The Labor Market Returns to a Private Two-Year College Education

Stephanie Riegg Cellini
George Washington University
scellini@gwu.edu

Latika Chaudhary
Scripps College
latika.chaudhary@scrippscollege.edu

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Abstract

A lengthy literature estimating the returns to education has largely ignored the for-profit sector. In this paper, we offer the first causal estimates of the earnings gains to private two-year colleges, the vast majority of which are for-profit institutions. We rely on data from the 1997 National Longitudinal Survey of Youth (NLSY97) to implement an individual fixed effects estimation strategy that allows us to control for time-invariant unobservable characteristics of students. We find that students completing associate's degrees in private postsecondary institutions experience earnings gains of 15 to 17 percent post-degree, or 8 percent per year of education, gains similar to those experienced by public community college students in our sample. Our point estimates on the differential return to the private sector suggest that private postsecondary students may earn an additional 1 to 2 percentage points per year, but these estimates are not statistically significant. Among a broader set of students who enroll in, but may or may not complete an associate's degree, we find earnings gains of about 6 percent and no evidence of differential gains for students in the private sector.

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I. Introduction

After several decades of strong growth and relatively little controversy in for-profit postsecondary education, a recent report by the Government Accountability Office has brought for-profit colleges into the spotlight. The report ignited a firestorm of media attention and debate by uncovering unscrupulous recruiting practices and fraud in federal financial aid programs at several large for-profit colleges (GAO 2010; Lewin 2010; Goodman 2010). In response, the Department of Education has proposed controversial new rules for federal student aid eligibility, requiring institutions to show that graduates meet strict income-to-debt ratios and loan repayment rates to maintain eligibility (Federal Register 2010).

Central to the debate over the proposed regulations is the question of the quality of a for-profit education. Proponents of the rules claim that for-profit colleges leave students with insurmountable debt and few skills, while opponents argue that these institutions provide valuable job training for underserved students. Both sides rely heavily on anecdotal evidence and descriptive comparisons of earnings of graduates in the private and public sector. Without a better understanding of the causal effects of a for-profit education it is difficult, if not impossible, to assess the merits of the proposed reforms. Thus, the question of college quality is one of the most important for policymakers, taxpayers, and students alike.

Our study is the first to our knowledge to provide a credible assessment of college quality in the sector, by estimating the labor market returns, or earnings gains, to a for-profit education. Due to data constraints, we focus on private two-year colleges, the vast majority of which are for-profit. Also known as proprietary schools, career colleges, vocational/technical institutes, occupational colleges, or simply for-profits, private two-year colleges offer short-term certificates and associate's degrees in fields ranging from computer programming to hairdressing. Such colleges are an integral

part of the sub-baccalaureate market, competing for students with public community colleges (Cellini 2009).

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 (see reviews by Card 1999, 2001; Ashenfelter, Harmon, and Oosterbeek1999). Several studies have also assessed the returns to public two-year community colleges (e.g., Kane and Rouse 1995, 1999; Jacobson, LaLonde, and Sullivan 2005; Jepsen, Troski, and Coomes 2010). Private sub-baccalaureate education has received much less attention in the literature. We know of only two studies examining the returns to private two-year colleges (Grubb 1993, Chung 2008), and neither identifies the causal effect of education on earnings.

Our study fills this gap in the literature, overcoming a crucial endogeneity problem that plagues studies of earnings gains: students in private institutions may differ on both observable and unobservable dimensions from those in public institutions. If these differences are correlated with a student's choice of institution and her labor market success, cross-sectional OLS estimates of the impact of private two-year colleges on employment and earnings will be biased.

We implement an individual fixed effects approach to mitigate this problem. Unlike students in four-year colleges, two-year postsecondary students often work before, during, and after they attend, allowing us to compare an individual student's earnings after attendance to her earnings before. In so doing, the individual fixed effects can control for all time-invariant student characteristics that may bias cross-sectional estimates of returns.

Using the 1997 panel of the National Longitudinal Survey of Youth (NLSY97), we find that students completing an associate's degree in either a public or private college experience a 15 to17 percent increase in earnings, or about 8 percent per year of education. Our point estimates suggest that the earnings for students in the private sector may be 1 to 2 percentage points higher per year of

education than in the public sector, but these coefficients are imprecisely estimated and statistically insignificant. Among all students who enroll in associate's degree programs, regardless of completion, we estimate earnings gains of around 6 percent. We find no differential return for private sector attendees and point estimates for this sample are close to zero. Other labor market outcomes reveal similar patterns for completers and attendees: we find no differential effect of private postsecondary education on hours worked, hourly wages, or the likelihood of being employed or working full-time.

We subject our findings to a variety of robustness checks. We first test for trends in pre-education earnings that might bias our estimates of both the absolute and differential returns to public and private sector education. Although we find that students in both sectors experience declines in earnings of around 4 percent in the two years prior to enrollment, there are no differential pre-education earnings trends for private sector students. We therefore consider our estimates of absolute returns to be upper bounds, but we remain confident in our estimates of the differential return to a private postsecondary education. Moreover, our findings remain robust to alternate assumptions about the timing of education and various measures of earnings.

Finally, we extend our analysis to two additional samples: vocational degree/certificate holders and students in the 1979 cohort of the NLSY. Again, we find no differential effects of attending a private postsecondary institution in either sample, although we caution that our definition of private sector attendance is imprecise in both extensions.

Given the high costs of a for-profit education, it may be concerning that private two-year college students do not generate significantly higher returns than their public sector counterparts. Further, if public community colleges generate additional value by encouraging and enabling students to transfer to four-year colleges, our results may underestimate the return to the public sector. Nonetheless, our analysis reveals that private sector students experience positive earnings

gains and labor market outcomes similar to those of students in the public sector. The question then becomes whether these gains are enough to offset the high cost of attendance.

A back of the envelope student-level cost-benefit analysis suggests that the present value of the earnings gains from completing an associate's degree in a private two-year college barely outweighs the costs for the average student. However, costs may outweigh benefits under a range of reasonable assumptions about interest rates and returns. We conclude that many for-profit two-year college programs are likely to be worth the large investment, but some students may be better served in lower-cost community colleges.

The rest of the paper proceeds as follows: Section II reviews the literature on the returns to education and provides background on private and public sub-baccalaureate education. Section III provides a conceptual framework and Section IV details our estimation strategy. Section V describes the data, Section VI presents results, and Section VII concludes.

II. Background

Over the past half-century, a large literature has developed to measure the returns to schooling. Reviews of the literature by Card (1999) and Ashenfelter et al. (1999) report that one additional year of education results in earnings gains in the range of 6 to 9 percent. More recent and better-identified analyses reveal higher returns, averaging 10 to15 percent per year (Card 2001, Goldin and Katz 2008). The vast majority of the research in this area has focused on high school and four-year college-going: relatively few studies emphasize differences in various sectors and levels of schooling (Ashenfelter et al. 1999).

Only a handful of studies examine two-year colleges, and those that do focus almost exclusively on public community colleges. Reviewing the literature on community college returns, Kane and Rouse (1999) find that a year at a community college generates returns between 4 and 6 percent just marginally below those for a four-year college and on average, students attending community

colleges earn 9 to 13 percent more than their counterparts who do not attend any type of postsecondary institution.

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 instrumental variables and sibling comparisons, few studies in the community college literature have implemented similar identification strategies. Marcotte, Bailey, Borkoski, and Kienzl (2005), Kane and Rouse (1995), Leigh and Gill (1997), Grubb (1993, 1995), Monk-Turner (1994), Heineman and Sussna (1977) estimate cross-sectional models comparing students attending community college to those who do not, generally controlling for ability with proxies, such as IQ scores.

Recent studies on the returns to community college education have implemented more credible identification strategies. Jacobson et al. (2005) and Jepsen et al. (2010) use an individual- or person-specific fixed effects approach (as in this paper) comparing the wages of displaced workers 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 percent per year of education for men and 13 percent 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. (2010) find higher returns—about 40 percent for an associate's degree or diploma for women and 18 to 20 percent for men completing degrees.

We know of only two studies, Grubb (1993) and Chung (2008) that attempt to assess the returns to two-year for-profit postsecondary education. Both draw on cross-sectional variation in earnings, comparing students who attend for-profit institutions with students who attend other types of postsecondary institutions. Chung (2008) controls for selection on observables, but neither study can control for unobservable characteristics of students that may bias estimates. Both studies provide limited evidence of positive effects of for-profit training, particularly for women and certificate programs, but generally show no significant differences in returns to the for-profit sector relative to other sectors.

In this study, we provide the first plausibly causal estimates of the impact of private (mostly for-profit) two-year postsecondary education on earnings. We follow Jacobson et al. (2005) and Jepsen et al. (2010), as well as earlier work by Angrist and Newey (1991) in implementing an individual fixed-effects approach to overcome the endogeneity issues that plague cross-sectional studies.[1]

Private Two-Year Colleges

Research on private two-year colleges is scarce, primarily due to a lack of data. Most studies of these colleges rely on a non-random sample of schools and students based on the U.S. Department of Education's Integrated Postsecondary Education Data System (IPEDS) and most are purely descriptive in nature (Apling 1993; Bailey, Badway, and Gumport 2001; Rosenbaum, Deil-Amen, and Person 2006; Turner 2006, Cellini and Conger 2010).[2] Administrative licensing data from California has added to our knowledge of these institutions in recent years and allowed for causal

[1] This approach has also been used in the broader labor economics literature. For example, Ashenfelter (1978) and Ashenfelter and Card (1985) 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) for an overview of the fixed effects strategy in labor economics.

[2] The IPEDS severely undercounts the number of private two-year colleges in the U.S. For many years the survey relied on snowball sampling and did not require the participation of two-year private colleges. In recent years, greater efforts have been made to track down institutions receiving federal financial aid, but many colleges remain unaccounted for in the data (Cellini 2009).

studies of institutional behavior (Cellini 2009, 2010), but in spite of these advances surprisingly little is known about private two-year colleges and their students.

What we have learned from these studies and data sources is that there are more than 3,800 private two-year colleges in California alone, but most of these colleges are quite small. Average enrollment is just 350 students, a figure that pales in comparison to community colleges that average 6,600 students each nationwide (NCES 2010). Private two-year colleges are primarily organized as for-profit institutions: only 8 percent of California private two-year colleges are reported to receive nonprofit or religious exemptions (Cellini 2009).

Existing research also shows that public and private sub-baccalaureate institutions compete for students and offer degrees and certificates in a wide range of overlapping vocational and academic fields (Cellini 2009), but some differences in program offerings remain. Turner (2006) reports that for-profit colleges award a disproportionate share of associate degrees and less-than-two-year certificates in pre-professional and vocational fields in which skills are easy to verify and physical plant requirements are modest. Community colleges award a much larger share of associate's degrees in the liberal arts than private colleges, in part due to their role as transfer institutions (Cellini 2009, Turner 2006). In most states, community colleges have articulation agreements with public four-year colleges, allowing students to pursue the first two years of their bachelor's degree at a community college before transferring to a university. Nonetheless, only a small portion of community college students appears to take advantage of the opportunity: in California, only 15 percent of students transfer within seven years (Sengupta and Jepsen 2006).

The most important difference between private and public two-year colleges is undoubtedly their price: required tuition and fees for public community colleges average just $7,000, while private two-year colleges charge more than three times as much, averaging $22,500 in 2009-10 (NCES 2010). Private two-year college students undoubtedly receive substantial federal, state, and

private financial aid awards to bring costs down, but, as in the four-year college market, the price difference remains substantial (Cellini 2010).

III. Conceptual Framework

If the market for two-year college education is perfectly competitive, the public-private price differential should reflect differences in the quality of education. With many of the same programs available in the public sector, informed consumers should only attend a private two-year postsecondary institution if the present value of future earnings gains is high enough to offset the opportunity cost of attendance, including the direct costs of education (e.g., tuition, fees, books, interest on debt) and foregone earnings.

There are two primary mechanisms through which two-year colleges can influence earnings and employment outcomes. The first is through the quality of instruction. Because private colleges are unrestricted by government bureaucracy, they may be able to attract more knowledgeable or dedicated faculty than community colleges. They may also be able to add and modify courses more quickly to meet industry demands, build new classrooms or laboratories, or make other changes to enhance instruction. If instructional quality is indeed superior at private colleges, we would expect private two-year graduates to experience greater earnings gains that their public sector counterparts.

Private colleges may also impact earnings through career development services. In case studies comparing seven of the best private two-year colleges to community colleges in Illinois, Rosenbaum, Deil-Amen, and Person (2006) find that private colleges provide students with extensive job placement services as well as frequent and mandatory advising. The colleges also engage in substantial outreach to build long-term relationships with local employers and teach "soft skills" to students in preparation for the workplace. These services are almost nonexistent in the public sector and may have an important impact on labor market outcomes.

Competition in the private sector provides the incentive to allocate resources to their most efficient use, whether those are student services or instructional resources, yet many observers have concerns over the quality of private two-year, and particularly for-profit institutions. The more complicated the skill, and the more difficult it is to verify skill acquisition, the less likely employers are to trust small mom-and-pop and online private colleges. Allegations of fraud and abuse in the sector have further contributed to a sense of mistrust in recent years and students may be basing their college choice decisions on misleading or inaccurate information.

Among other things, colleges have been accused of making misrepresentations about the salaries of graduates, tuition costs, time-to-degree, and the transferability of coursework (e.g., GAO 2010; Lewin 2010; Hechinger 2010; Goodman 2010; Arenson 2005; Hefftor 2007). For example, one for-profit college recruiter told an undercover investigator that he could expect earnings of over $100,000 to $250,000 for a barber certificate, when 90 percent of barbers make less than $43,000 per year. At several colleges students were told tuition costs based on 9 months of coursework, when the programs in question required 12 months of coursework (GAO 2010).

While the extent of such fraud and abuse is still unclear, if students are misled into believing their costs will be lower and earnings potential will higher than what they experience in reality, we would expect to observe relatively low earnings gains that may not fully offset the cost of attendance. Further, in the presence of widespread improprieties, or simply pervasive rumors of fraud, employers may be hesitant to hire students coming out of for-profit institutions. In that case, we would expect earnings gains from private two-year colleges to be low, and perhaps lower than their more trusted—and regulated—public sector counterparts. Two-year private graduates would also be less likely to be employed or employed full-time, relative to public community college graduates.

IV. Estimation

In our first set of regressions, we estimate log weekly earnings and other employment outcomes (i.e., log hourly wages, log hours worked per week, and indicators for whether the individual was employed and whether she was employed full-time), y_{it}, for individual *i* in year *t* as a function of two-year college completion, private college attendance, and individual characteristics as specified in equation (1):

$$y_{it} = \beta_0 + \beta_1(Private_i * Post_{it}) + \beta_2(Post_{it}) + \beta_3 X_{it} + d_t + \varepsilon_{it} \quad (1)$$

The variable $Post_{it}$ identifies the timing of each student's degree completion. It equals 1 in the year after the individual receives their degree or certificate, and remains 1 in subsequent years. We interact this variable with $Private_i$, an indicator for whether the individual received their degree from a private two-year college. The result is that the variable of interest $(Private_i * Post_{it})$ equals 1 for private two-year college students in each year after degree completion and 0 otherwise.[3] We add a vector of calendar year fixed effects, d_t, to control for inflation and other time-varying effects that are common across individuals. Finally, we control for individual characteristics X_{it}, including sex, race (black, white, Hispanic, Asian, and other), region (Northeast, North Central, South, and West), and age. We add both age and age squared, but our preferred estimates employ age fixed effects to allow for more flexible controls for these young workers.

We first estimate simple OLS regressions, incrementally adding more detailed controls, such as students' scores in math knowledge and paragraph comprehension on the Armed Services Vocational Aptitude Battery (ASVAB) exam (as proxies for ability); whether the student is foreign-born; whether a language other than English is the primary language spoken at home; and

[3] We do not include the main effect $Private_i$ in our OLS specifications to better match our fixed effects estimates, where this term is unidentified: adding it does not change our results (available on request).

household income in the years before attendance.[4] In this specification, $\beta_1 + \beta_2$ captures the absolute return to a private two-year college, while β_1 reflects the differential gains that accrue to private college students compared to their counterparts in public community colleges, but we caution that several potential complications confound causal inference.

The most important problem is that individuals in private and public two-year colleges may differ in observable and unobservable ways. If these differences are correlated with labor market outcomes, estimates of both the absolute and relative returns to a private two-year college education will be biased. To accurately estimate the causal effect of private sector attendance, we would ideally like to randomize individuals across public and private institutions. In the absence of randomization, equation (1) attempts to control for a number of observable differences between public and private students. But, even with these controls, cross-sectional OLS estimates of equation (1) will be biased if omitted variables, such as innate ability, are correlated both with earnings and the choice of sector. In the case of public and private two-year colleges, ability is only one of the omitted variables we might be concerned about. A student's motivation, location, social network, information about educational options, or knowledge of the local labor market may cause additional biases.

Previous studies of the returns to education have used a range of methods to mitigate this endogeneity problem, including the addition of proxies for ability (i.e., IQ score), propensity score matching, instrumental variables, and sibling or twin comparisons, yet few of these studies can adequately control for all unobservable characteristics of individuals that might be correlated with schooling and earnings.

In this study we implement a stronger identification strategy, employing fixed effects for each individual in the sample. Unlike high school and four-year college students, two-year college

[4] To avoid endogeneity, we only use measures of household income that are reported in the years before attendance. When multiple years of data are available, we use the earliest year.

students typically work either before or during their coursework, making it possible to observe earnings before and after attendance. We can therefore estimate:

$$y_{it} = \beta_0 + \beta_1(Private_i * Post_{it}) + \beta_2(Post_{it}) + \beta_3 X_{it} + d_t + d_i + \varepsilon_{it} \qquad (2)$$

where d_i is a vector of dummy variables for each individual. The individual fixed effects absorb the effects of all time-invariant individual characteristics, leaving age as our only control in X_{it}.

The primary advantage of the fixed effects approach is that in addition to controlling for observable time-invariant characteristics, it also controls for unobservable time-invariant characteristics of the individual that might bias cross-sectional estimates. These include innate ability, motivation, and other correlated idiosyncrasies. The model is identified off of changes in each student's earnings before and after college attendance. β_1 therefore reflects comparisons of the before-after earnings gains for private sector students to the before-after gains for public community college students.

The individual fixed effects go a long way in mitigating endogeneity from omitted variables, but several considerations remain. First, it is important to note that our estimates apply only to a selected sample of individuals. Our main analytical samples include only the set of students who ever enroll in an associate's degree program and for whom we can observe earnings before and after education. These restrictions may cause our absolute estimates of returns to be biased upwards. We discuss our samples in detail in Section V.

Second, our estimates of differential returns may be biased if both the returns to education and selection into private colleges are heterogeneous by demographic, family, or other individual characteristics (Card 1999, 2001). However, we find relatively few differences between public and private college students on observable dimensions, suggesting that this bias may be small. We explore these differences in Section V.

We may also be concerned about biases associated with the dynamic selection of individuals into college. In particular, estimates of the absolute returns to a two-year college 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.[5] The same type of selection may occur in this case if individuals who are laid off or otherwise experience a decline in earnings are more likely to enroll in associate's degree programs than others. This type of dip would cause our estimate of absolute returns to a two-year degree to overstate the true gain.

In this study, however, our primary focus is less on the absolute return to attending a private two-year college, but rather on the differential return to a private two-year college relative to a public community college (β_1). Our use of public sector students as controls for private students mitigates the problem of Ashenfelter's dip to some extent. We have no reason to believe that individuals with negative earnings shocks would be more or less likely to choose the private sector. If the pre-education earnings trends of public and private sector students are similar, then our estimates of the differential returns to private postsecondary education will remain unbiased. We explore this issue descriptively in Section V and formally test for differential pre-education earnings trends in Section VI.

One final limitation of the fixed effects approach merits discussion. Freeman (1984) and Griliches and Hausman (1986) demonstrate that fixed effects may exacerbate bias from classical measurement error. The nature of the longitudinal data used in fixed effects analyses means that individuals misclassified in one period will be misclassified in later periods, amplifying the attenuation bias of fixed effects estimates relative to cross-sectional OLS. In the presence of

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

sizable measurement error, our fixed effects estimates will provide a lower bound on the returns to a private two-year college education.[6]

V. Data

To implement our analysis, we draw on the 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 representative panel of 8,984 youths who were 12 to 18 years old when they were first surveyed in 1997. The youths are interviewed each year and we use data available through 2008. We thus have a group of individuals ranging from age 24 to 30 by 2008. The panel contains in-depth questions on educational attainment, earnings, and other related topics.[7]

To identify the differential effects of private postsecondary education on earnings, we restrict our analysis to two specific samples of individuals. Given our focus on the returns to two-year college degrees, in both samples we drop individuals who went on to receive a bachelor's degree or higher degree, and anyone who completed 16 or more years of schooling even if they do not have a bachelor's degree. This restriction creates a selection problem if public community college students transfer to four-year institutions at higher rates than private students and may generate an upward bias on the differential return to private two-year colleges. However, this is a necessary restriction of the data to ensure we observe earnings for individuals both before and after they complete their postsecondary program.[8] By focusing on two-year college students who do not transfer to four-year colleges in the time period we observe, we are also limiting the analysis to a pool of individuals who are likely to be more similar to each other relative to four-year college students.

[6] For an overview of issues related to fixed effects estimation in labor economics, see Angrist and Krueger (1999).
[7] In an extension discussed in detail in Section VI, we draw on the 1979 cohort of the NLSY to look at earnings gains over a longer timeframe. We discuss these data below.
[8] We observe earnings for 76 percent of public and 79 percent of private two-year college students in the NLSY97. This difference is not statistically significant.

Our first sample, hereafter referred to as the "completers" sample, focuses on students who completed an associate's degree. Although samples of graduates are commonly used in the literature, we recognize it is not without problems because it selects individuals on the basis of the positive outcome of completion. Higher ability individuals may be more likely to both complete an associate's degree and earn higher wages. Hence, this may bias estimates of the absolute return to a two-year education, but it is unlikely to bias the differential return between a public and private college unless completion rates differ by sector: we return to this issue below.

Our second sample, the "attendees" sample, includes both the completers and students who reported enrolling in an associate's degree program but not receiving a degree in the time period we observe.[9] Since this sample does not condition on completion, it offers perhaps more credible estimates of the absolute returns to a two-year college education. But, as noted above, selection remains an issue as our sample only includes students who pursue an education beyond high school and this may again generate an upward bias in our estimate of absolute returns.[10] If public and private students are similar, however, our estimate of the differential return to private two-year college attendance will be accurate.

The two key variables for our analysis are indicators for the post-education time period and whether an individual attended a private college. We have several options to define the variable $Post_{it}$ in equation (1). To construct a consistent measure across the two samples, we code $Post_{it}$ as 1 only when we are certain that individuals are out of school. In the case of completers, this includes all years following the receipt of an associate's degree. In the case of attendees, for our baseline specifications, we assume they attend college for one year. $Post_{it}$ therefore switches to 1

[9] Note that the group of "non-completers" in this sample includes both students who dropped out of an associate's degree program and those who are still enrolled but have not yet completed. Our data does not allow us to distinguish between these two groups.

[10] In specifications not reported, we added individuals with only a high school diploma who never attended a two-year college. For these individuals, we created a falsified definition of *Post* three years after high school graduation to generate a before-after difference in our fixed effects specifications. Our results were unchanged and are available on request.

the year after they first report attending a two-year institution and remains 1 for all subsequent years. We report results using other definitions of $Post_{it}$ such as counting the year of graduation as post (for completers), assuming two years of attendance (for attendees who do not complete), and dropping the years in school from the analysis.

We use a strict definition of private college to ensure that our measure accurately captures the sector of the institution where an individual received her degree. For each college that an individual attends, the NLSY97 asks whether the college was public. Using this information, we code students enrolling in a private postsecondary institution if the respondent reported attending a non-public college in the year of completion and/or attendance. For years where this information is missing, we rely on reports from previous years.[11] As noted above, our variable of interest, $(Private_i * Post_{it})$, is equal to 1 beginning the year after the individual receives an associate's degree from a private college (or one year after attendance for those who do not complete) and remains 1 for all subsequent years.

Following the returns literature (e.g., Angrist and Krueger 1991), our main dependent variable is the log of weekly earnings—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 ten jobs, but the number of people reporting wages for more than five jobs is less than one percent over our 12-year period. Hence, we focus on average weekly earnings across the first five jobs. We also report results using measures of weekly earnings based on the first reported job and all reported jobs.

We further limit our analysis to students who are 16 years or older to avoid capturing wages in informal early jobs (e.g., babysitting, paper route). We also drop 12 observations with weekly

[11] If students reported attending both public and private institutions in the year of completion/attendance, we code them as public. Other definitions of private college attendance yield similar results (available on request). We also acknowledge that our definition of private college relies on students' self-reported assessment of college type. To the extent that students are unclear about the sector of their college, a small amount of (presumably) random measurement error may be introduced, thereby attenuating our estimates.

earnings above $100,000 because they are clear outliers.[12] Observations reporting zero earnings are treated as missing and dropped from the analysis: our estimates therefore reflect returns conditional on employment.[13] Among completers, we observe earnings for an average of 6 years before and 4 years after degree completion. For attendees, we observe 4 years pre- and 5.5 years post-attendance.[14] Since we observe post-degree earnings for a short time period for very young workers, our findings may underestimate the returns to both public and private colleges if earnings are more responsive to degree completion in the long run. We test for this possibility using data from the 1979 NLSY in Section VI below.

In addition to weekly earnings, we examine several other labor market outcomes. We first decompose weekly earnings and estimate the effects on log hourly wages and log hours worked per week across the first five jobs. We also estimate the effects of private and public two-year college education on full-time employment and any employment, this time including observations with missing earnings data. Our measure of full-time employment equals one if an individual reports working 35 or more hours per week, and an indicator for any employment equals one if an individual reports non-zero weekly earnings per week.

In addition to the individual fixed effects models, we also present cross-sectional OLS regressions controlling for observable individual characteristics. Following Cellini and Conger (2010), we construct dummy variables for an individual's gender, race, foreign-born status and whether a language other than English is the primary language spoken at home. We also include controls for math and reading comprehension test scores, and household income. Tables 1A and 1B report the summary statistics for the main variables used in our analysis in the completers and

[12] Our results are robust to both keeping these observations and dropping the individuals reporting these observations.
[13] We also estimated regressions where we substituted $1 for individuals with missing weekly earnings. The estimates on private college were smaller in magnitude, but statistically insignificant, similar to our main findings.
[14] The only significant differences by sector are for attendees post-education (5.48 and 5.73 years for public and private students, respectively, with a t-statistic of 4.25).

attendees samples, respectively. We present the means by sector and by pre- and post-education for the time varying variables.

In the completers sample, in Table 1A, we follow 670 individuals of which 139, or 21 percent earned their associate's degree from a private two-year college. There are no significant pre-education differences in employment outcomes between the public and private students, suggesting no differential pre-education earnings dip for the two groups. Post-education, individuals completing their degree at a private college earn higher weekly earnings ($657 compared to $605), but the difference is statistically insignificant. Among the other employment outcomes, only the likelihood of average hours worked per week is marginally higher for private college individuals. On observable time invariant demographic characteristics in the lower panel, private college associate's degree holders are more likely to be female, Asian, and have higher reading scores compared to public associate's degree holders, but on all other dimensions the groups look remarkably similar.

In the attendees sample, we add 1,718 individuals who enrolled in two-year associate's degree programs but did not complete their degree, for a total of 2,388 individuals. Relative to the completers, a smaller proportion of the sample enrolled in private colleges at 15 percent. Comparing the two samples, 39 percent of private sector students complete degrees versus 26 percent in the public sector, suggesting that our estimates will likely overstate the differential return to the private sector in the completers sample.

In Table 1B, as in the completers sample, we again find no pre-education differences in the employment outcomes among public and private sector attendees. Post-education, the public community college students earn $454 per week compared to $462 per week among the private postsecondary students. The public and private college attendees reveal similar differences in

demographic characteristics as the completers sample. Private college attendees are more likely to be female and non-white.

To further examine the time pattern of earnings, Figures 1A and 1B graph log weekly earnings by sector against years since college entry for completers and attendees, respectively. We do not observe an obvious decline in earnings in the years prior to entry for either group, and earnings for both groups appear to experience similar increases after attendance. Most importantly for our estimates of differential returns, we see little difference between private and public students in the pre-education earnings trends in either sample. In Figure 1B, there may be a slight dip for attendees in the year prior to enrollment, but this effect does not appear to be large. Notably, the two groups also show similar variance in earnings in every year except the last (for which we have very few observations). We formally test for differential trends in pre-education earnings in the next section. Finally, Figures 2A and 2B graph the age profile of earnings for the completers and attendees samples, respectively. In both samples, the age of entry is similar. Private college students have slightly higher post-education earnings by age in both samples but the differences appear small.

VI. Results

Table 2 presents our first set of findings on log weekly earnings for students who completed their two-year college training and received an associate's degree. Specifications (1) through (5) are standard OLS models exploiting variation across individuals, as in equation (1). Specification (1) includes only age and age squared along with year fixed effects, which are included in all the models. Specification (2) employs the more flexible age fixed effects. Specifications (3) to (5) incrementally add fixed effects for sex, race and region, proxies for ability such as math and reading test scores, foreign-born status, primary language other than English, and household income.[15]

[15] The sample size changes across specifications due to a large amount of missing data for these control variables, particularly household income.

Turning first to the second row of Table 2, the OLS specifications indicate that receiving an associate's degree at any type of institution generates earnings gains of roughly 12 to 13 percent, or about 6 percent per year. This finding is robust to the inclusion of all the covariates across columns (1) to (5). While we recognize these absolute returns are likely biased, our estimate is comparable to other cross-sectional estimates in the literature on returns to two-year colleges (e.g., Kane and Rouse 1995, 1999).

As noted above, the main focus of our analysis is on the differential return to private two-year colleges relative to public. Hence, the first row in Table 2 includes an interaction of post-education with an indicator for individuals that received their degree from a private two-year college. Relative to a public community college, receiving a degree from a private two-year college increases earnings by an additional 10 percentage points in the OLS models although the coefficient becomes smaller and insignificant when we include household income (column (5)).

Given the endogeneity problems inherent in cross-sectional estimates, we turn to our preferred individual fixed effects estimates in specifications (6) and (7). These estimates compare earnings for the same individual before and after they receive their degree, according to equation (2). Specification (6) includes age and age squared, while specification (7) includes the more flexible age fixed effects. Both specifications also include year fixed effects. The estimates of absolute returns to receiving an associate's degree, regardless of institution in the individual fixed effects are slightly higher than the OLS estimates. The estimates in row 2 of specifications (6) and (7) average 15 to 17 percent, or about 8 percent per year of education, and are significant at the one percent level. However, in contrast to the OLS estimates, a two-year college education in the private sector can no longer be shown to have higher returns than a public sector education. Using a consistent sample, the magnitude of the interaction between private college and post-degree drops from 11

percent (columns (1) and (2)) to 3 percent with the fixed effects (columns (6) and (7)), and the standard errors increase substantially.

We interpret these results as suggesting that students completing associate's degrees in private two-year colleges experience earnings gains similar to those of students graduating from public community colleges. After controlling for time-invariant individual characteristics, we cannot show that private postsecondary students' earnings differ from those of their public sector counterparts. We acknowledge, however, that the point estimates on $(Private_i * Post_{it})$ are positive and it may be the case that after including the fixed effects, we simply do not have sufficient power to detect a difference. If this is the case, a maximal interpretation of our results suggests that private two-year college students may earn up to 3 percentage points (or 1 to 2 percentage points per year) more than students completing associate's degrees in the public sector.

In Table 3, we further explore the differential effect of private college attendance on other employment outcomes such as hourly wage, hours worked per week, full-time and any employment, using our preferred individual fixed effects regression with age and year fixed effects. For comparison, we show the results on weekly earnings in column (1).

The results in columns (2) and (3) suggest the positive effects of an associate's degree on earnings arise from both higher hourly wages (10 percent higher) and more hours worked per week (7 percent higher). But again, we are unable to detect a statistically significant differential effect on either wages or hours worked for private college students. The point estimate on hours worked suggests that hours may be 2 percentage points higher for private college students, but again we cannot say with confidence whether this estimate is significantly different from zero. The estimate on hourly wage is both very small in magnitude and imprecisely estimated. Specifications (4) and (5) are linear probability models for full-time and any employment, and similar to earnings, there

are no differential effects of private college attendance on these outcomes. In fact, the point estimate on $(Private_i * Post_{it})$ for any employment is negative.

Table 4 extends our analysis to the attendees sample, considering both the completers and individuals that enrolled in a two-year associate's degree program but never received a degree in the 12 years we observe. For this sample, enrolling in a two-year college in either sector generates earnings gains of 6 percent (row 2, specification 1)—less than half as large as the increase for completers. This effect appears to be driven by the number of hours worked per week (column (3)), rather than by an increase in hourly wages (column (2)), a pattern that might be expected if many of these students drop out of college and spend more time working in low-wage jobs. Similar to the completers sample, we again observe no statistically significant difference between the public and private sector (row 1). Moreover, the point estimates on the $(Private_i * Post_{it})$ interaction are small in magnitude, suggesting that any positive impact of private two-year colleges is not economically significant.[16]

As noted above, the dynamic decision to enter college can bias estimates of the absolute returns to either a public or private two-year college. This type of bias arises if an individual's earnings decline in the years immediately prior to college entry. If the pre-earnings trend is different for public and private sector students, then this will also bias the estimates of the differential return to private colleges. To assess the role of such biases, Table 5 shows the results of specifications that replace $Post_{it}$ with an indicator for the two years before enrollment, denoted $Pre\text{-}Ed_{it}$.[17] Again, we add an interaction with private college $(Private_i * Pre\text{-}Ed_{it})$. We also include our standard individual, year and age fixed effects.

[16] Other specifications not reported here included controls for completion and omitting completers: results were similar. We also tested for a differential effect of gender on individuals either completing their degree from or attending a private college, and found no significant differences.

[17] We assume that completers first enroll two years prior to completion.

The results are similar across the two samples suggesting that both completers and attendees experience a dip in earnings of about 4 percent in the two years before enrolling in any type of college. They are also less likely to be employed or employed full-time. More important for our purposes, however, is that we find little evidence of differential earnings trends for private and public two-year college students. None of the coefficients on $(Private_i * Pre\text{-}Ed_{it})$ in Table 5 are distinguishable from zero at the five percent level in either sample, and only two are marginally significant (at the 10 percent level). Of most concern is the sizable positive coefficient on $(Private_i * Pre\text{-}Ed_{it})$ in the earnings regression for the attendees sample (specification 1, row 3), suggesting that private postsecondary students experience less of an earnings dip than community college students. This may lead us to understate the post-education earnings differential between the two groups in the attendees sample.

Robustness Checks

Our findings, thus far, reveal no significant differential impact of a two-year private college education on either earnings or other employment outcomes. In the top and bottom panels of Table 6 we subject our findings to various robustness checks for the completers and attendees samples, respectively. In both panels we show our baseline individual, year and age fixed effects regression for comparison as specification (1). Specification (2) weights the observations using the sampling weights provided by the NLSY97. Specification (3) includes a measure of potential experience, defined as age-schooling-6. In both samples, the results remain similar to the baseline.

Specifications (4) and (5) use alternate definitions of $Post_{it}$. In the completers sample, specification (4) counts the year of graduation as one, rather than zero. For May graduates, this specification counts earnings accruing in the remaining months of the year as post-education. As expected, it decreases the estimate of earnings gains to degree completion slightly, but results for the differential return to the private sector remain the same. Specification (5) drops the

observations corresponding to the two years that completers are (presumably) in school: results are unchanged.

In the attendees sample, we also show alternate definitions of $Post_{it}$ in specifications (4) and (5), but these differ slightly from the completers sample.[18] Specification (4) sets $Post_{it}$ equal to one starting two years after we observe attendance for non-completers, rather than one year after, as in our baseline specification. In specification (5) we return to our assumption that non-completers spend one year in school, but we drop the observation in that year. Our estimate of returns to attendance in either sector diminishes substantially and is no longer significant. Our coefficient on $(Private_i * Post_{it})$ is negative, but remains insignificant.

In specification (6), we drop all public sector students and restrict our focus to the 139 individuals that earned their degree from a private college in the completers sample and the 355 individuals that attended a private college in the attendees sample. Unlike our baseline specifications, this regression is a simple before and after comparison of earnings, and cannot distinguish between the effect of earning a two-year degree and earning that degree from a private college. Using the limited sample, earning an associate's degree from a private college confers a 14 percent return (top panel of Table 6, column (6)). Effects of attending a private postsecondary institution are around 4 percent for the restricted sample, but cannot be shown to be different from zero (bottom panel of Table 6, column (6)).

Specifications (7) and (8) use alternate definitions of weekly earnings to address concerns regarding our construction of the earnings measure. Our main results measure the average weekly earnings over the first five jobs reported in a year. Specification (7) focuses on earnings from the 1st job (the current/most recent job in the NLSY97) and specification (8) focuses on earnings averaged

[18] In results not shown, we include the year of first attendance as *Post* for non-completers in the attendees sample—this yields negative returns to attendance in either sector, as might be expected if students stop working in the year they attend. The differential return to a private college remains small and insignificant: results available on request.

over all the jobs worked during the year. The results on these alternate earning measures are again similar to those reported earlier. Although the point estimates on the interaction in the completers sample suggest the possibility of higher earnings for private students, again we cannot reject the null hypothesis that these estimates are statistically different from zero. We subjected the other employment outcomes to these robustness tests and the results were unchanged.

Vocational Degrees and Certificates

Since many two-year colleges award short-term (typically one-year or less) vocational certificates and degrees in addition to associate's degrees, we extend our analysis to include these students in our completers sample. Unfortunately, the NLSY97 does not allow us to precisely identify whether individuals pursued their vocational degree/certificate in a public or private institution. We therefore construct a proxy for private college attendance using a question that asks about the "type of training" that an individual participated in. We classify students as attending private institutions if they describe their training as "vocational, technical, or trade; business/secretarial; nursing school; or correspondence course" in the year they report receiving their vocational degree or certificate.[19] Students reporting training in a "community or junior college" were coded as public. We omit students reporting employer-provided, government, and apprenticeship training.[20] We acknowledge that the wording of the question is vague and may reflect the occupation of the student rather than the sector of attendance: we therefore urge caution in the interpretation of these results.

Table 7 presents the results of our analysis. The variable $Post_{it}$ now captures the post-degree effects of either an associate's degree or a vocational degree. To identify any differential return to vocational degrees and certificates in either type of institution, we add the interaction term

[19] We also experiment with a narrower definition of private college attendance, using only students who identified "vocational, technical, or trade" as the type of training: results are similar.

[20] We acknowledge that the wording of the question is vague and may reflect the occupation of the student rather than the sector of attendance.

($Voc_i * Post_{it}$) to the model. We also include an additional interaction term ($Private_i * Voc_i * Post_{it}$) to identify the differential returns to vocational degrees/certificates from private institutions.

The coefficient on ($Voc_i * Post_{it}$) suggests that vocational certificates generate earnings gains of about 14 to 15 percent lower than an associate's degree from either sector. This finding makes sense in light of the short duration of many of these programs. Echoing our earlier findings for associate's degree holders, we again find no differential effects of obtaining a vocational degree or certificate from a private institution.

Earnings over the Lifecycle

One of the main drawbacks of the NLSY97 is that the individuals in the sample are still quite young—only 24 to 30 years old in 2008—and we observe on average just 4 to 5 years of earnings after attendance or completion. Therefore, our results may not fully reflect the earnings gains that may accrue to students over the lifecycle. To investigate this possibility, we turn to the 1979 cohort of the NLSY.

Like the 1997 cohort, the NLSY79 is based on a nationally representative sample of young men and women who were first surveyed as teens in 1979. The 12,686 respondents were interviewed annually through 1994 and have been interviewed on a biennial basis since, with the latest round in 2006. As in the NLSY97, the 1979 survey identifies students who enroll in (and complete) associate's degree programs, but unlike the newer cohort, the 1979 participants were never asked whether the college they attended was public or private. To get around this problem, we rely on a variable identifying the "location of training." This variable is analogous to the "type of training" variable that we used to create a proxy for private attendance among vocational degree/certificate holders in the NLSY97.

To identify private college students in the NLSY79, we first limit our analysis to associate's degree completers and attendees to generate estimates of returns comparable to those in our main

NLSY97 samples. We then count as "private," associate's degree attendees who listed a "vocational/technical institute, business college, nurses program, barber/beauty school, flight school, or correspondence course" as the location of their training in the year they receive their degree or first reported attendance. There was no community college option on the list of choices in the NLSY79, we therefore count as public all associate's degree holders who did not specify one of the options above. We again acknowledge that our definition is imprecise and urge caution in interpretation.[21]

Corroborating the patterns found for the 1997 cohort, we find no differential effects of our proxy for private two-year college graduation on weekly earnings, hourly wages, or hours worked per week among completers or attendees in Table 8 (column (1)). Even a maximal interpretation of the point estimates yields returns only two percentage points higher for private students in both samples. Estimates of earnings gains to an associate's degree in either type of institution are higher than those found in the NLSY97 for completers (about 25 percent) but are similar among attendees (4.5 percent).[22]

VII. Concluding Remarks

This study takes a first step in assessing the quality of private (mostly for-profit) two-year college education, comparing the earnings gains of students attending private postsecondary institutions to students attending public community colleges. Using an individual fixed effects approach and data from the 1997 NLSY, we cannot reject the hypothesis that students in private and public sub-baccalaureate institutions earn similar returns: completing an associate's degree in either type of institution yields earnings gains of about 15 to 17 percent or 8 percent per year of education.

[21] Our sample of completers includes 833 students, of which 13 percent attend private institutions. Our sample of attendees includes 3,397 students, with 8 percent attending private institutions. As a robustness check, we use a narrower definition of private college, including only students who attended vocational/technical institutes: results are unchanged.

[22] We also limit the NLSY79 sample to young workers to match the age profile in the 1997 cohort: results are similar to the full sample. As a final extension, we add vocational degree/certificate holders: our results are similar to those for the 1997 cohort.

Point estimates on the differential return to the private sector suggest that private two-year college students may experience an additional increase of up to 3 percentage points (or 1 to 2 percentage points per year of education), but these estimates are not statistically significant. Importantly, we can rule out that private two-year college students experience declines in earnings. However, even a maximal interpretation (suggesting 9 to 10 percent gains) reveals returns that are somewhat lower than the most recent estimates of the returns to education for other types and levels of schooling.

Assessing the effects of two-year college attendance on the full set of students who enroll in, but do not necessarily complete an associate's degree, reveals that students in both sectors experience earnings gains of about 6 percent. We again find no differential effect of attending a private postsecondary institution relative to a public and in this sample point estimates on differential returns are close to zero.

Several caveats are in order. First and most importantly, our analysis excludes students who later transfer to four-year degree programs. If, as anecdotal evidence suggests (Hechinger 2005), students in public community colleges are more likely to transfer to four-year institutions, the returns to the public sector may be understated and community college students may indeed experience larger earnings gains than their private counterparts in the long run.

Second, our data have substantial limitations. The NLSY97 relies on a small sample of young workers and may not reflect earnings gains over an entire career. Further, the data do not allow us to distinguish between the returns to for-profit and not-for-profit private two-year colleges (although there are relatively few not-for-profit institutions at this level).

In spite of these limitations, if we accept that the returns to education are roughly equal or only slightly larger for private sector students who do not transfer to four-year colleges, then our results beg the question as to whether the higher price of for-profit colleges can be justified. From a student's perspective, it would seem that given roughly similar returns, a lower-cost community

college would likely be a better choice. 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. On the other hand, students may simply value other attributes of for-profit colleges. For example, for-profits may have better student services, such as on-site childcare that may not be reflected in earnings gains. Further, some programs may be offered in the private sector that are not offered in the public sector.

The more important question, then, is not why a student might choose a private college, but whether the student's earnings gains are sufficient to offset the cost of education in the private sector. Despite its higher cost, a private sector education may still be worthwhile if the present value of the student's lifetime earnings fully offset the foregone earnings and direct costs of education (e.g., tuition, fees, books, and interest on debt) the student incurs from attending.

In Table 9 we present a back-of-the-envelope cost-benefit analysis for the average student. Under the most plausible assumptions in our base case analysis, we estimate that the earnings gains generated by completing an associate's degree in a for-profit college are 16.7 percent (from Table 2, column 7, row 2). Our analysis suggests that for the average student, the benefits exceed the costs, with a net present value of around $23,000 over a student's lifetime. However, the results are quite sensitive to our assumptions about the return and the interest rate. In our best-case scenario, an interest rate of 6.8 percent and earnings gains of 19.9 percent (Table 2, column 7, row 1 + row 2) could raise the benefits above the costs by about $48,000, while a worst-case analysis suggests that costs would exceed benefits by $6,900 assuming a 12.8 percent earnings gain (Table 6A, column 4, row 2) and a 12.8 percent interest rate. The costs could be pushed much higher still if students face

higher interest rates or finance their education solely with private loans (e.g., students in schools that are not eligible for federal financial aid programs).[23]

Our cost-benefit analysis is, admittedly, quite simplistic and we caution that we do not attempt to value the full social costs and benefits of a for-profit two-year education. Importantly, we omit the costs to taxpayers of subsidizing student loans and providing grants to students to attend for-profit colleges. We also omit the social benefits of education, such as decreased crime and improved civic engagement. Future research will assess these additional costs and benefits and compare the full social costs and benefits of a for-profit education to the costs and benefits of educating a student in a public community college.

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. Future studies should examine four-year college transfer rates and assess whether returns differ by occupation, institution size, financial aid eligibility, and other characteristics of institutions and individuals. For now, however, our estimates demonstrate that private two-year colleges on average generate positive earnings gains that are similar or perhaps slightly higher than those experienced by students in the public sector. These institutions may indeed be worth the high price for some students—particularly those that cannot find their needs met in the public sector. However, in light of the much higher cost of a degree in the private sector, it is likely that at least some students who can find similar programs in the public sector would be better served in lower-cost community colleges.

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[23] See Table 9 for detailed calculations and assumptions.

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Table 1A. Summary Statistics, Completers Sample, NLSY97

	Pre-Education			*Post-Education*		
	Public (sd)	Private (sd)	Diff (t-stat)	Public (sd)	Private (sd)	Diff (t-stat)
Weekly Earnings	$326	$327	-$1	$605	$657	-$52
	($1,443)	($925)	(0.03)	($1,912)	$1,820	(0.45)
Avg Wage	$12.01	$11.82	$0.19	$17.62	$17.89	-$0.27
	($76)	($41)	(0.10)	($45)	($51)	(0.09)
Avg Hrs Worked/Week	29.0	29.0	-0.1	35.0	36.3	**-1.23**
	(10.8)	(11.4)	(0.11)	(10.2)	(10.6)	**(1.89)**
Full Time Employment	28.4%	30.0%	-1.6%	51.3%	52.5%	-1.2%
	(45.1%)	(45.9%)	(0.99)	(50.0%)	(50.0%)	(0.43)
Any Employment	78.5%	81.8%	-3.3%	80.0%	77.6%	2.4%
	(41.1%)	(38.6%)	(2.42)	(40.0%)	(41.7%)	(1.05)
Age	19.7	19.5	0.1	24.0	23.8	0.1
	(2.6)	(2.6)	(1.18)	(1.8)	(1.9)	(1.19)

	Time-Invariant Variables		
	Public (sd)	Private (sd)	Diff (t-stat)
Male	47.3%	41.2%	**6.1%**
	(50.0%)	(49.3%)	**(3.2)**
White, Non-Hispanic	47.5%	48.9%	-1.4%
	(50.0%)	(50.0%)	(0.7)
Black, Non-Hispanic	27.0%	23.6%	**3.5%**
	(44.4%)	(42.5%)	**(2.1)**
Asian, Pacific Islander	0.6%	1.6%	**-0.9%**
	(8.0%)	(12.5%)	**(2.1)**
Hispanic	20.2%	23.1%	**-2.9%**
	(40.0%)	(42.1%)	**(1.8)**
Other Race	2.2%	1.5%	0.8%
	(14.8%)	(12.0%)	(1.6)
Foreign Born	5.0%	4.7%	0.3%
	(21.7%)	(21.1%)	(0.4)
Primary Language Not English	4.9%	5.2%	-0.3%
	(21.6%)	(22.3%)	(0.4)
Math Scores (ASVAB)	0.69	0.69	0.0%
	(0.53)	(0.51)	(0.0)
Reading Scores (ASVAB)	0.69	0.74	**-5.1%**
	(0.52)	(0.49)	**(2.5)**
Household Income	$56,550	$58,253	-$1,703
	($54,081)	($62,528)	(0.6)
No. of Individuals	531	139	

Notes: See text for detail on samples. Individuals in the samples are age 16 or older. Weekly earnings, hourly wage and hours worked per week are means across the first five jobs worked in the year.

Table 1B. Summary Statistics, Attendees Sample, NLSY97

	Pre-Education			*Post-Education*		
	Public (sd)	Private (sd)	Diff (t-stat)	Public (sd)	Private (sd)	Diff (t-stat)
Weekly Earnings	$240	$242	-$2	$454	$462	-$8
	($727)	($535)	(0.10)	($1,428)	($1,075)	(0.25)
Avg Wage	$8.53	$9.19	-$0.66	$14.09	$13.98	$0.11
	($27)	($22)	(0.99)	($60)	($36)	(0.10)
Avg Hrs Worked/Week	27.9	27.6	0.4	34.1	33.8	0.27
	(10.9)	(10.8)	(1.18)	(10.3)	(10.2)	(0.99)
Full Time Employment	24.5%	24.3%	0.2%	44.8%	45.5%	-0.7%
	(43.0%)	(42.9%)	(0.18)	(49.7%)	(49.8%)	(0.56)
Any Employment	74.5%	79.7%	-5.2%	76.5%	80.4%	**-3.9%**
	(43.6%)	(40.2%)	(4.86)	(42.4%)	(39.7%)	**(3.95)**
Age	18.6	18.5	0.1	22.6	22.7	-0.1
	(2.3)	(2.2)	(0.86)	(2.4)	(2.4)	(1.85)

	Time-Invariant Variables		
	Public (sd)	Private (sd)	Diff (t-stat)
Male	47.4%	44.1%	**3.3%**
	(50.0%)	(50.0%)	**(2.2)**
White, Non-Hispanic	46.6%	39.6%	**7.0%**
	(50.0%)	(49.0%)	**(4.8)**
Black, Non-Hispanic	26.7%	31.2%	**-4.6%**
	(44.2%)	(46.4%)	**(3.3)**
Asian, Pacific Islander	1.1%	1.0%	0.1%
	(10.4%)	(10.2%)	(0.2)
Hispanic	21.9%	25.5%	**-3.5%**
	(41.4%)	(43.6%)	**(2.7)**
Other Race	2.3%	1.9%	0.4%
	(15.1%)	(13.8%)	(0.9)
Foreign Born	5.4%	6.5%	-1.1%
	(22.6%)	(24.7%)	(1.5)
Primary Language Not English	5.9%	5.4%	0.4%
	(23.5%)	(22.7%)	(0.6)
Math Scores (ASVAB)	0.71	0.71	-0.3%
	(0.53)	(0.51)	(0.2)
Reading Scores (ASVAB)	0.71	0.72	-0.01
	(0.52)	(0.54)	(0.4)
Household Income	$49,862	$51,507	-$1,645
	($52,271)	($57,259)	(0.7)
No. of Individuals	2,033	355	

Notes: See text for detail on samples. Individuals in the samples are age 16 or older. Weekly earnings, hourly wage and hours worked per week are means across the first five jobs worked in the year.

Figure 1A. Time Pattern of Log Earnings, Completers Sample, NLSY97

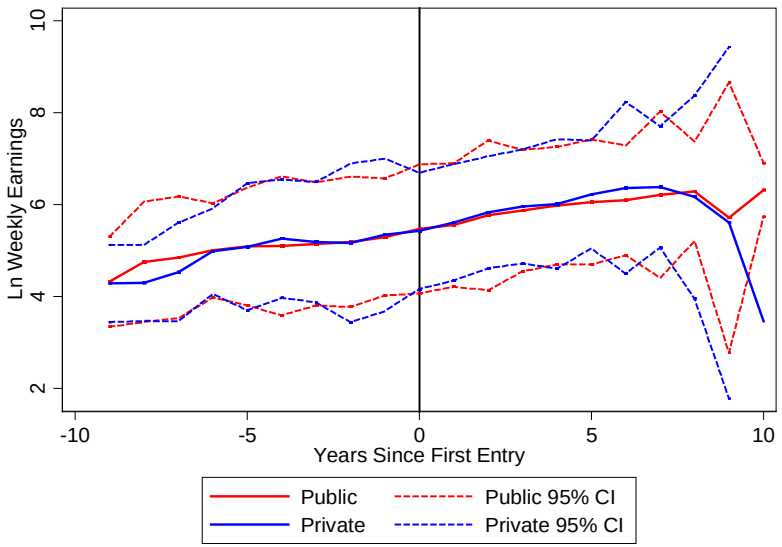

Figure 1B. Time pattern of Log Earnings, Attendees Sample, NLSY97

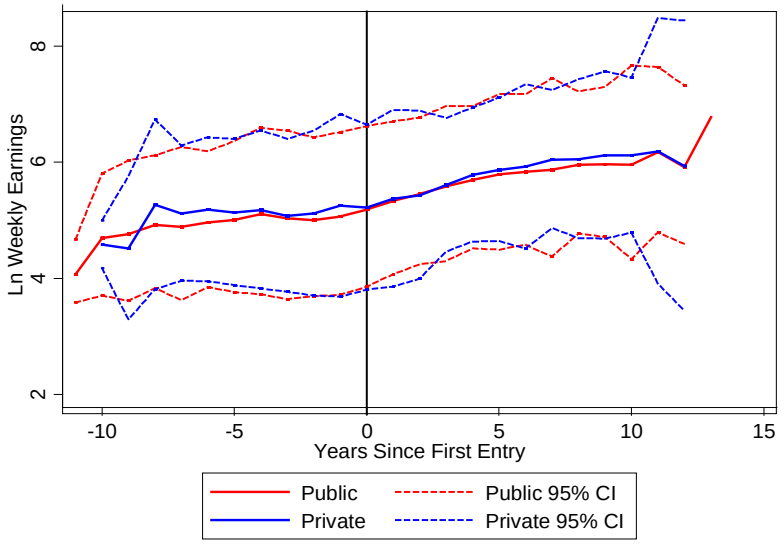

Figure 2A. Age Profile of Earnings, Completers Sample, NLSY97

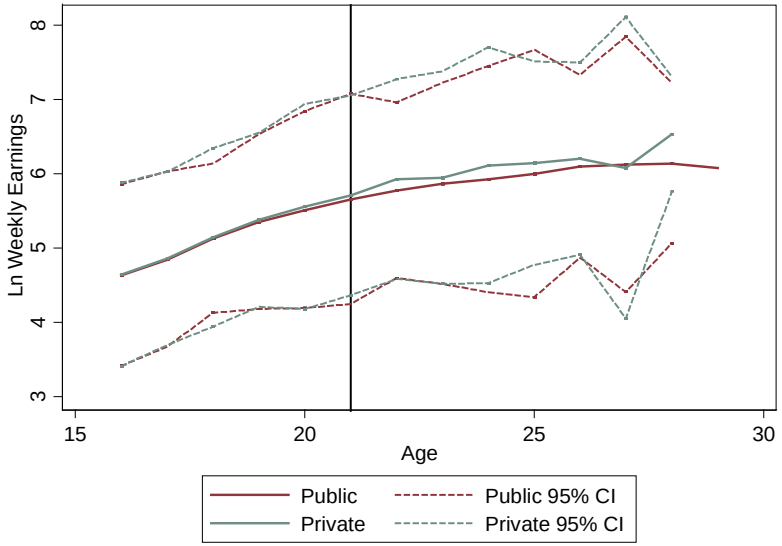

Figure 2B. Age Profile of Earnings, Attendees Sample, NLSY97

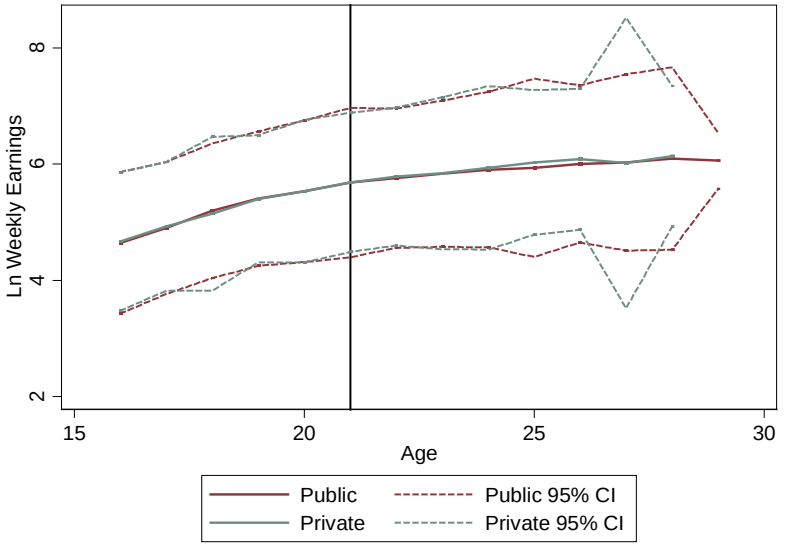

Table 2. Returns to Private Two-Year College Completion, Log Weekly Earnings, NLSY97

	Completers Sample						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Private*Post	0.108**	0.106**	0.0765*	0.105**	0.0816	0.0331	0.0320
	[0.0462]	[0.0463]	[0.0456]	[0.0522]	[0.0584]	[0.0584]	[0.0586]
Post	0.127***	0.137***	0.136***	0.123***	0.125***	0.152***	0.167***
	[0.0307]	[0.0311]	[0.0309]	[0.0330]	[0.0354]	[0.0375]	[0.0378]
Age & Age Squared	Yes	No	No	No	No	Yes	No
Age FE	No	Yes	Yes	Yes	Yes	No	Yes
Male, Region & Race FE	No	No	Yes	Yes	Yes	No	No
Ability (ASVAB scores)	No	No	No	Yes	Yes	No	No
Foreign Born, Language	No	No	No	Yes	Yes	No	No
Household Income	No	No	No	No	Yes	No	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	No	No	Yes	Yes
No. Obs.	5487	5487	5441	4236	2924	5487	5487
No. Individuals						670	670

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: Log weekly earnings are the natural log of mean earnings across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Post = 1 in the year after degree completion and every year thereafter. Estimates are conditional on employment (obs with missing earnings are dropped).

Table 3. Effects of Private Two-Year College Completion on Labor Market Outcomes, NLSY97

	Completers Sample				
	Log Wkly Earn	Log Hrly Wages	Log Hrs/Week	FT Employ	Any Employ
	(1)	(2)	(3)	(4)	(5)
Private*Post	0.0320	0.00494	0.0211	0.00832	-0.0379
	[0.0586]	[0.0488]	[0.0440]	[0.0419]	[0.0341]
Post	0.167***	0.104***	0.0679***	0.0733***	-0.0127
	[0.0378]	[0.0329]	[0.0262]	[0.0254]	[0.0213]
Age & Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
No. Obs.	5487	5487	5487	6923	6923
No. Individuals	670	670	670	671	671

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: Weekly earnings, hourly wage, and hours worked per week are means across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Post = 1 in the year after degree completion and every year thereafter. Cols. (1)-(3) are conditional on employment (obs. with missing earnings are dropped).

Table 4. Effects of Private Two-Year College Attendance on Labor Market Outcomes, NLSY97

	Attendees Sample				
	Log Wkly Earn	Log Hrly Wages	Log Hrs/Week	FT Employ	Any Employ
	(1)	(2)	(3)	(4)	(5)
Private*Post	0.00707	-0.00546	0.0122	0.0112	-0.0142
	[0.0356]	[0.0278]	[0.0239]	[0.0213]	[0.0204]
Post	0.0605***	0.0203	0.0387***	0.0290**	-0.0113
	[0.0179]	[0.0135]	[0.0123]	[0.0122]	[0.0110]
Age & Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
No. Obs.	18639	18639	18639	24448	24448
No. Individuals	2377	2377	2377	2388	2388

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: Weekly earnings, hourly wage, and hours worked per week are means across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Post = 1 in the year after the student first reported attendance and every year thereafter. Cols. (1)-(3) are conditional on employment (obs. with missing earnings are dropped).

Table 5. Effects of Private Two-Year Colleges on Pre-Education Outcomes, NLSY97

	Log Wkly Earn	Log Hrly Wage	Log Hrs/Wk	FT Employ	Any Employ
	(1)	(2)	(3)	(4)	(5)
Completers Sample					
Private*Pre-Ed	0.00382	-0.0399	0.0522	0.0551	0.0571*
	[0.0651]	[0.0518]	[0.0413]	[0.0366]	[0.0306]
Pre-Education	-0.0444*	-0.0150	-0.0274	-0.0527***	-0.0311**
	[0.0264]	[0.0193]	[0.0204]	[0.0162]	[0.0143]
No. Obs.	5487	5487	5487	6923	6923
No. Individuals	670	670	670	671	671
Attendees Sample					
Private*Pre-Ed	0.0700*	0.0256	0.0341	0.0276	-0.0122
	[0.0382]	[0.0300]	[0.0295]	[0.0246]	[0.0233]
Pre-Education	-0.0414***	-0.00523	-0.0308***	-0.0194**	-0.0156*
	[0.0149]	[0.0102]	[0.0112]	[0.00882]	[0.00910]
No. Obs.	18639	18639	18639	24448	24448
No. Individuals	2377	2377	2377	2388	2388

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include age, year, and individual fixed effects. Weekly earnings, hourly wage, and hours worked per week are means across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Pre-Education = 1 in the two years prior to first attendance (assuming two years of attendance for completers). Cols. (1)-(3) are conditional on employment (obs. with missing

Table 6. Returns to Private Two-Year College Completion, Alternative Specifications and Measures, NLSY97

	Log Weekly Earnings, 1st 5 jobs						Log Wkly Earn 1st job only	Log Wkly Earn all reported jobs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Completers Sample				
Private*Post	0.0320	0.0480	0.0192	0.0341	0.0383	0.144**	0.0356	0.0445
	[0.0586]	[0.0672]	[0.0626]	[0.0561]	[0.0693]	[0.0627]	[0.0719]	[0.0591]
Post	0.167***	0.157***	0.178***	0.128***	0.169***		0.157***	0.154***
	[0.0378]	[0.0397]	[0.0390]	[0.0401]	[0.0550]		[0.0431]	[0.0380]
No. Obs.	5487	5487	5133	5487	4353	1154	5340	5340
No. Individuals	670	670	670	670	665	139	670	670
				Attendees Sample				
Private*Post	0.00707	0.0263	0.00925	0.0228	-0.0203	0.0432	-0.00978	0.00638
	[0.0356]	[0.0392]	[0.0357]	[0.0332]	[0.0446]	[0.0422]	[0.0423]	[0.0357]
Post	0.0605***	0.0557***	0.0626***	0.0718***	0.00505		0.0750***	0.0579***
	[0.0179]	[0.0198]	[0.0180]	[0.0179]	[0.0251]		[0.0220]	[0.0183]
No. Obs.	18639	18639	17479	18639	15893	2922	18062	18062
No. Individuals	2377	2377	2376	2377	2359	354	2375	2375
Age & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Weights	No	Yes	No	No	No	No	No	No
Experience	No	No	Yes	No	No	No	No	No
Alternate Post	No	No	No	Yes	No	No	No	No
Drop Year(s) in School	No	No	No	No	Yes	No	No	No
Privates Only	No	No	No	No	No	Yes	No	No

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include age, year, and individual fixed effects. In cols. (1)-(6) log weekly earnings is the natural log of the means across the first five jobs worked in the year. Col. (7) uses the first job only and col. (8) uses all jobs (10 max). Private = 1 if the individual attended a private institution. Post = 1 in the year after degree completion and every year thereafter in the completers sample, and = 1 in the year after the student first reported attendance and every year thereafter in the attendees sample. Sample weights are the cumulative cases weights provided in the NLSY97. Experience = age-schooling-6. "Alternate Post" counts the year of graduation as Post in the completers sample and counts two years after first attendance as Post in the attendees sample. Estimates are conditional on employment (obs. with missing earnings are dropped).

Table 7. Effects of Private Vocational Degree/Certificate Completion on Labor Market Outcomes, NLSY97

	Completers Sample + Vocational Degree/Certificate Holders				
	Log Wkly Earn	Log Hrly Wages	Log Hrs/Week	FT Employ	Any Employ
	(1)	(2)	(3)	(4)	(5)
Private*Post	0.0426	0.0149	0.0252	0.0207	-0.0182
	[0.0546]	[0.0460]	[0.0409]	[0.0400]	[0.0330]
Private*Voc*Post	-0.0765	0.00994	-0.0648	-0.0445	0.00831
	[0.0789]	[0.0632]	[0.0548]	[0.0529]	[0.0462]
Post	0.192***	0.102***	0.0898***	0.0727***	-0.0466***
	[0.0326]	[0.0276]	[0.0236]	[0.0217]	[0.0179]
Voc*Post	-0.146***	-0.0666**	-0.0772***	-0.0777***	-0.0227
	[0.0346]	[0.0288]	[0.0249]	[0.0227]	[0.0193]
Age & Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
No. Obs.	20435	20435	20435	26889	26889
No. Individuals	2585	2585	2585	2596	2596

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: Weekly earnings, hourly wage, and hours worked per week are means across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Post = 1 in the year after degree completion and every year thereafter. Voc = 1 if the student completed a vocational degree or certificate program. Cols. (1)-(3) are conditional on employment (obs. with missing earnings are dropped).

Table 8. Effects of Private Two-Year Colleges on Labor Market Outcomes, NLSY79

	Log Wkly Earn	Log Hrly Wage	Log Hrs/Wk	FT Employ	Any Employ
	(1)	(2)	(3)	(4)	(5)
NLSY79 Completers Sample					
Private*Post	0.0216	-0.0187	0.0290	0.0289	0.0128
	[0.0773]	[0.0632]	[0.0461]	[0.0340]	[0.0411]
Post	0.246***	0.106***	0.130***	0.0507***	-0.00328
	[0.0388]	[0.0259]	[0.0273]	[0.0151]	[0.0164]
No. Obs.	7307	7307	7307	18118	18118
No. Individuals	820	820	820	833	833
NLSY79 Attendees Sample					
Private*Post	0.0187	-0.000144	0.0223	0.00204	-0.00165
	[0.0494]	[0.0338]	[0.0310]	[0.0211]	[0.0225]
Post	0.0447**	0.0387***	0.0136	0.0194**	0.0287***
	[0.0207]	[0.0141]	[0.0123]	[0.00808]	[0.00882]
No. Obs.	27453	27453	27453	73894	73894
No. Individuals	3245	3245	3245	3397	3397

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include age, year, and individual fixed effects. Weekly earnings, hourly wage, and hours worked per week are means across the first five jobs worked in the year. Private = 1 if the individual attended a private institution. Post = 1 in the year after degree completion and every year thereafter. Cols. (1)-(3) are conditional on employment (obs. with missing earnings are dropped). All specifications

Table 9. Student-Level Cost-Benefit Analysis of Private Two-Year College Completion

BENEFITS		RANGE
PV of gains in lifetime earnings for private college AA completers	**$90,122**	
Return to completing private degree, from Table 2 (col. 7)	16.70%	[12.8, 19.9]
Annual average earings for those with only a high school diploma	$31,300	
Years of work	35	
Discount rate	3%	
Total Benefits	**$112,316**	**[$86,086, $133,837]**
COSTS		**RANGE**
PV of foregone earnings	**$61,688**	
Annual average earings for those with only a high school diploma	$31,300	
Years of earnings foregone	2	
Discount rate	3%	
Tuition, fees, and other educational expenses	**$19,681**	
PV of interest on loan	**$5,470**	
Principal	$19,681	
Interest rate	9.80%	[6.8, 12.8]
Origination fee	1%	
Years to repay	10	
Discount rate	3%	
Total Costs	**$89,141**	**[$85,615, $92,950]**
NET PRESENT VALUE	**$23,174**	**[-$6,864, $48,222]**

Notes: The range represents the range of values of our most sensitive parameters that we use to calculate a best- and worst-case scenario. The assumptions used in each calculations are listed below the cost/benefit. Tuition, fees, and other educational expenses are assumed to be equal to the average amount borrowed by associate's degree students in for-profit institutions (Finaid.org 2010). Foregone earnings are based on the average annual earnings of students with only a high school diploma reported by the Bureau of Labor Statistics (2010). Our best case interest rate assumes a student borrows only from federal student loan programs at a fixed interest rate of 6.8 percent, as in the Stafford Loan Program (U.S. Department of Education 2010). Our worst case interest rate adds 6 percentage points to reflect what we believe to be a reasonable rate on private loans (Finaid.org 2010). Our base case takes the average of the two. We apply a 3 percent discount rate to all future costs and benefits.