlling for full-time enrollment						
Negative Change in Allocation X Eligibility	-0.0849	-0.00831	-0.105	-0.0191	-0.00307	0.00484
	(0.0622)	(0.0699)	(0.126)	(0.00841)	(0.00389)	(0.00396)
Change in Allocation X Eligibility	0.0885	0.00864	0.113	0.0174	0.00247	-0.00601
	(0.0619)	(0.0730)	(0.126)	(0.00799)	(0.00403)	(0.00413)
R-squared	0.224	0.212	0.232	0.174	0.061	0.017
N	45281	45281	45281	44678	45281	45281

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Each column represents a separate regression with year and institution fixed effects, as described in Equation (1) in the text. Clustered standard errors in parentheses. GPA outcomes are only estimated for students with grades in college-level courses.

TABLE 2-7: ROBUSTNESS CHECK USING A BROADER DEFINITION FOR FWS ELIGIBILITY

	Credits Earned, Fall Yr 1	**Credits Attempted, Yr 1**	**Total College Credits Earned, Yr 1**	**GPA, Yr 1**	**Persisted, Yr 2**	**Earned Degree, by Yr 4**
Change measured in dollars per undergraduate						
Negative Change in Allocation X Eligibility	-0.0685	0.0124	-0.0710	-0.0118*	-0.00191	0.00349
	(0.0390)	(0.0467)	(0.0936)	(0.00497)	(0.00338)	(0.00434)
Change in Allocation X Eligibility	0.0727	0.000481	0.0909	0.0127	0.00204	-0.00538
	(0.0394)	(0.0479)	(0.0937)	(0.00545)	(0.00344)	(0.00450)
Negative Dollar Change in Allocation	0.0201	0.0989*	0.0791	0.0147**	0.00699*	-0.00715
	(0.0343)	(0.0329)	(0.0736)	(0.00379)	(0.00218)	(0.00378)
Positive Dollar Change per Financial Aid Recipient	-0.0417	-0.0879*	-0.116	-0.0165**	-0.00666*	0.00821
	(0.0332)	(0.0338)	(0.0703)	(0.00368)	(0.00226)	(0.00390)
FWS Eligible Students, attending Institutions that Lost Aid	0.356	0.331	0.633	0.131**	0.0363*	-0.0583*
	(0.158)	(0.276)	(0.417)	(0.0313)	(0.0145)	(0.0168)
FWS Eligible Students, attending Institutions that did not Lose Aid	-0.331*	-0.392*	-0.733*	-0.136**	-0.0350*	0.0253
	(0.125)	(0.136)	(0.261)	(0.0314)	(0.0130)	(0.0160)
R-squared	0.406	0.352	0.334	0.197	0.069	0.016
N	45281	45281	45281	44678	45281	45281

Notes: * p<0.05, ** p<0.01, *** p<0.001. Each column represents a separate regression with year and institution fixed effects, as described in Equation (1) in the text. Clustered standard errors in parentheses. The *de facto* eligibility threshold for FWS is $15,000 across each column. GPA outcomes are only estimated for students with grades in college-level courses.

2.5.2 Robustness Check

Table 2.7 presents estimates using a higher EFC threshold for FWS eligibility. Changing the threshold is one way to test concerns that FWS funding changes resulted in changes to work study awards for *de facto* ineligible students. Increasing the *de facto* eligibility threshold from \$10,000 to \$15,000 accounts for 98% of students that received some form of FWS and 72% (326 of 448) of the originally *de facto* ineligible students who received a FWS award. The pool of eligible students increases from 25,734 students to 30,390 students, an 18% increase. The number of FWS recipients with an EFC within the *de facto* eligible threshold increased by 92 students, a 7% increase.

Under this new specification, increases in FWS funds per undergraduate student have no significant impact on the likelihood and size of FWS offers for *de facto* eligible students, although the estimates are similar in magnitude to the primary specification. As before, there are no significant impacts on credit accumulation, persistence or degree completion. The positive impact of funding gains on first-year GPA is no longer significant under the broadened eligibility threshold, although the estimate is similar in magnitude. The impact of funding losses, under this specification, does vary significantly from the impact of funding gains (-0.012, $p<0.05$). Although the magnitude of the estimates are similar across Table 2.7 and Table 2.5, the loss of precision may be expected given that the higher eligibility threshold substantially expands the pool of potential FWS students.

2.5.3 Comparison with Prior Research

Other FWS evaluations have also estimated the impact of FWS funds on student outcomes using quasi-experimental methods. Soliz and Long (2016) and Scott-Clayton (2011) instrument for FWS dollars using differences in FWS funding levels across institutions in Ohio and West Virginia, respectively. In Ohio, Soliz and Long (2016) find that a $100 increase in FWS funding increases the number of college credits earned in the first year by 1.2 credits. In West Virginia, Scott-Clayton (2011) also finds suggestive evidence that FWS funds may increase first year credit completion, particularly for men. Unlike Scott-Clayton (2011), I do not observe whether students earn their FWS award. My focus on annual changes to FWS funds, however, does suggest that funding increases can increase the size of FWS awards by $14.25 (Table 2.3). Using this estimate to scale the estimated academic impacts suggests similar, albeit imprecise estimates, on credit accumulation: a $100 increase in a student's FWS award may increase credit completion in the first year by 0.6 credits, on average.[24]

In terms of student grades, both papers find suggestive, but statistically insignificant, negative impacts on students' first-year GPA (Soliz & Long, 2016; Scott-Clayton, 2011). The insignificant, negative GPA impact from these studies is consistent with national estimates of FWS participation using propensity score matching methods (Scott-Clayton & Minaya, 2016). Scott-Clayton and Minaya (2016) also find larger, significant negative impacts (-0.055, $p<0.01$) on student grades for FWS recipients who likely would not have worked without the FWS offer. My results suggest the opposite: a $100 increase in a student's FWS award increases their GPA by 0.11 points, on average. I discuss potential hypotheses and mechanisms for these results in the next section.

For longer-term degree outcomes, Soliz and Long (2016) find no impact on first-to-second year persistence. Scott-Clayton (2011) finds suggestive evidence of negative impacts on degree attainment, although these are again statistically insignificant. In contrast, Scott-Clayton

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[24] I scale these estimates by dividing the estimate academic effect by $14.25 and multiplying by $100.

and Minaya (2016) find that FWS participation can increase first-to-second year persistence (1.1 pp, p<0.05) and six-year Baccalaureate degree attainment rates (3.2 pp, p<0.01). Scott-Clayton and Minaya (2016) suggest that these discrepancies may be due to context: Soliz and Long (2016) and Scott-Clayton (2011) study FWS program effects in more rural areas, while Scott-Clayton and Minaya (2016) estimate program effects from a national sample. My results on first-to-second year persistence and degree attainment are also insignificant, which may be reasonable given the similarity between Ohio, West Virginia, and Kentucky.

2.5.4 Potential Mechanisms & Considerations

Why might changes to FWS funding affect student grades in the Kentucky context, but not others? One hypothesis might be that institutions in Kentucky change their financial aid packaging practices as a result of FWS funding changes. Work study dollars are awarded to students in conjunction with grant aid and loan aid. Grant and loan aid also influence student academic outcomes. Understanding whether and how changing FWS funding levels impacted other sources of financial aid may help elucidate the mechanisms behind these results. Table 2.8 presents the impact of changes to FWS funding on other financial aid sources, such as grant aid and student loans. Changes to an institution's FWS allocation had no significant impact on student grant aid receipt. There was also no significant impact on borrowing. The estimated impact on student grant and loan aid remains insignificant across alternative specifications.[25]

It is thus unlikely that the null results on credit completion, and small impacts on GPA, are due to interactions between the FWS program and other sources of aid. Although Soliz and

[25] As before, these alternative specifications consisted of an alternative measure of funding changes ($ change per financial aid recipient); controlling for enrollment intensity with a full-time enrollment indicator rather than continuous measure of credits attempted; and, broadening the FWS eligibility threshold to $15,000 EFC.

Long (2016) and Scott-Clayton (2011) do not report the impact of FWS dollars on other sources of student financial aid, Scott-Clayton and Minaya (2016) find that FWS recipients are 20 pp ($p<0.001$) more likely to borrow than matched, non-recipients. Nationally, FWS recipients also borrow approximately $1,270 ($p<0.001$) more in their first year than non-recipients (Scott-Clayton and Minaya, 2016). In my sample, FWS recipients do have a much higher rate of borrowing (78% vs. 68%); however, this does not appear to result directly from marginal changes to institutional FWS funds.

Table 2.8 also presents financial aid outcomes for students' second year of enrollment. These estimates suggest that FWS funding changes had no significant impact on students' financial aid packages in the second year. Although insignificant, the estimates on second year work study dollars is similar in magnitude and direction as in the first year, suggesting that multi-year aid dynamics may be important to consider. Under this specification, I do not account for how changes to FWS allocations in a student's second (or later) year are affecting these estimates of second-year impacts. The model also bypasses whether and how funding changes in earlier years may affect observed FWS offers in a given year. In these estimations, institutions may move across treated (and untreated) groups from year to year, depending only on the FWS funding change in a given year. These results thus may be biased if earlier funding changes affected a given year's FWS allocation and a student's FWS award.

TABLE 2-8: EFFECTS OF FWS FUNDING CHANGES ON NON-WORK STUDY FINANCIAL AID

	First Year Outcomes				Second Year Outcomes						
	Grant Aid (%)	**Grant Aid ($)**	**Any Loans (%)**	**Loan Aid ($)**	**Applied for Financial Aid (%)**	**Grant Aid (%)**	**Grant Aid ($)**	**Any Loans (%)**	**Loan Aid ($)**	**Federal Work Study ($)**	**Any Work Study ($)**
Change measured in dollars per undergraduate											
Negative Change in Allocation X Eligibility	0.00270	-36.22	0.00831	53.22	-0.00205	0.00355	-49.31	0.000958	19.76	-12.90	-4.883
	(0.00367)	(76.46)	(0.00427)	(47.18)	(0.00359)	(0.00377)	(124.9)	(0.00336)	(49.66)	(7.337)	(3.253)
Change in Allocation X Eligibility	-0.00109	6.956	-0.00485	-7.253	-0.0000226	-0.00356	53.03	-0.00117	11.75	11.57	-3.818
	(0.00350)	(77.42)	(0.00429)	(61.54)	(0.00378)	(0.00410)	(110.1)	(0.00333)	(53.68)	(6.573)	(1.757)
Negative Dollar Change in Allocation	-0.00151	-12.48	-0.00631	11.17	0.00624**	-0.00139	-178.2	0.00262	82.40	-4.907	1.811
	(0.00322)	(69.99)	(0.00400)	(44.68)	(0.00175)	(0.00253)	(90.46)	(0.00202)	(45.89)	(5.773)	(3.670)
Positive Dollar Change per UG enrolled	0.000347	-7.564	0.00381	-28.55	-0.00461	0.000525	130.4	-0.00248	-67.43	4.177	10.30*
	(0.00337)	(55.86)	(0.00398)	(57.02)	(0.00207)	(0.00285)	(93.17)	(0.00224)	(47.30)	(4.739)	(3.100)
FWS Eligible Students, attending Institutions that Lost Aid	0.0507	696.7	-0.00395	281.9	0.0246	0.0536	681.5	0.0268	415.4	5.599	-35.35
	(0.0290)	(452.1)	(0.0225)	(445.9)	(0.0115)	(0.0323)	(437.6)	(0.0258)	(267.1)	(24.23)	(33.16)
FWS Eligible Students, attending Institutions that did not Lose Aid	-0.0258	-535.1	0.0138	-35.26	-0.0525**	-0.0393	-119.6	-0.0224	-241.7	28.59	-8.553
	(0.0178)	(345.9)	(0.0435)	(441.9)	(0.0135)	(0.0217)	(399.9)	(0.0400)	(443.9)	(31.30)	(18.74)
R-squared	0.141	0.229	0.076	0.136	0.071	0.051	0.152	0.039	0.112	0.035	0.054
N	45281	45281	45281	45281	45281	45281	33338	45281	33338	33338	45281

Notes: * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Each column represents a separate regression with year and institution fixed effects, as described in Equation (1) in the text. Clustered standard errors in parentheses.

These results would also be biased if institutional responses to FWS funding changes resulted in selective increases in FWS funds for a subset of FWS eligible students. For example, if aid administrators systematically awarded additional FWS funds to the most academically prepared FWS eligible students, then these positive gains in GPA may understate the impact among students actually impacted by the funding change and overstate the impact on the average student. Future research should thus examine alternative definitions of FWS eligible thresholds that include high school GPA, standardized test scores, and/or merit aid receipt. In addition, future research should examine the role of heterogeneous treatment effects. Many of the recent innovations in DD estimation methods suggest that estimators under changing treatment conditions may be biased under treatment effect heterogeneity (Goodman-Bacon, 2019; Callaway & Sant'Anna, 2020). This paper varies slightly from the papers discussing these methodological innovations in that I use cross-sectional data and rely on variation in funding levels, rather than time. Even so, further analysis is needed to explore these issues of estimation and generalizability.

2.6 Conclusion

The FWS program is a small fraction of the resources available to institutions and students. Changes in program funding, however, could play an important role in how institutions package financial aid dollars and how students finance their education. In this study, I examined the impact of FWS funding changes at Kentucky public four-year institutions. I find that changes to the FWS program had no effect on credit accumulation, persistence, and degree completion. The effect on credit accumulation is similar in magnitude to FWS evaluations in other contexts

(Soliz & Long, 2016; Scott-Clayton, 2011). I find small, positive impacts on student grades, which is at odds with other FWS evaluations (Soliz & Long, 2016; Scott-Clayton, 2011).

My results also highlight how institutions may respond to federal funding changes. I find evidence that institution responses varied depending on whether funding is increased or decreased. Increases in FWS allocations resulted in both an increased likelihood of FWS receipt and larger FWS awards, on average. Decreases in FWS allocations had little impact, suggesting that rather than decrease the likelihood and size of FWS awards in response to fewer program dollars, institutions may attempt to smooth over funding losses for students. I find no evidence that Kentucky institutional aid offices substitute work study with grant and/or loan aid in response to FWS allocation changes. Future research, however, should examine whether institutional allocation practices vary based on the presence of additional aid and if this might explain the positive impact on student grades in this context.

Future research into student employment should also consider other ways institutional context might mediate students' work study experiences and quasi-experimental results. In particular, prior-year funding levels and changes to federal funding may set a precedent for financial aid awards that is currently unaccounted for in this analysis. Future research should examine whether and how prior-year funding gains and losses also affect FWS awards and student outcomes.

Conclusion to the Dissertation

As the United States again faces high unemployment rates due to the COVID-19 pandemic, understanding how student employment responds to economic downturns – and how expanding campus-based work study programs may impact student success – can inform current policy discussions. This dissertation presents a set of complementary studies as a small contribution to that effort. The first study examines longitudinal changes in the factors predicting student employment before, during, and after the 2008 recession. The second study examines the causal effects of the FWS program in Kentucky, immediately after stimulus dollars from the 2009 American Recovery and Reinvestment Act were dispersed to states, businesses and higher education institutions.

I find that similar to the broader labor market, student employment rates are heavily influenced by local unemployment rates. In addition, I find that college costs are less predictive of student employment than has historically been the case. My findings also suggest that part-time and full-time workers may have different determinants of work, in line with other work examining how labor market elasticities vary across contexts (Keane & Rogerson, 2015).

In the second paper, I present evidence that increasing funding for campus-based work study programs can increase the number and size of FWS offers. I find some evidence that these funding increases may slightly increase student grades; the statistical significance of these effects, however, are sensitive to model specifications. Funding declines, on the other hand, appear to have no effect on FWS aid offers. The effect of funding declines is also statistically indistinguishable from the effect of funding increases. This suggests that institutions play an important mediating role in student employment: institutions appear to pass on funding increases to students while shielding them from funding declines.

Combined, these papers suggest that increasing work study funds may boost the supply of jobs available to undergraduate students and help mitigate the possibility that students' job prospects are increasingly tied to macroeconomic forces. Whether increasing FWS dollars is a viable policy solution to declining student employment rates, however, still requires additional research. In particular, future research into the quality of FWS positions and students' decision-making processes around work (e.g. who takes FWS jobs when offered and why) can help educators and policymakers build more student-centered and student-focused educational systems. The findings from the dissertation are one reminder that students do not make educational choices in a vacuum – rather, local macroeconomic contexts and institutional decisions may profoundly affect the opportunities available to students and their ability to succeed.

Appendices

Appendix A: Additional Tables for Chapter 1

APPENDIX TABLE 1-A: OLS REGRESSION OF STUDENT EMPLOYMENT MEASURES, 1999-2018

	Employment Rate (%)	**Weekly hours worked**
Male	-0.0233***	2.332***
	(0.004)	(0.120)
Nonwhite	-0.0898***	0.716***
	(0.004)	(0.149)
Age 25-34	0.163***	8.648***
	(0.005)	(0.170)
Age 35-44	0.180***	10.15***
	(0.008)	(0.260)
Age 45-54	0.164***	10.63***
	(0.010)	(0.320)
Age 55 and Over	0.0153	9.823***
	(0.017)	(0.677)
Midwestern State	0.104***	1.342***
	(0.006)	(0.185)
Southern State	0.0519***	2.595***
	(0.005)	(0.179)
Western State	0.0633***	1.291***
	(0.006)	(0.185)
Married	0.0371***	3.531***
	(0.006)	(0.186)
Public 4-Year	-0.0558***	-1.659***
	(0.004)	(0.124)
Private 4-Year	-0.0888***	-2.797***
	(0.005)	(0.191)
State by Year Unemployment Rate	-0.0171***	-0.160***
	(0.001)	(0.029)
Intercept	0.608***	23.60***
	(0.007)	(0.246)
R-sq	0.051	0.191
F	346.5	781.1
N	116,702	62,856

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Standard errors are in parentheses and estimated at the individual level to account for repeated sampling of the same individual across survey years. Estimates are weighted using the EDSUPPWT provided with the IPUMS-CPS data (Flood, *et al.,* 2020).

APPENDIX TABLE 1-B: SUMMARY OF VARIABLES USED ACROSS NPSAS SURVEYS

Variable Type	***Original NPSAS Variable***
Outcome Variables	
Likelihood of Working	Constructed from JOBHOUR2; students were marked as employed if they reported more than 0 hours of work per week, on average during the school year.
Hours Worked Per Week	JOBHOUR2: average hours worked per week during the school year, including work-study jobs
Demographic Variables	
Age	AGE: The student's age as of Dec. 31st of the first calendar year in the academic year (e.g., Dec. 31, 2003). Provided by NPSAS.
Expected Family Contribution	Constructed from EFC
Financial Dependency Status	Constructed from DEPEND2
First-Generation Status	Constructed from PAREDUC; harmonized to account for differences in categories across NPSAS years
Gender	GENDER: Two categories, male or female.
Income	Constructed from CINCOME and adjusted for inflation using CPI
Race/Ethnicity	RACE variable; recoded to account for the addition of "More than one race" in 2012 & 2016
College Cost Variables	
Amount Borrowed	Constructed from TOTLOAN and adjusted for inflation using CPI
Cost of Attendance	Constructed from BUDGETAJ and adjusted for inflation using CPI
Financial Need	Constructed from SNEED5 (unmet need after grant aid) and adjusted for inflation using CPI
Enrollment Variables	
Concurrent Enrollment Status	Constructed from ATTNSTAT
Full-time Enrollment Status	Constructed from ATTNPTRN
In-state Enrollment Status	Constructed from SAMESTAT
Institution Type	Constructed from SECTOR4 and Carnegie Classifications as reported to IPEDS.
On-Campus Residency Status	Constructed from LOCALRES; harmonized so that categories match across NPSAS administrations.
Technical Variables	
Excluding enrollment at Puerto Rico Institutions	Constructed from COMPTTO87 variable. Accounts for the different sampling frame for NPSAS:12, which excluded institutions in Puerto Rico.
Undergraduate Student Status	Constructed from STYPELST. In NPSAS:04, graduate students were included in the NCES data files.
Analytic Weight	WTA000
Strata	ANALSTR
Primary Sampling Unit	ANALPSU
Replicate Weights	WTA001-WTA200. NPSAS:04 replicate weights were obtained from the NCES-provided revised weight file (published in 2009).

APPENDIX TABLE 1-C: OLS ESTIMATES OF STUDENT EMPLOYMENT, WITH DIFFERENT VARIANCE ESTIMATION PROCEDURES, 2012.

	Likelihood of Working				**Hours Worked**			
		Certainty	**Scaled**	**Centered**		**Certainty**	**Scaled**	**Centered**
Demographic Characteristics								
Non-White	-0.046***	(0.006)	(0.006)	(0.006)	0.09	(0.20)	(0.20)	(0.20)
Other Race	-0.019	(0.015)	(0.015)	(0.015)	0.1	(0.67)	(0.67)	(0.67)
Male	-0.018**	(0.006)	(0.006)	(0.006)	1.84***	(0.20)	(0.20)	(0.20)
First Generation	-0.01	(0.006)	(0.006)	(0.006)	1.06***	(0.20)	(0.20)	(0.20)
Financial Dependent	-0.08***	(0.010)	(0.010)	(0.010)	-4.74***	(0.36)	(0.36)	(0.36)
Independent, without Dependents	-0.025**	(0.008)	(0.008)	(0.008)	-0.51	(0.31)	(0.31)	(0.31)
Age 25-34	-0.011	(0.009)	(0.009)	(0.009)	2.04***	(0.34)	**(0.35)**	(0.34)
Age 35-44	-0.032*	(0.014)	**(0.015)**	(0.014)	3.41***	(0.45)	(0.45)	(0.45)
Age 45-54	-0.085***	(0.016)	(0.016)	(0.016)	2.83***	(0.61)	(0.61)	(0.61)
Over 55	-0.193***	(0.032)	**(0.033)**	**(0.033)**	2.02*	(0.89)	**(0.90)**	(0.89)
Income (2016 $, logged)	0.021***	(0.001)	(0.001)	(0.001)	0.40***	(0.05)	(0.05)	(0.05)
EFC (2016 $, logged)	0	(0.001)	(0.001)	(0.001)	0.08*	(0.03)	(0.03)	(0.03)
College Costs								
Student Budget (2016 $, logged)	-0.007	(0.008)	(0.008)	(0.008)	-3.10***	(0.29)	(0.29)	(0.29)
Unmet Need (2016 $00's)	0	(0.000)	(0.000)	(0.000)	0	(0.00)	(0.00)	(0.00)
Loan Amount (2016 $000s)	0.003***	(0.000)	(0.000)	(0.000)	0.01	(0.02)	(0.02)	(0.02)
Unemployment Rates								
Unemployment Rate	-0.009***	(0.002)	(0.002)	(0.002)	-0.09	(0.06)	(0.06)	(0.06)
Enrollment Patterns								
Enrolled at 2-Year Institution	0.039*	(0.018)	(0.018)	(0.018)	-3.69***	(0.51)	(0.51)	(0.51)
Enrolled at 4-Year Institution	0.053**	(0.018)	(0.018)	(0.018)	-4.93***	(0.51)	**(0.52)**	**(0.52)**
Enrolled In-State	0.045**	(0.014)	(0.014)	(0.014)	-0.97**	(0.37)	**(0.38)**	(0.37)
Lives Off-Campus	0.173***	(0.009)	(0.009)	(0.009)	4.86***	(0.35)	(0.35)	(0.35)
Ever Enrolled Full-Time	-0.079***	(0.009)	**(0.010)**	(0.009)	-1.41***	(0.30)	(0.30)	(0.30)

(continued)

Intercept								
Intercept	0.503***	(0.078)	**(0.079)**	(0.078)	54.83***	(2.91)	**(2.94)**	**(2.92)**
R-sq		0.052	0.052	0.052		0.215	0.215	0.215
F		74.47	73.05	73.15		184.6	181.3	181.1
Strata (n)		735				734		
PSU (n)		1,460				1,459		
Subpopulation Size		20,318,520				13,435,010		
N		78,830				46,910		

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Each panel presents results from an Ordinary Least Squares (OLS) regression, as described in Equation (1) in the text. Standard errors across three different variance estimation methods using Stata's *svy* command are represented in parentheses; bolded values identify when the method resulted in a different standard error value. Statistical significance levels of the estimates did not change across variance estimation methods. Standard errors generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200). Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000.

APPENDIX TABLE 1-D: ALTERNATIVE SPECIFICATIONS OF REWEIGHTED RIF KOB DECOMPOSITION, 25TH PERCENTILE OF HOURS WORKED, 2004-2016

	Untransformed Age	**Excluding Work Study**	**Interacted Re-weighting Factor**
Hours Worked (2016)	19.11***	20.68***	19.11***
	(0.56)	(0.09)	(0.56)
Hours Worked (Counterfactual Year 2)	20.66**	21.20***	20.76***
	(0.36)	(0.25)	(0.32)
Hours Worked (2004)	20.60***	20.93***	20.60***
	(0.11)	(0.11)	(0.11)
Simple Difference (Year 2 – Year 1)	-1.59**	-0.24~	-1.49**
	(0.57)	(0.14)	(0.57)
Compositional Effects			
Total	-1.54**	-0.51*	-1.65**
	(0.54)	(0.23)	(0.53)
Pure Explained	-0.69**	-0.46*	-0.64**
	(0.25)	(0.21)	(0.24)
Specification Error	-0.86~	-0.005	-1.01*
	(0.48)	(0.14)	(0.48)
Structural Effects			
Total	0.06	0.27	0.16
	(0.37)	(0.27)	(0.33)
Reweight Error	0.46*	0.38~	0.43*
	(0.22)	(0.21)	(0.21)
Pure Unexplained	-0.4	-0.11	-0.27
	(0.26)	(0.18)	(0.22)
Sample Size, Year 1	65,960	65,960	91,110
Sample Size, Year 2	71,500	91,110	78,830

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

APPENDIX TABLE 1-E: ALTERNATIVE SPECIFICATIONS OF REWEIGHTED RIF KOB DECOMPOSITION, 75TH PERCENTILE OF HOURS WORKED, 2004-2016

	Untransformed Age	**Excluding Work Study**	**Interacted Re-weighting Factor**
Hours Worked (2016)	41.21***	41.35***	41.21***
	(0.06)	(0.06)	(0.06)
Hours Worked (Counterfactual Year 2)	41.44***	41.55***	41.48***
	(0.09)	(0.09)	(0.10)
Hours Worked (2004)	41.85***	41.80***	41.85***
	(0.05)	(0.05)	(0.05)
Simple Difference (Year 2 – Year 1)	-0.64***	-0.45***	-0.64***
	(0.07)	(0.07)	(0.07)
Compositional Effects			
Total	-0.23*	-0.21*	-0.27**
	(0.09)	(0.09)	(0.10)
Pure Explained	-0.28*	-0.25*	-0.30*
	(0.13)	(0.12)	(0.13)
Specification Error	0.06	0.04	0.03
	(0.09)	(0.09)	(0.10)
Structural Effects			
Total	-0.41***	-0.25**	-0.37***
	(0.09)	(0.09)	(0.10)
Reweight Error	0.13	0.09	0.13
	(0.08)	(0.07)	(0.10)
Pure Unexplained	-0.54***	-0.34***	-0.50***
	(0.08)	(0.08)	(0.08)
Sample Size, Year 1	65,960	65,960	91,110
Sample Size, Year 2	71,500	91,110	78,830

Notes: ~ p<0.1; * p<0.05; ** p<0.01; *** p<0.001. Sample includes all NPSAS survey respondents with complete demographic information. Financial variables are adjusted for inflation to 2016 dollars using the CPI. Estimates are weighted using NCES-provided analytic weight WTA000. Standard errors are generated using 200 replicates and NCES-provided bootstrap weights (WTA001-WTA200).

Appendix B: Regression Analysis of Student Employment Using CPS Data

Figures 1.2 in the text displays predicted and actual values of two measures of student employment: employment rates and hours worked. The values were calculated using data from the Current Population Survey's (CPS) October Supplement using IPUMS-CPS data (Flood, *et al.*, 2020). Predicted values were estimated using the following model:

$$\text{(A.1)}\ \hat{Y}_i = \beta_0 + \beta_1 \overrightarrow{Demographics_i} + \beta_3 \overrightarrow{UR_{st}} + \beta_4 \overrightarrow{Enrollment_i} + \epsilon_i \ ,$$

where $\hat{Y}_{it}$ represents either the probability of a student working or average weekly hours worked, for individual, *i*. The probability of a student working is calculated for all undergraduate records in the CPS. Average weekly hours worked are only calculated among undergraduate workers. Demographic characteristics include indicator variables for gender, race, age categories[26], geographic region, and marital status. I also include indicators for whether a student is enrolled at a public or private four-year institution and state-by-year unemployment rates, following Scott-Clayton (2012). Unlike Scott-Clayton (2012), I do not include information on students' parental education and income.[27] Estimates are weighted using population weight EDSUPPWT, as recommended by Flood, *et al.* Standard errors are clustered at the individual level to account for repeated sampling across consecutive years. Appendix Table 1-A presents the resulting estimates from the regression; I use these estimates to calculate the population-weighted average employment rate and weekly hours worked for each year.

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[26] Scott-Clayton (2012) uses indicator variables for each age represented in her sample of 18-22 year old undergraduates. I use indicator variables for categories of age groupings aligned with the labor market literature and Bureau of Labor Statistics reporting: less than 24; 25-34; 45-54; over 55.

[27] Flood, et al. recommend using family income data after 2010 with caution due to the high number of income values that were originally missing.

Appendix C: Re-weighting Counterfactuals for KOB Decompositions

The traditional KOB decomposition relies on comparing linear regression models for two groups – in this case, two cohorts of undergraduate students (e.g. students enrolled in AY2003-04, compared to students enrolled in AY2015-16). If the linear model represented by Equation (1) is accurate, then the $(\bar{X}_B)\hat{\beta}_A$ terms in the decomposition (Equation 2) accurately reflects what the employment patterns of AY2015-16 undergraduates would be, under the structural relationships of AY2003-04 undergraduates. The $(\bar{X}_B)\hat{\beta}_A$ terms would be inaccurate if the relationship were nonlinear, due to an inaccurate estimate of $\hat{\beta}_A$. To loosen the linearity restriction, I calculate a counterfactual group. This method creates a comparison group where the observations for one group (e.g., Group B) are weighted based on the ratio of the probability of belonging to Group B relative to Group A, using both the raw probability and conditional probability of group membership (Rios-Avila, 2020; Fortin, Lemieux & Firpo, 2011). To do so, I utilize a logit regression model estimates the conditional probability of group membership to first predict the likelihood of belonging to Group B. Equation B.2 represents one specification used to calculate the reweighting[28]:

$$\text{(B.2)} \quad \widehat{logit}(Group\ B = 1) = \beta_0 + \beta_1 \overrightarrow{Demographics} + \beta_2 \overrightarrow{Finance} + \beta_3 \overrightarrow{Enrollment} + \epsilon_i$$

The predicted probabilities of Group B membership and the raw probability of Group B membership in the full analytic sample determine the reweighting factor, $\hat{\psi}(X)$:

[28] I also run alternative specifications, with interactions between college costs and enrollment; and financial dependency status and income. The results are similar across interacted and uninteracted models.

$$(\text{B.3})\ \ \hat{\psi}(X) = \frac{\frac{\hat{P}(Group\ B|X)}{\hat{P}(Group\ B)}}{\frac{\hat{P}(Group\ A|X)}{\hat{P}(Group\ A)}}$$

The reweighting factor calculated in equation (B.3) is akin to an inverse probability weight: it re-scales Group B's distribution to represent what would have occurred if Group B had the same overall characteristics as Group A. The reweighting factor generates a counterfactual distribution as well as specific counterfactual coefficients ($\hat{\beta}_B^C$) and counterfactual means (X_B^C) of student employment:

$$\hat{\beta}_B^C = \left(\sum_{i \in B} \hat{\psi}(X_i) \cdot X_i \cdot X_i^T \right)^{-1} \cdot \sum_{i \in B} \hat{\psi}(X_i) \cdot X_{Bi} \cdot X_i$$

$$X_B^C = \sum_{i \in B} \hat{\psi}(X_i) \cdot X_i$$

The re-weighted counterfactual also sheds light on the consistency and accuracy of the decomposition results. If the reweighting factor were consistent, and the OLS model underlying the decomposition in equation (1) was accurate, then the means and coefficients would be the same across the traditional KOB decomposition and decomposition with the re-weighted counterfactual (Fortin, Lemieux, & Firpo, 2011).

Appendix D: Decomposition of Student Employment Using CPS Data

To test the robustness of my results, I run similar decomposition models on data from the Current Population Survey's (CPS) October Supplement using IPUMS-CPS data (Flood, *et al.*, 2020). The October Supplement collects information on students' educational enrollment patterns, which allows me to observe what grade (including undergraduate and graduate school) a student is enrolled in. The survey also collects rich demographic information on student's gender, race, age, and marital status. CPS data, however, differ from NPSAS data in key areas. First, the CPS does not collect information on which specific institution students are attending and whether a student is attending an institution in-state. It also does not collect information on students' financial aid packages and borrowing behavior. In addition, the racial categories in the CPS are much more nuanced than the NPSAS categories. Thus, while the decomposition using CPS data is similar to Equations (1) and (2) in the text, the college enrollment and college cost variables differ substantially. For college enrollment factors, students enrollment intensity is only collected for 16-24 year olds. Because many students who enroll in college are older than 24, I do not control for enrollment intensity in the decomposition model using CPS data. For college cost factors, I use Integrated Postsecondary Education Data System (IPEDS) on cost of attendance and number of students who borrow to estimate enrollment-weighted state-level averages of college cost and borrowing rates. For student demographic characteristics, I omit indicators for students' financial dependency status and parental education. I also collapse the indicators for race into a non-White and White indicator (Latino students are categorized based on their race).

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