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The labor market returns to a for-profit college education☆

Stephanie Riegg Cellini [a,b,*], Latika Chaudhary [c]

[a] Trachtenberg School of Public Policy and Public Administration, George Washington University, United States
[b] NBER, United States
[c] Graduate School of Business and Public Policy, Naval Postgraduate School, Monterey, California, United States

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ABSTRACT

A lengthy literature estimating the returns to education has largely ignored the for-profit sector. In this paper, we estimate the earnings gains to for-profit college attendance using restricted-access data from the 1997 National Longitudinal Survey of Youth (NLSY97). Using an individual fixed effects estimation strategy that allows us to control for time-invariant unobservable characteristics of students, we find that students who enroll in associate's degree programs in for-profit colleges experience earnings gains of about 10% relative to high school graduates with no college degree, conditional on employment. Since associate's degree students attend for an average of 2.6 years, this translates to a 4% return per year of education in a for-profit college, slightly lower than estimates of returns for other sectors found in the literature.

1. Introduction

After several decades of moderate growth and relatively little controversy in for-profit college education, the 2000s ushered in a new era in higher education. The low cost of providing online education, the availability of federal student aid, and the growing number of students seeking skills beyond the high school level, combined to produce enormous growth in the for-profit sector. Since 2000, enrollment in for-profit colleges has more than tripled, climbing to 2.5 million students in 2010 (National Center for Education Statistics (NCES), 2012, Tables 219 and 222).

With this growth has come increased attention from policymakers, the media, the education community, and students themselves. In the past few years, reports of unscrupulous recruiting practices, fraud in federal financial aid programs, low graduation rates, and high student loan default rates (e.g., Goodman, 2010; Lewin, 2010; United States Government Accountability Office (GAO),

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* Corresponding author at: Trachtenberg School of Public Policy and Public Administration, 805 21st Street, NW, #601, Washington, DC 20052, George Washington University, United States. Tel.: +1 202 994 0019.
E-mail addresses: scellini@gwu.edu (S.R. Cellini), lhartman@nps.edu (L. Chaudhary).

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2010; U.S. Senate Committee on Health, Education, Labor, and Pensions, 2012) have led to new policy proposals and an intense debate over the costs and benefits of for-profit higher education. Notably, the Obama administration recently proposed controversial "gainful employment" regulations that would link an institution's eligibility for federal student aid to student debt, default rates, and labor market outcomes (Federal Register, 2014).

Central to the debate over these regulations and the future of the for-profit industry is the question of the quality of a for-profit education. Those arguing in favor of additional regulation believe that for-profit colleges leave students with insurmountable debt and few skills, while proponents argue that these institutions provide valuable job training for underserved students. Both sides rely heavily on anecdotal evidence and simple comparisons of for-profit students with students in other sectors. Without a better understanding of the causal effects of a for-profit education on earnings, it is difficult, if not impossible, to assess the merits of the proposed regulations, other policies affecting for-profit students, and the sector as a whole.

This study provides an assessment of college quality in the for-profit sector by estimating the labor market returns, or earnings gains, to associate's degree programs in for-profit colleges. We focus on associate's degree programs due to data restrictions, but also because of the importance of the for-profit sector in sub-baccalaureate education. For-profit colleges account for a disproportionate share of associate's degree enrollment and degree receipt. For-profits enroll 11% of all postsecondary students, yet they confer 21% of all associate's degrees – about 194,000 each year (National Center for Education Statistics (NCES), 2012, Table 219).[1]

A long literature on the returns to education has focused on estimating the earnings gains generated by a year of high school or four-year college. Several studies have also assessed the returns to associate's degrees in public community colleges, but for-profit sub-baccalaureate education has received much less attention in the literature.[2] Our study fills this gap in the literature, attempting to mitigate a crucial endogeneity problem that plagues studies of earnings gains: both observable and unobservable factors may jointly influence a student's decision to attend a for-profit institution and her subsequent earnings. For example, students who pursue a degree at a for-profit college may be more motivated than students who do not enroll in any postsecondary education, but motivation can also independently influence subsequent labor market success. Such endogeneity issues can bias cross-sectional OLS estimates of the impact of for-profit colleges on employment and earnings.

To address this problem we draw on 14 years of data contained in the restricted-access 1997 panel of the National Longitudinal Survey of Youth (NLSY97) to implement an individual fixed effects approach. Unlike bachelor's degree candidates, students pursuing associate's degrees often work before, during, and after they attend college, allowing us to compare an individual student's earnings after attendance to her earnings before. We compare the before–after earnings gains of for-profit associate's degree students to a falsified before–after for high school graduates without college degrees, generating a difference-in-difference estimate of the returns to for-profit college attendance.

The individual fixed effects control for all time-invariant student characteristics that may bias cross-sectional estimates of returns, but time-varying unobservables and dynamic selection issues may still remain. To address such concerns, we include an extensive set of controls including an individual-specific county of residence trend, an indicator for the years in which individuals are enrolled in college, and an indicator to control for pre-education dips in earnings among for-profit students. We conduct many robustness checks to test the sensitivity of our estimates to the inclusion of various controls, different measures of earnings, alternate samples, and heterogeneous effects.

We find that students enrolled in associate's degree programs in for-profit colleges experience a 10% increase in weekly earnings, conditional on employment, in our baseline fixed effects specification. Dividing by 2.6 to account for the average length of enrollment yields a 4% return per year of education – a result that is incredibly robust to various assumptions and specifications. Most of the increase comes from hourly wages rather than more hours worked per week. For-profit students are also slightly more likely to be employed full-time than our control group of high school graduates, but we find no difference in the likelihood of any employment. Unconditional estimates including observations with zero earnings (thereby conflating employment and earnings effects) suggest higher earnings gains of around 18%, or 7% per year. We also find suggestive evidence that students completing associate's degrees in for-profits earn about 20% more than their counterparts who dropout.

Overall, our results suggest that students enrolling in associate's degree programs in for-profit colleges earn positive returns on the order of 4% (conditional on employment) to 7% (unconditional) per year. These estimates are slightly higher than other estimates of for-profit returns (Turner, 2012), but fall below estimates of the returns to public community colleges (Jacobson et al., 2005; Jepsen et al., in press) and traditional four-year colleges (Oreopolous & Petronijevic, 2013) found in the literature. They also fall below the returns needed to offset the private and social costs of for-profit associate's degree attendance (Cellini, 2012), suggesting that for-profits may not be worthwhile for the average student.

The rest of the paper proceeds as follows: Section 2 reviews the literature on the returns to education and Section 3 provides background on for-profit colleges. Section 4 details our estimation strategy. Section 5

[1] Although bachelor's degree and graduate programs are among the fastest growing in the for-profit sector (Deming, Goldin, & Katz, 2012), for-profits still account for just 7% of bachelor's degrees and 9% of graduate degrees conferred (National Center for Education Statistics (NCES), 2012, Table 219). In contrast, for-profits confer 30% of all diplomas and certificates. We cannot examine the returns to diplomas and certificates given data constraints.

[2] We review the literature in detail in Section 2.

describes the data, Section 6 presents results, and Section 7 concludes.

2. Related literature

Over the past half-century, a large literature has developed to measure the returns to schooling. Reviews of the literature by Card (1999, chap. 30) and report that one additional year of education results in earnings gains in the range of 6–9%. More recent and better-identified analyses reveal higher returns for college attendance, averaging 10–15% per year (Card, 2001; Goldin & Katz, 2008; Oreopolous & Petronijevic, 2013). The vast majority of the research in this area has focused on high school and four-year colleges: few studies identify the returns to two-year colleges (Ashenfelter, Harmon, & Oosterbeek, 1999a, 1999b; Oreopolous & Petronijevic, 2013).

Of the handful of studies examining returns to associate's degrees and two-year colleges, most focus exclusively on public community colleges. Reviewing the literature on community college returns, Kane and Rouse (1999) find that a year of community college attendance generates returns between 5 and 8%, just marginally below the average return to a four-year college attendance. Students who complete associate's degrees earn 15–27% more than observationally similar individuals with no postsecondary education.

Central to the literature on returns is a debate over the accuracy of various methods to identify the causal effect of education on earnings. Students who pursue additional education are likely to differ on both observable and unobservable dimensions from those who do not. If these differences are correlated with subsequent earnings, cross-sectional estimates of the returns to schooling will be biased. While a number of studies of high school and four-year college returns have attempted to address this endogeneity problem using natural experiments and sibling comparisons, few studies in the community college literature have implemented similar identification strategies. Many studies estimate cross-sectional models comparing students attending community college to those who do not, generally controlling for ability with proxies, such as IQ scores (for example, Grubb, 1993, 1995; Heineman & Sussna, 1977; Kane & Rouse, 1995; Leigh & Gill 1997; Monk-Turner, 1994; Marcotte, Bailey, Borkoski, & Kienzl, 2005).

Recent studies on the returns to community college education have implemented stronger identification strategies. Jacobson et al. (2005) and Jepsen et al. (in press) use an individual- or person-specific fixed effects approach comparing the wages of students before and after they attend a public community college, thereby controlling for time-invariant individual characteristics that may bias cross-sectional estimates. Among displaced workers in Washington State, Jacobson et al. (2005) find returns of 9% per year of education for men and 13% for women, with much higher returns to quantitative and technically oriented vocational coursework than less-quantitative coursework in the humanities, social sciences, and basic skills. Among all community college students in Kentucky, Jepsen et al. (in press) find higher returns – 56% for an associate's degree for women and 24% for men completing degrees.

We know of very few studies that attempt to assess the returns to for-profit postsecondary education. Most rely on cross-sectional variation in earnings, comparing students who attend for-profit institutions with students who attend other types of postsecondary institutions to estimate the differential returns to the for-profit sector. In a working paper, Chung (2008) instruments for college choice using tuition and concentration of community colleges. Along with Grubb (1993),[3] Chung finds limited evidence of positive effects of for-profit training, particularly for women and certificate programs, but generally shows no significant differences in returns to the for-profit students relative to students in other sectors. Deming et al. (2012), Lang and Weinstein (2012) and Lang and Weinstein (2013)[4] draw on the Beginning Postsecondary Student (BPS) data along with OLS and propensity score methods. Deming et al. (2012) estimate the differential return to for-profits compared to other postsecondary institutions finding that for-profit students (in all types of degree programs) earn about 8% less than observationally similar students in other sectors, but differences are smaller and not significant when conditioning on employment. In contrast, Lang and Weinstein (2013) find that annual income is at least 20% higher for students beginning associate's degrees in for-profits relative to other sectors. They attribute this finding to the fact that more students in public and not-for-profit institutions go on to obtain bachelor's degrees, so students with only an associate's degree in the public and non-profit sector are negatively selected.

In the only other working paper (that we know of) to use panel data, Turner (2012) implements the same identification strategy using tax data. Although he is unable to identify students' degree program in his data, he finds that for-profit college students in two-year institutions earn about a 2% return, conditional on employment. Public and non-profit students fare better, gaining about 6%.

In our study we estimate the impact of for-profit college education on earnings using an individual fixed effects approach, and we use this approach to study the return to for-profit associate's programs specifically. By comparing the before and after wages for the same individual, our fixed effects approach mitigates some of the most critical endogeneity problems that plague cross-sectional studies of the for-profit sector.[5]

[3] Note that a related paper (Grubb, 1993b), using the same data and methods to estimate returns to community colleges was found to be severely flawed by Kane and Rouse (1995b).

[4] Lang and Weinstein (2013) is an update of their 2012 working paper using BPS transcript data, rather than BPS survey data. We report results from the 2013 version.

[5] This approach has also been used in the broader labor economics literature. For example, Ashenfelter (1978) and Angrist and Newey (1991) use individual fixed effects to assess the impact of job training programs on earnings. Angrist and Newey (1991) and Freeman (1984) examine the impact of union status on earnings. See Angrist and Kreuger (1999, chap. 23) for an overview of the fixed effects strategy in labor economics.

3. Background

Our paper estimates the returns for students enrolled in associate's degree programs in for-profit institutions. Associate's degree programs typically require two years of full-time coursework and result in the attainment of an Associate of Arts (AA) or Associate of Science (AS) degree.[6] Students may obtain their degree in any number of majors, including traditional liberal arts and science majors like history, psychology, or computer science, as well as more vocational fields such as medical assisting, paralegal studies, or homeland security.

For-profit institutions that award associate's degrees are classified as two-year or four-year institutions by the U.S. Department of Education, based on the highest degree that they offer. For example, many of the large for-profit chains such as University of Phoenix or DeVry University offer both associate's and bachelor's degree programs, designating them as four-year institutions. Two-year colleges offer associate's degrees as their highest degrees, such as the Heald College chain along with many smaller local colleges. Interestingly, of the roughly 194,000 students obtaining associate's degrees from for-profits, 65% of students receive them from institutions that also offer bachelor's degrees (National Center for Education Statistics (NCES), 2012). We include students pursuing associate's degrees in both sets of institutions in our analysis.[7]

As noted above, for-profits account for 21% of the associate's degrees conferred, while the vast majority of associate's degrees are conferred by public community colleges (74%). In contrast to the market for bachelor's degrees where not-for-profits dominate the private sector, not-for-profit institutions confer just 5% of associate's degree (National Center for Education Statistics (NCES), 2012). In this analysis, we drop students pursuing associate's degrees in public and non-profit institutions and focus exclusively on generating the absolute return to enrollment in a for-profit institution relative to students who graduate from high school, but do not receive any college degree.[8]

Research on for-profit colleges is scarce, in part due to the fact that, until recently, very few publicly available data sources included for-profit institutions and students. Most studies of for-profit colleges examine only federal aid-eligible for-profits that participate in the U.S. Department of Education's Integrated Postsecondary Education Data System (IPEDS) and most are descriptive in nature (Apling, 1993; Bailey, Badway, & Gumport, 2001; Chung, 2012; Deming et al., 2012; Rosenbaum, Deil-Amen, & Person, 2006; Turner, 2006, chap. 3).[9] Administrative licensing data has added to our knowledge of these institutions in recent years and allowed for causal studies of institutional behavior (Cellini, 2009, 2010) and a more accurate count of institutions (Cellini & Goldin, in press), but in spite of these advances surprisingly little is known about for-profit colleges and their students.

What we have learned from these studies and data sources is that there are approximately 7500 for-profit institutions in the Unites States, at any level i.e., two-year, four-year, or less-than-two-year, about 3000 of which are eligible for federal student aid programs (Cellini & Goldin, in press). Due to the nature of our data, we focus on aid-eligible institutions in this analysis. Average enrollment in aid-eligible for-profits is just 500 students, a figure that pales in comparison to public community colleges that average about 7000 students each nationwide (National Center for Education Statistics (NCES), 2012).

Research on the sub-baccalaureate market has emphasized the similarities and differences between for-profits and community colleges. Cellini (2009) shows that for-profits and community colleges compete for students: when funding for community colleges increases, for-profit colleges exit the market. Similarly, Chung (2012) finds that community college tuition positively impacts the probability of attending a for-profit college. Both sectors also offer a wide range of overlapping degree programs (Cellini, 2009), but community colleges tend to offer more academic and liberal arts programs and for-profits often specialize in vocational fields in which skills are easy to verify and physical plant requirements are modest (Turner, 2006, chap. 3). Both offer a large number of part-time, evening, and online programs to meet the needs of working students (Rosenbaum, Deil-Amen, & Person, 2006), but some notable differences in student characteristics remain.

Deming et al. (2012) report that, relative to community colleges, for-profit institutions (including aid-eligible two-year and four-year institutions) enroll a higher proportion of women (65 versus 57%), blacks (22 versus 14%), GED recipients (17 versus 10%), and single parents (29 versus 12%). Income differences are also substantial: the average income of a for-profit student is roughly $15,000–20,000 less than a community college student. The most important difference between for-profit and public two-year colleges is undoubtedly their price: required tuition and fees for public community colleges average just $2650 for in-state students, while for-profit two-year colleges charge more than five times as much, averaging $14,000

[6] We cannot differentiate between these degrees in our data.

[7] We find no differences between the returns to associate's degrees in two- versus four-year for-profit institutions, see Table 5.

[8] A previous version of this paper emphasized estimates of the differential return to for-profits relative to public community colleges. Since we found we had little power to detect differences between the sectors, in this paper, we focus more specifically on the absolute returns to for-profits relative to high school graduates with no college degree. However, we include an estimate of the differential effect of public sector institutions as a robustness check in Table 5.

[9] The IPEDS undercounts the total number of for-profit colleges in the U.S. because it excludes those that do not participate in federal student aid programs. In fact, for many years the IPEDS did not accurately track nor require the participation of any for-profit institutions. Since about 2000, all institutions participating in federal student aid programs have been required to complete the IPEDS survey, but for-profits that do not participate in federal aid programs are still not required to participate and remain largely unaccounted for in the data. Only about 200 of an estimated 4500 non-aid-eligible institution voluntarily respond to the IPEDS (see Cellini & Goldin, in press). Further, these institutions are typically not assigned an identification number that allows merging onto other data sources, such as the NLYS97, used here.

in 2011–2012 (National Center for Education Statistics (NCES), 2012, Table 381).[10]

Of course, students at for-profit institutions receive a disproportionate share of federal student aid to pay for their education. For-profit students account for 24% of all federal Pell grant aid, 26% of student loan disbursements, and 36% of Post 9–11 GI Bill benefits, while enrolling just 11% of all students (Deming et al., 2012). In two-year for-profit colleges, a full 90% of students receive some kind of aid. Of those who receive aid, for-profit two-year college students average $4350 in federal grants and $7750 in federal loans (National Center for Education Statistics (NCES), 2012). Student loan default rates are remarkably high in the for-profit sector: two-year cohort default rates are 12.5%, while the 10-year cumulative rate (based on number of loans rather than students) is 35% (U.S. Department of Education, 2011).

In light of the financial burden placed on both students and taxpayer, we should expect that the earnings gains from attendance would be positive for the average student. Whether the gain is enough to offset the high cost of attendance is an issue we explore in our conclusions below.

4. Empirical methods

Many studies of the returns to education estimate cross-sectional regression models following Mincer (1974). The main concern with such estimates is the potential endogeneity of attendance because individuals more likely to earn higher earnings may also be more likely to attend college, resulting in biased estimates of the returns to education.

To address this endogeneity problem, our research design exploits the panel structure of the NLSY97 data. Unlike high school and traditional four-year college students, associate's degree students (and particularly those in for-profit colleges) often work before, during, and after attendance. We can therefore add individual-level fixed effects to the Mincer model, exploiting the variation in earnings for the same individual before and after she attends a for-profit college. This strategy effectively controls for time-invariant unobserved heterogeneity at the student level, though other types of selection may remain (we return to this point below).

Our treatment group is the sample of for-profit college students who report working towards or receiving an associate's degree in our period of study (1997–2010). Our main estimates therefore reflect the return to for-profit attendance, rather than degree completion. They can be interpreted as the average gain in earnings that students experience after enrolling in a for-profit institution regardless of whether or not they graduate. We believe these are the most useful estimates for policy analysis, since they indicate the average outcome we can expect for a student who enrolls in a for-profit associate's degree program. As a robustness check, we also estimate the earnings gains to degree completion. However, it is important to note that graduates are a select sample of for-profit students and are likely not representative of the average student's experience. Further, completion is an outcome and is therefore endogenous. That is, many factors may jointly influence whether an individual completes their associate's degree at a for-profit college and their subsequent earnings, making it difficult to discern causal effects for this group.

We construct a control group that includes high school graduates who never report working toward an associate's degree in the time period we observe to better identify the absolute returns to for-profit attendance beyond high school. We construct a falsified post-education variable for this "high school only" group.[11] Therefore, our estimates of returns to for-profit colleges can be thought of as difference-in-difference estimates: the first difference is between high school graduates with no college degree and for-profit students, and the second difference is the pre- versus post-education (or falsified post-education) earnings for each individual.

We estimate log weekly earnings and other employment outcomes, y_{ict}, for individual i in county c and year t as a function of for-profit college attendance and individual characteristics as follows:

$$y_{ict} = \beta_0 + \beta_1(Post_{it}) + \beta_2(Post_{it} * ForProfit_i) + \gamma X_{it} + \delta_{ct} + \tau_t + \eta_i + \varepsilon_{ict}$$

The variable $Post_{it}$ identifies the post-education time period for each student. It switches from 0 to 1 the year after an individual reports receiving an associate's degree (for degree completers) and the year after an individual last reports working towards an associate's degree (for dropouts). It then remains 1 in all subsequent years. For the high school only group the falsified $Post_{it}$ is based on age, as described in the next section. We interact this variable with $ForProfit_i$, an indicator for whether the individual attended a for-profit college. The result is that the variable of interest $Post_{it} * ForProfit_i$ equals 1 for for-profit college students in each year after attendance or degree completion and 0 otherwise.

To control for time-varying factors that may jointly influence for-profit college attendance and earnings, we include a variety of controls in X_{it}. Specifically, we add age fixed effects, interactions of the age fixed effects with both race and gender, an indicator for the years in which individuals are enrolled in college, and an indicator for the two years before college for the for-profit students. We discuss the reasons for each of these controls below. We also include a time trend for each individual's county of residence, δ_{ct}, to proxy for changes in local labor market conditions and for-profit college supply. We include year fixed effects, τ_t, to capture changes in the macro-economic climate influencing all individuals in the same manner.

Most importantly, we add indicators for each individual, η_i. These individual fixed effects control for time-invariant unobservable characteristics that are correlated both with earnings and the decision to attend a for-profit

[10] The average for for-profits include only federal aid-eligible institutions of the type we examine here. For-profits that are not aid eligible charge about 78% less (Cellini & Goldin, in press).

[11] We refer to this group as both the "high school only" and "high school graduates" group throughout.

institution. In the case of for-profit associate's degree students, a student's innate ability and motivation are the primary omitted variables we should be concerned about. To the extent that these do not change over time, our fixed effects will provide adequate controls for these potentially confounding omitted variables. The key identifying assumption of the fixed effects approach is that conditional on observables, nothing else that affects earnings changes contemporaneously with college attendance. However, this assumption can breakdown and bias estimates of the returns to a for-profit education in two primary ways.

First, our sample consists of young workers that may exhibit changing labor force attachment over the age-range we observe. We deal with this issue in several ways. Our county-year trends and the controls interacting age fixed effects with race and sex help mitigate this bias, allowing for flexible trends in earnings for different groups. Our indicator for the years in college accounts for the opportunity cost of attendance, since students likely need to cut back on hours or give up working completely while in college. We also restrict our analysis to person-year observations over age 18 to avoid capturing earnings from informal jobs such as babysitting. Finally, as a robustness check, we limit the sample to individuals with valid earnings data in the pre-period, as is common in the job-training literature.

Second, we worry about biases associated with the dynamic selection of individuals into college. In particular, estimates of returns may be biased upwards if individuals experience a decline in earnings in the years immediately preceding enrollment. This issue, sometimes referred to as "Ashenfelter's dip," is well-known in the job training literature, as individuals with negative earnings shocks are more likely to enroll in training than individuals in untreated control groups.[12] This type of dip would cause our estimate of the returns to for-profits to overstate the true gain. To address such concerns, we include an indicator variable for the two years before attendance for the for-profit students.

5. Data

To implement our analysis, we draw on the restricted-access 1997 panel of the National Longitudinal Survey of Youth (NLSY97), a major nationally representative longitudinal survey that tracks a cohort of students through secondary school, college, and beyond. The NLSY97 is based on a panel of 8984 youths who were 12–18 years old when they were first surveyed in 1997.[13] The youths are interviewed each year and we use data available through 2010. We thus have a group of individuals ranging from age 25 to 31 by 2010.

The NLSY97 has both advantages and disadvantages relative to other datasets. On the down side, it is a young and relatively small cohort of individuals. Less than 1% of the individuals are in their 30s by 2010 and we have a small sample of individuals to draw from who enroll in associate's degree programs in for-profit colleges. Many for-profit colleges offer certificates and diplomas, but these are not well documented in the education module of the NLSY97 so we are unable to estimate the returns to non-degree programs in for-profit colleges.[14]

On the up side, the NLSY97 is a panel with rich information on the education and earnings trajectory of individuals. Most importantly, it surveys individuals annually for 14 years, allowing us to implement an individual fixed effects estimation that gets closer than OLS and propensity score estimates to identifying the causal effect of education on earnings, as described above. But it has several other advantages as well. It contains detailed data on weekly wages and hours for multiple jobs each year, allowing us a more accurate estimate of earning gains than many other surveys. The restricted access data also contains county of residence in each year and allows us to merge the NLSY97 with the IPEDS to accurately identify an individual's college. Overall, we believe the panel structure and detailed information on education and earnings outweigh the disadvantages of the NLSY97 for this type of analysis.

Our main sample is restricted to individuals who report working towards or receiving an associate's degree at a for-profit college (the treatment group) and a control group of individuals who have graduated from high school but never report working towards or receiving an associate's degree. We drop individuals who received a bachelor's degree or have completed 16 or more years of education from both groups, since we observe very few post-education years for these individuals. However, we leave in students who at some point may report working towards (but not obtaining) a bachelor's degree, as a sizable proportion of students at for-profit colleges who on occasion state they are working towards a bachelor's degree also report pursuing or obtaining an associate's degree.[15] Since students who drop out of traditional bachelor's degree programs in not-for-profit or public institutions might be positively selected vis-à-vis for-profit students, we perform robustness checks dropping these students from the high school only group. Our final sample includes 388 for-profit students and 2303 individuals in the high school only control group.

We focus on associate's degree students in the for-profit sector for several reasons. First, as noted above, for-profits offer a disproportionate share of associate's degrees, so focusing on these students allows us a large enough sample size to identify absolute returns. Second, the individual fixed effects approach requires earnings before and after attendance, and most students in associate's degree programs work before attending college, but this is not typically the case with "traditional" bachelor's degree students. We observe earnings for 72% of our for-profit

[12] See Ashenfelter (1978), Ashenfelter and Card (1985), and Heckman and Hotz (1989) for more discussion.

[13] According to the Bureau of Labor Statistics website, individuals surveyed in the NLSY97 were 12–16 years of age as of December 31, 1996. We report the age in the survey year. For example, a majority of the individuals (94%) are documented as 12–16 in the 1997 survey year, 6% are 17 years old and 12 individuals are 18 years old.

[14] The NLSY97 collects information on certificates as part of the job training module, but we do not believe these can be matched to IPEDS college codes.

[15] About 40% of for-profit associate's degree students also report working toward a bachelor's degree at some point in time. We believe that many of these students aspire to a bachelor's degree, but end up getting an associate's degree and then leave college.

associate's students in the pre-education period and 77% in the post-education period.

5.1. Measuring for-profit attendance

The two key variables in our analysis are indicators for post-education and whether an individual attended a for-profit college. To construct a consistent measure of post-education, we code $Post_{it}$ as 1 only when we are certain that individuals are out of college. In the case of associate's degree graduates, $Post_{it}$ switches to 1 the year after they report receiving their degree and remains 1 for all subsequent years.[16] For dropouts (i.e., attendees who do not complete a degree in the time period we observe), we use self-reported information on whether they are "working towards an associate's degree" to code post-education. 76% of dropouts at for-profit colleges report working towards their associate's degree for a few years and never report working towards an associate's degree again. For this group, we switch $Post_{it}$ to 1 the year following the last time they report working towards an associate's degree.

We also observe individuals who work towards an associate's degree for a few years, then drop out (i.e., do not report working towards an associate's degree) and report working towards an associate' degree again after a few years before dropping out again. For these "double dropouts", we switch $Post_{it}$ to 1 the year following the last observed enrollment and all the years before are treated as 0.[17] Finally, we have 47 individuals attending for-profit colleges who are still working towards their associate's degree in 2010. We leave these individuals in the sample but they do not contribute to identification.

To identify students attending a for-profit institution we use the sector of the college as listed in the IPEDS. We separately identify two-year and four-year for-profits as a robustness check. Since colleges are listed every year, for degree completers, we use the IPEDS college code in the year that they graduate to identify them as for-profit students. For dropouts, we use the college listed in the last observed year of enrollment.[18]

For the high school only group, we create a falsified $Post_{it}$ that turns on at age 23, which is the average age of students in the NLSY97 the first year after their last observed enrollment in an associate's degree program (for dropouts) and the first year after they complete an associate's degree (for graduates).[19] We also create a falsified in-school variable assuming the high school only group are in college for two years before their post-education period begins.

5.2. Dependent variables and controls

Following the returns literature (e.g., Angrist & Kreuger, 1991), our main dependent variable is the natural log of weekly earnings, where weekly earnings are calculated as the product of the individual's hourly wage and average hours worked per week. In the NLSY97, individuals report their wages and hours for up to eleven jobs in a year, but the number of people reporting wages for more than five jobs is less than 1%. Hence, we focus on average weekly earnings across the first five jobs.[20] There are a few clear outliers in the average wage and hours worked per week variables that we use to calculate weekly earnings. We drop person-year observations where the average hourly wage across the first five jobs is less than $1 or greater than $100, and where the hours worked per week is greater than 160.[21]

As noted above, we also limit our analysis to person-year observations where individuals are 18 years or older to avoid capturing wages in informal early jobs.[22] Since the NLSY97 is a young cohort, we observe earnings for 3.4 years on average in the post-education period (3.6 years for for-profit students and 3.4 years for high school graduates). Our estimates of returns are thus estimates of the short run returns to for-profit colleges and may underestimate returns if earnings are more responsive in the long run.

Using the natural log of weekly earnings as the dependent variable has some advantages and drawbacks. First, observations with zero or missing earnings data are dropped from the analysis: our estimates therefore reflect returns conditional on employment. These estimates are comparable to previous estimates of returns in other sectors and can be interpreted as percentage changes, however, they do not capture whether or not for-profit students are more likely to be employed than high school graduates with no college.

[16] The NLSY97 has many variables to identify degree completion. We use the most inclusive set of variables (highest degree received, highest degree received ever, and type of degree received since date of last interview) to identify degree completion. Our results are robust to using only a subset of these variables to identify degree completion. We also create an indicator variable for years in college based on self-reported information. Ten percent of for-profit students who eventually obtain an associate's degree never report working towards an associate's degree. For this group, we assume they were in college for two years before receiving their degree.

[17] Among dropouts, 17% of individuals attending for-profits dropout twice and 2% dropout three times.

[18] The NLSY97 lists up to five colleges per year per student, but the vast majority (85%) of students list only one in the year of graduation or of last observed enrollment. As long as at least one college is for-profit in the year of graduation or the year of last observed enrollment, we code the student as for-profit. We drop students who also report attending institutions in the public sector in the same year as a robustness check in Table 5. Finally, for a few individuals, the IPEDS code is missing in the year of graduation or the year of last observed enrollment. We use the IPEDS code from the previous year to code for-profit in these instances.

[19] The comparable average age of post-education for the sample of for-profit students is 23.7 and our results are robust to using this age for the high school only group. However, we prefer to use the average age of all associate's degree students (23) due to concerns about endogeneity.

[20] The results are robust to alternate definitions of weekly earnings based on the first job alone and all reported jobs. We show the latter result in Table 5.

[21] The results are robust to keeping these outliers in the analysis. The $1 average wage cut-off is below the 1 percentile of the wage distribution ($2.08) and $100 is above the 99 percentile of the wage distribution ($62.5). Since there are 168 total hours in a week, reports of individuals working more than 160 are also clear outliers.

[22] Our results are robust to including individuals 16 and over.

To assess employment outcomes and to better understand our estimates of returns, we therefore also examine several other labor market outcomes. We first decompose weekly earnings to estimate the effects of for-profit attendance on log hourly wages and log hours worked per week across the first five jobs. We also estimate the effects of for-profit attendance on full-time employment and any employment, this time including observations with zero earnings.[23] Our measure of full-time employment equals one if an individual reports working 35 or more hours per week, and our indicator for any employment equals one if an individual reports non-zero weekly earnings. Finally, we report unconditional estimates of returns, using weekly earnings in dollars as the dependent variable and again including observations with zero earnings. These estimates combine both the earnings gains and employment effects of for-profit attendance.

As noted above, in addition to individual fixed effects, we include age fixed effects, calendar year fixed effects, a dummy for when individuals are in school and flexible controls for observable characteristics by interacting age fixed effects with dummies for both race and sex. We also interact the 1997 county of residence from the geo-coded portion of the NLSY97 (restricted access) with time trends to control for changing local labor market conditions. We believe the 1997 county of residence, the first year of the survey, is the most exogenous measure of location. As a robustness check, we also interact the county of residence in the year before an individual begins college.[24]

5.3. Summary statistics

Table 1 reports the summary statistics for the main variables used in our analysis. We present the means separately for the for-profit sample and for the high school only group. Our for-profit sample consists of 388 individuals of which 43% received an associate's degree by 2010. On average, for-profit students are more likely to be female (57%) and non-white (63%), as found in previous research (Deming et al., 2012).[25] The average weekly earnings during the entire time period are slightly lower for the for-profit sample compared to our high school sample ($371 versus $384), but this is probably related to the larger proportion of males in the latter group. Although employment rates are similar for both groups, individuals from for-profits are less likely to be employed full time over the 1997–2010 period at 49% compared to 54% for high school graduates.

[23] The NLSY97 codes five types of missing variables – refusals, do not know, invalid skips, valid skips, and non-interviews. In the NLSY97, if a person is not working, they are coded as a valid skip. We therefore recode observations with valid skips in both wages and hours as having zero earnings. In our baseline sample, 8134 person-year observations (30%) are missing of which 3072 (11%) are valid skips.

[24] Dropping the county-year trend does not affect our results, as shown in Table 2, column (3).

[25] We constructed the race variable incorporating information on both race and ethnicity from the NLSY97.

Table 1
Summary statistics, NLSY 1997.

Variable	For-profit students		HS graduates	
	Mean	Std dev	Mean	Std dev
Weekly earnings ($)	$371.23	$276.68	$384.43	$278.30
Hourly wage	$10.94	$7.16	$11.14	$7.48
Hours worked per week	33.8	10.0	34.7	10.5
Full time employment	49.1%	50.0%	53.7%	49.9%
Any employment	87.4%	33.2%	86.1%	34.6%
Age	22.7	3.1	22.8	3.1
Male	42.9%	49.5%	58.3%	49.3%
White (non-Hispanic)	37.3%	48.4%	46.1%	49.8%
Black (non-Hispanic)	36.5%	48.1%	29.1%	45.4%
Asian	0.7%	8.4%	0.9%	9.4%
Hispanic	10.8%	31.1%	11.5%	31.9%
Other	14.7%	35.4%	12.4%	33.0%
Person-year obs.	3964	3964	23,534	23,534
Number of individuals	388		2303	

Note: Statistics are person-year averages from 1997 to 2010 for individuals age 18 and older. Weekly earnings, average hourly wage, and hours worked per week are conditional on employment (i.e., zeros not included). Any employment and full time employment count individuals with zero earnings as unemployed.

To visually examine the time patterns of earnings, we graph the raw mean of log weekly earnings for each group five years before and after post-education in Fig. 1. Year 0 is the first post-education year and on average, students enter college 2.6 years earlier, where the dashed line appears. Note that for-profit students appear to have only a very slight advantage over high school graduates in the post-education period. More noticeable is the dip in earnings for for-profit students prior to enrollment that does not appear to occur for the high school group. As noted above, we include controls for the two years prior to enrollment to control for this difference. We next turn to the results to see if these visual post-education gains stand up to regression analysis.

6. Results

6.1. Main results

Table 2 presents our first set of findings on log weekly earnings. Specifications (1) and (2) are cross-sectional OLS models common in the literature, while specifications (3) through (5) are our preferred estimates, exploiting within-individual variation by including individual fixed effects.

The first row of Table 2 finds small and statistically insignificant effects of post-education ($Post_{it}$) in every specification. This result is expected because $Post_{it}$ is falsified for the high school only group, so they should see no jump in earnings at age 23 in the absence of further education. More importantly, in the second row of Table 2 we find a significant effect of for-profit college attendance on weekly earnings across all specifications. Cross-sectional OLS estimates in specification (1) suggest a significant return to for-profit attendance of 13.6% over

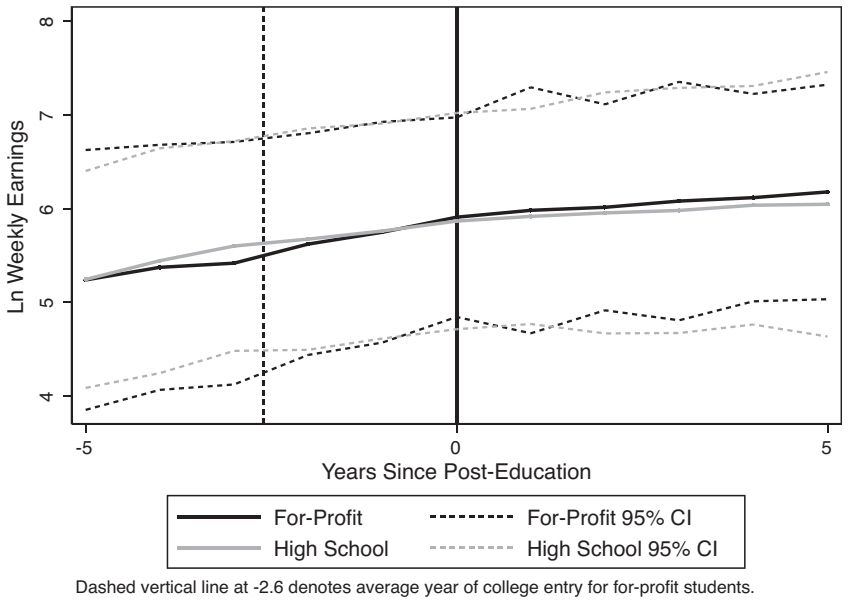

Fig. 1. Unadjusted log weekly earnings over time.

Table 2
Returns to for-profit attendance, log weekly earnings.

	(1)	(2)	(3)	(4)	(5)
Post-Education	0.026	0.021	−0.010	−0.011	−0.012
	[0.035]	[0.035]	[0.033]	[0.033]	[0.033]
Post-Educ * For-Profit	0.136***	0.132***	0.110***	0.104***	0.103***
	[0.036]	[0.036]	[0.035]	[0.036]	[0.037]
Demographics	Yes	Yes	Yes	Yes	Yes
County of residence trend	No	Yes	No	Yes	Yes
Individual FE	No	No	Yes	Yes	Yes
Ashenfelter's Dip	No	No	No	No	Yes
Observations	19,364	19,364	19,364	19,364	19,364
Individuals			2627	2627	2627

Robust standard errors clustered at the individual level in brackets. *Note*: Post-education is an indicator variable that turns on at age 23 for the high school group (falsified post), the year after graduation for the for-profit degree completers and the year after the last observed enrollment for the for-profit dropouts. For-Profit is an indicator variable for individuals who attended an associate's degree program or received an associate's degree from a for-profit college. Demographics include age fixed effects, year fixed effects, age fixed effects * race, age fixed effects * male, and an indicator for the years when individuals are in college. County of residence is an individual's county of residence in 1997, the first year of the survey. Ashenfelter's dip is an indicator variable for the two-years before for-profit students begin college.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

high school graduates.[26] As we add more controls (specification 2) and the individual fixed effects (specifications 3–5), the coefficients on $Post_{it} * ForProfit_i$ decrease slightly in magnitude but not statistical significance. Our preferred fully loaded individual fixed effects specification (5), which includes both the county of residence trends and the pre-education indicator finds a statistically significant 10.3% return to for-profit college attendance relative to high school graduates with no college degree.

Since the for-profit associate's degree students in our sample attend college for an average of 2.6 years, dividing our estimated returns by this figure yields a measure of returns to a single year of education that can be more easily compared to previous studies. Based on this calculation, for-profit associate's degree programs yield a 4% increase in earnings per year of education. This estimate is slightly higher than that found by Turner (2012) for for-profit

[26] We report our findings as percentages, but they are actually log points. To convert log points to percentages use (elog points) − 1. But note that this transformation has virtually no impact on the interpretation for log points below 0.30 (as we report here) so, for simplicity, we describe them as percentages throughout.

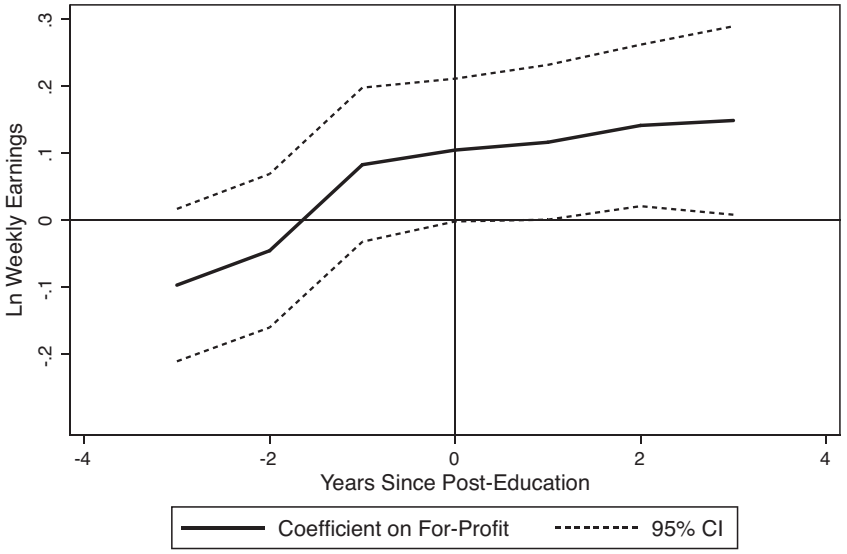

Fig. 2. Dynamic returns to for-profit attendance.

two-year college attendance (2% per year),[27] and lower than the returns to a year of public community college attendance reported by Jacobson et al. (2005) (9–13% per year). Our estimates remain even slightly below the 5–8% range found for public community college students in cross-sectional studies in the 1980s and 1990s as reported by Kane and Rouse (1999).

To explore the dynamic pattern of earnings gains in more detail, we implement a type of event-study analysis. We add interactions of *ForProfit* with each pre- and post-education year within a three-year window and plot the coefficients and confidence intervals in Fig. 2.[28] As expected, our results are significant only in the post period (0 is the first post-education year) and there is a slight growth in earnings over time after attendance. In the fourth year post-education, earnings gains grow to 15%, or about a 5.8% return per year.

In Table 3, we explore the effect of for-profit enrollment on employment and other labor market outcomes using our preferred fully loaded individual fixed effects specification (specification (5) in Table 2). For comparison, we show the results on log weekly earnings in specification (1). The results in specifications (2) and (3) suggest that the positive returns to for-profit enrollment are driven mostly by higher hourly wages (6.8% higher than high school graduates) rather than higher hours worked per week (3.6% higher than high school graduates but statistically insignificant). Specifications (4) and (5) are linear probability models for employment, now including observations with zero earnings. We find that attending a for-profit college increases the probability of full-time employment relative to high school graduates by 6.4 percentage points. In specification (5), we also find a marginally significant positive effect of for-profit college attendance on any employment, but this effect is only significant at the 10% level.

Finally, in specification (6), we analyze unconditional weekly earnings by moving away from the log specification and including the zeros in weekly earnings. The coefficient on *Post * ForProfit* suggests for-profit students earn $58 per week more than high school graduates, an increase of 17.6% over the average weekly earnings of for-profit students.[29] Dividing by 2.6, the unconditional return to a year of education in a for-profit college is about 7%. Since the unconditional returns conflate employment outcomes and earnings gains, it is perhaps unsurprising that we find slightly larger returns to unconditional earnings than conditional returns since for-profit college attendance may increase the probability of full time employment, as shown in column (4).

6.2. Robustness checks

Table 4 reports the results of several alternate specifications and robustness checks. In our main specifications (Table 3) we dropped person-year observations where average hourly wages across the first five jobs were less than $1 or greater than $100. While it is possible that individuals in the NLSY97 did earn these average wages,

[27] The difference might be explained by the fact that Turner's sample includes students pursuing certificates and diplomas as well as associate's degrees in two-year institutions.

[28] This regression includes age FE, year FE, age FE * race, age FE * male, individual FE, county of residence trends, and an indicator for the two years before for-profit students enroll in college. Instead of an indicator for when students are in college, we include the main effects for each pre- and post-education year within the three-year window and their interactions with for-profit.

[29] Average weekly earnings for the for-profit students are $330.

Table 3
Effects of for-profit college attendance on labor market outcomes.

	(1) Log Wkly Earn	(2) Log Hrly Wages	(3) Log Hrs/Week	(4) FT Employ	(5) Any Employ	(6) Wkly Earn ($)
Post-Education	−0.012	0.012	−0.022	−0.013	0.014	−3.736
	[0.033]	[0.025]	[0.022]	[0.029]	[0.020]	[13.851]
Post-Educ * For-Profit	0.103***	0.068**	0.036	0.064**	0.034*	58.006***
	[0.037]	[0.027]	[0.026]	[0.029]	[0.019]	[15.667]
Observations	19,364	19,364	19,364	22,436	22,436	22,436
Individuals	2627	2627	2627	2672	2672	2672

Robust standard errors clustered at the individual level in brackets.
Note: All regressions include age fixed effects, year fixed effects, age fixed effects * race, age fixed effects * male, an indicator for the years when individuals are in college, a county of residence trend, and an indicator for the two years before for-profit students begin college. Specifications (4) and (5) are linear probability models that include individuals with valid zero earnings. In specification (6) the dependent variable is the unconditional weekly earnings (in dollars), including individuals with valid zero earnings. See Table 2 and the text for descriptions of the Post-Education and Post-Education * For-Profit variables.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Table 4
Robustness checks, log weekly earnings.

	(1)	(2)	(3)	(4)	(5)	(6)
Post-Education	−0.012	−0.022	−0.012	−0.040	0.016	−0.004
	[0.033]	[0.033]	[0.033]	[0.046]	[0.036]	[0.033]
Post-Educ * For-Profit	0.103***	0.111***	0.104***	0.104**	0.121***	0.097***
	[0.037]	[0.036]	[0.037]	[0.049]	[0.036]	[0.037]
Notes	Preferred FE specification (Table 3)	Dropping wages below 1 percentile and above 99 percentile	Including weekly earnings across all reported jobs, rather than 5 jobs	Restricting to individuals employed in the 3 consecutive years before enrollment	Redefining *Post* to switch to 1 after first dropout, rather than last dropout	Using county of residence in the year before enrollment in county trend, rather than 1997
Observations	19,364	19,240	19,364	12,567	19,364	19,364
Individuals	2627	2624	2627	1404	2627	2627

Robust standard errors clustered at the individual level in brackets.
Note: All regressions include age fixed effects, year fixed effects, age fixed effects * race, age fixed effects * male, an indicator for the years when individuals are in college, a county of residence trend, and an indicator for the two years before for-profit students begin college. See Table 2 and the text for descriptions of the Post-Education and Post-Education * For-Profit variables. Please refer to the text and notes below each specification for specific changes.
* $p < 0.1$
** $p < 0.05$.
*** $p < 0.01$.

these cut-offs are below the 1st percentile and above the 99th percentile of the average hourly wage distribution for this sample. So, in specification (2) of Table 4, we drop person-year observations where the average hourly wage is below the 1st percentile ($2.08) and above the 99th percentile ($62.5) of the distribution. For comparison we report the estimates from our preferred fully loaded FE in specification 1 (from Table 2, specification 5). The results in specification (2) are only slightly higher indicating a return of 11.1% to for-profit colleges.[30]

Specification (3) uses earnings from all reported jobs (up to eleven each year), rather than the first five. The results are very close to our baseline, at 10.4%. We find similar results when we rely only on the first reported job for our measure of earnings (not reported).

As noted above, our baseline estimates include several controls to address potential biases created by the changing labor force attachment of young workers. We add one additional robustness check here. Specification (4) uses the presence of non-missing earnings data in the pre-education period as a selection criterion. We restrict the sample to individuals who were employed in each of the three consecutive years prior to enrolling in college. Despite losing 1223 individuals, our estimates remain very similar to the full sample, suggesting that for-profit students earn 10.4% over high school graduates.

A small proportion of our for-profit sample consists of individuals who dropout and re-enroll multiple times. In our baseline specification, our definition of post-education

[30] Our estimates are also robust to including the outliers.

Table 5
Heterogeneous effects and alternate samples, log weekly earnings.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-Education	−0.012	−0.012	0.013	−0.02	0.009	−0.028	−0.018
	[0.033]	[0.033]	[0.034]	[0.036]	[0.036]	[0.034]	[0.018]
Post-Educ * For-Profit	0.103***	0.097**	0.019	0.075**	0.135***	0.122***	
	[0.037]	[0.049]	[0.040]	[0.038]	[0.038]	[0.038]	
Post-Educ * For-Profit* 4-year FP		0.012					
		[0.063]					
Post-Educ * For-Profit* AA Degree			0.198***				
			[0.045]				
Post-Educ * Any College							0.088***
							[0.020]
Post-Educ * Any College* For-Profit							0.026
							[0.035]
Notes	Preferred FE specification (Table 3)	Interaction: 4-year For-Profit	Interaction: AA degree completion	Dropping individuals who also report attending a public college	Dropping double dropouts	Dropping individuals who report working towards a BA in HS group	Adding public college AA attendees
Observations	19,364	19,364	19,364	19,022	19,005	16,300	33,502
Individuals	2627	2627	2627	2581	2585	2175	4459

Robust standard errors clustered at the individual level in brackets.
Note: All regressions include age fixed effects, year fixed effects, age fixed effects * race, age fixed effects * male, an indicator for the years when individuals are in college, a county of residence trend, and an indicator for the two years before for-profit students begin college. See Table 2 and the text for desciptions of the Post-Education and Post-Education * For-Profit variables. For-Profit * 4-year is an indicator for for-profit students who attended or completed an associate's degree program in a four-year for-profit college. AA degree is an indicator for individuals who complete an associate's degree in a for-profit college. Any college is an indicator for individuals who attended or completed an associate's degree program in either a for-profit or public college.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

switches to 1 after the last observed dropout for these individuals, but this may be too restrictive if these individuals enroll for just a few weeks after the first or second dropout before deciding to quit again. In specification (5) we use an alternate definition of post-education for "double dropouts" that switches to one when we first observe them drop out. For associate's degree holders and individuals who only dropout once, *Post* is defined as before. The coefficient is marginally bigger using this alternate definition of post-education suggesting for-profits earn 12% over high school graduates.

Finally, specification (6) uses the county of residence in the year before individuals enroll in college to construct the county of residence trend, rather than the county in 1997 as in our baseline specification. Unlike the 1997 county of residence, which is typically a parental decision, county of residence immediately prior to enrollment may be endogenous to the schooling decision. On the other hand, it may better capture changes in local labor markets relevant to young adults. The estimated returns to for-profits are robust to this alternate county of residence trend at 9.7%.

6.3. Heterogeneous effects and alternate samples

In Table 5, we test for heterogeneous effects and estimate the returns to for-profit college attendance using several different samples. Again, we report the estimates from our preferred FE specification in column 1 for comparison. As noted in the background section, individuals can pursue an associate's degree in either a two-year or four-year for-profit institution. In our sample, 53% of individuals attend or receive their associate's degree from a four-year for-profit institution. We find no evidence of a differential return to four-year for-profit colleges in specification (2). The coefficient on the four-year interaction is small in magnitude and statistically insignificant. [31]

In all of our prior estimates, we combine individuals who complete associate's degrees with those who do not because degree completion is endogenous. However, the returns to attendance are likely higher for completers than for dropouts. We therefore test for differential effects by degree completion in specification (3) in Table 5. We find a large, positive and significant effect of completing an associate's degree in a for-profit college on subsequent earnings on the order of 20% over dropouts. Given the potential endogeneity of completion in our model we hesitate to overemphasize the finding, but future research into for-profit college quality should carefully explore the margin of completion versus attendance at for-profit colleges.[32]

[31] We conduct a similar exercise to test for a differential effect by gender (not reported). We find no significant difference in the earnings gains for men and women, perhaps because we do not have enough power to detect differences in our small sample.

[32] A few studies address these endogeneity problems. For example, Jepsen et al. (in press) use unique data on whether a student aspires to complete a degree in their first year, their first year courses, and field of study to capture differences in intent between degree completers versus dropouts.

Table 6
Matching estimation, log weekly earnings.

		Matched sample individual FE estimates		Matching estimates Avg. Post Educ Earnings	
	(1)	(2)	(3)	(4)	(5)
Post-Education	−0.012	−0.081	−0.027		
	[0.033]	[0.054]	[0.054]		
Post-Educ * For-Profit	0.103***	0.125***	0.114**		
	[0.037]	[0.048]	[0.046]		
For-Profit				0.148***	0.112*
				[0.039]	[0.060]
Notes	Preferred FE Specification (Table 3)	County of residence not used in matching	County of residence used in matching	County of residence not used in matching	County of residence used in matching
Observations	19,364	6897	6641	1646	1540

Robust standard errors clustered at the individual level in brackets.
Note: In columns 2 and 3 we first use propensity scores to create a matched sample of individuals who attended for-profit colleges to high school graduates. We use the following observables in constructing the matched sample: indicators for age, male, race, 1999 ASVAB math and verbal score percent, average pre-period hourly wage, and average pre-period hours. In column 3 we also match on the county of residence in 1997. Then, we run our preferred fully loaded individual FE specification on this matched sample. See Table 2 and the text for descriptions of the Post-Education and Post-Education * For-Profit variables. In columns 4 and 5 we report estimates using the 5-nearest neighbors matching estimator. We match on the same set of variables as in columns 2 and 3 respectively. The dependent variable in the matching estimations is the log of average post-education weekly earnings and we report bootstrapped standard errors.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

In specifications (4) through (6), we create alternate samples by dropping three types of individuals. We first drop 46 students who report attending both for-profit and public institutions in the year of degree completion or the year of last observed enrollment (for dropouts), in specification (4). When we drop these students, we find that the return to for-profit college attendance is marginally lower at 7.5%. It could be that higher ability students transfer to public institutions either to finish their associate's degree or to begin a bachelor's degree program.[33]

In specification (5), we drop the 42 "double dropouts", individuals who drop out of for-profit colleges and re-enroll. The results are somewhat higher than our baseline returns, at about 13.5%, as we might expect since double dropouts are undoubtedly negatively selected. In specification (6), we drop 452 individuals who report ever working towards a bachelor's degree in our high school only group. In contrast to the double dropouts, these individuals are likely positively selected, or at least have higher aspirations than others in the high school only group, as they are attending public and not-for-profit institutions with the (unrealized) goal of obtaining a bachelor's degree.[34] In this specification we find that for-profit students earn 12.2 percentage points over high school graduates.

Finally, in specification (7) we test for a differential return between for-profit students and public community college students by adding to our sample 1832 individuals who pursued or obtained an associate's degree in the public sector. In this specification, the variable *Post* refers to the post-education (or falsified post) period for all groups. *Post * AnyCollege* captures the differential gains to any type of college attendance (public or for-profit) over the high school only group. And, *Post * AnyCollege * ForProfit* captures the further differential effect of post-education for the for-profit students. Interestingly, we find no differential return to attending a for-profit college relative to a public community college. The point estimate on *Post * AnyCollege * ForProfit* is positive but small and statistically insignificant. Our results imply lower returns to community colleges than found in other studies (Jacobson et al., 2005; Jepsen et al., in press), perhaps because we exclude students who go on to obtain a bachelor's degree in our analysis. If community college students are more likely to transfer and complete bachelor's degrees than for-profit students, these estimates may understate the differences between the sectors.

6.4. Matching estimation

The individual fixed effects analysis addresses what we believe to be the most important endogeneity problem, i.e., unobservable differences in ability and motivation between students. But, it is open to critiques. For example, the time-varying characteristics of the control group of high school graduates may differ from the for-profit students in a systematic manner that we do not observe. As an alternative to the fixed effects estimation, we present

[33] Note, however, that these students never finish a bachelor's degree due to our sample restriction.
[34] Note, again, that these students never finish a bachelor's degree and they must be working before and after attending to contribute to identification. So even though they may be positively selected on aspirations, they may not be positively selected on other dimensions.

propensity score matching estimates in Table 6. This strategy can generate a more comparable control group, but it relies on a much smaller sample and cannot control for unobserved differences across groups.

We undertake two different matching exercises. First, we create a one-to-one matched sample of high school graduates to for-profit students. As a baseline, we match on indicators for age, male, race, average hourly wage for the pre-period, average hours worked for the pre-period and a measure of cognitive ability, the Armed Services Vocational Aptitude Battery (ASVAB) math and verbal score taken in 1999. Our preferred specification adds the 1997 county of residence to the list of match variables. Then, we run our preferred fully loaded individual FE estimation on this matched sample. The results are reported in specifications 2 and 3 of Table 6. The estimates on *Post * ForProfit* are similar to the individual FE for the entire sample (specification 1). In the matched sample, for-profit students earn 12.5% more than high school graduates, but the effect drops to 11% when we include the 1997 county of residence in the match. In comparison, the individual FE coefficient for the full non-matched sample suggest returns of 10.3% (specification 1).

Second, we report estimates from a more traditional propensity score matching in specifications 4 and 5 using the five-nearest neighbors matching method.[35] We match on the same set of variables described above (specification 4) and include the 1997 county of residence in specification 5. The dependent variable in these regressions is the log of average weekly earnings in the post-education period. Again the results are similar to the individual FE. Without controlling for the 1997 county, results are larger and closer to our OLS results (Table 2, columns 1 and 2), around 15%. When adding county to the match, our results are closer to our fixed effects estimates (11%), suggesting that geography plays an important role in selection. More importantly, the similarity of the FE and matching estimates suggests that time-varying unobservables are not driving our results.

7. Concluding remarks

This study takes a step toward assessing the quality of for-profit postsecondary education, estimating the before–after earnings gains of students pursuing associate's degrees in for-profit institutions. Using an individual fixed effects approach and data from the restricted-access NLSY97, we find that students who enroll in for-profit institutions experience earnings gains of about 10% over individuals who complete high school without any college degree, conditional on employment, in the 3 or 4 years after attendance. Given that the average length of associate's degree program attendance is 2.6 years, this amounts to a 4% return per year of education. Unconditional estimates raise our estimates of returns to 7% per year due to a slight increase in the likelihood of full-time employment among for-profit students.

Our estimates are robust to a wide array of specification checks. In extensions, we find no differential returns between four-year and two-year for-profit colleges and only minor differences when we exclude small groups of students who may be positively or negatively selected. We also find that students who complete degrees in for-profits earn 20% more than dropouts, but we cannot fully control for the endogeneity of completion in our model.

Overall, it is noteworthy that the returns to for-profit attendance appear positive and significant in all specifications and they are somewhat higher than the only other panel estimates of returns to for-profit colleges that we know of Turner (2012). We further find no difference in the returns between public community college students and for-profit students. However, our estimates for both groups fall below similar estimates of the returns to public community college associate's degree programs (Jacobson et al., 2005; Jepsen et al., in press) and well below the average returns to bachelor's degree attendance in the literature (Oreopolous & Petronijevic, 2013). Of course, we note that our estimates are for a small sample of young workers and we should be cautious comparing these returns to other samples and longer-term estimates.

Until now, we have focused solely on the private benefit to a for-profit education, but it is important to weigh these benefits against the cost of attendance. Tuition at a two-year aid-eligible for-profit college averages $14,000 per year compared to just $3000 at a public community college (National Center for Education Statistics (NCES), 2012). To pay for these programs, for-profit students take on more debt than students in other sectors. Almost 90% of students in aid-eligible for-profits borrow to finance their education and their loans average $7300 annually – more than any other sector (Cellini & Darolia, 2014). Added to this, roughly 22% of students default on their student loans within 3 years (U.S. Department of Education, 2011). These patterns are concerning, especially if average earnings gains are fairly low.

The most important question then becomes whether for-profit students' earnings gains are sufficient to offset both the private and social costs of education in this sector. Recent work assessing the costs of a for-profit associate's degree to students and taxpayers finds that a return of 8.5% per year is needed to offset the private costs to students of attending, including tuition, foregone earnings, and debt service. Adding in the cost to taxpayers (including federal and state grants, loan defaults, and subsidies) raises the needed return to 9.8% (Cellini, 2012). All of our conditional estimates fall well below both thresholds, suggesting that for the average student, the short-run returns appear to be too low to justify the private cost and much too low to justify the additional cost to taxpayers, at least for the federal aid-eligible institutions that we examine here. However, our unconditional estimates and those for degree completers come much closer to the benchmark for private costs, suggesting that some above-average students who complete degrees and find employment may find a for-profit college education worthwhile. It may also

[35] Our results are robust to using fewer or more neighbors in the match, and to using other matching techniques such as kernel. We use psmatch2 developed by Leuven and Sianesi (2003).

be the case that the longer-term returns are higher or lower than those presented here.

Still, from a student's perspective, it would seem that a lower-cost community college would likely be a better choice than a for-profit associate's degree program. It may be the case that students are unaware of the options available at local community colleges (Cellini, 2009) and we cannot rule out that aggressive recruiters in the private sector might mislead students into believing that the earnings gains will be higher than in the public sector (United States Government Accountability Office (GAO), 2010). On the other hand, students may simply value other attributes of for-profit colleges. For example, some programs may be offered in the private sector that are not offered in the public sector, and public institutions may be capacity constrained, making for-profit institutions the only viable option.

This study is just a first step toward understanding the quality of education in the for-profit sector. More studies using alternative data sources and methods are needed to definitively assess student outcomes in for-profit postsecondary institutions.

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