# The Benefits of Alternatives to Conventional College: Comparing the Labor-Market Returns to For-Profit Schools and Community Colleges

*Christopher Jepsen,[*] Peter Mueser, Kenneth Troske and Kyung-Seong Jeon*

**Abstract:**

This paper provides novel evidence on the labor-market returns to for-profit postsecondary school and community college attendance. We link administrative records on college attendance with quarterly earnings data for nearly 400,000 students in one state. Five years after enrollment, quarterly earnings conditional on employment exceed earnings in the absence of schooling by 20-29 percent for students attending for-profit schools and 16-27 percent for students attending community colleges. In aggregate, the benefits of attendance generally exceed the costs in both for-profit schools and community colleges. Our analyses suggest the two types of schools serve very different markets, both in terms of the characteristics of students and the fields they study. When we perform matching analyses with comparable students in comparable fields, we do not find that returns are consistently higher in for-profit schools or community colleges.

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[*] Corresponding author. Affiliations: Jepsen, University College Dublin, CES-Ifo, and IZA; Mueser, University of Missouri and IZA; Troske, University of Kentucky and IZA; and Jeon, University of Missouri. We thank Todd Elder, Lance Lochner, Sandra McNally, Lesley Turner, and seminar participants at IZA, the Stockholm School of Economics, the University of Jyväskylä, the London School of Economics, the Society of Labor Economists/European Association of Labor Economists annual conference, Colorado State University, Grinnell College, Lancaster University, the European Society for Population Economics annual conference, the Centre for Vocational Education Research annual conference, the Education Finance and Policy annual conference, the BIBB Conference on the Economics of Vocational Training, the CES-IFO Institute of Economics of Education conference, and the 20th IZA/SOLE Transatlantic meeting for useful comments. We thank the European Commission (Marie Curie Career Integration Grants) and the Spencer Foundation (Small Grants) for financial assistance. Several individuals in the state Department of Higher Education provided invaluable assistance in procuring and interpreting the school data. All opinions and errors are the authors' sole responsibility.

## I. Introduction

The relative earnings for high-school graduates have declined substantially over the last few decades, and, prior to the onset of the pandemic, job opportunities for less-skilled workers were becoming more limited. U.S. states have drastically reduced funding for higher education (Phelan 2014). In response, between 2000 and 2010 enrollment in for-profit colleges[1] (also known as proprietary schools) increased by 350 percent while enrollment in public higher education institutions rose by less than 30 percent (Snyder, de Brey, and Dillow 2019). Since 2010, total enrollment has fallen by 42 percent in for-profit schools and 20 percent in public two-year schools.

At the same time, the U.S. government, along with several states, increased oversight of the for-profit school industry in response to "abusive practices," such as false promises to students of future earnings and employment opportunities (U.S. Department of Education 2015). Under the Obama administration, the Department of Education developed "Gainful Employment" regulations designed to deny federal loans to students attending for-profit schools whose graduates did not attain certain earnings or whose debt exceeded specified levels. The Trump administration rescinded these rules, but they were reinstituted under the Biden administration and became effective July 1, 2024. In the midst of this ongoing controversy on the effectiveness of for-profit schools as well as the decline in funding for public schools, evidence on whether these schools improve labor-market outcomes and what type of school is more efficient at improving outcomes is critical for deciding the level of public support and oversight

[1] The majority of for-profit colleges are vocational institutions that provide associate's degrees (up to two years of full-time study) or less-intensive certificates. Community colleges are public schools that provide vocational associate's degrees and certificates, but they also provide non-award enrichment classes, course credit to be transferred to other institutions toward four-year degrees, as well as academic associate's degrees. See Jepsen, Troske, and Coomes (2014) for more information on community colleges and the awards offered.

that for-profit schools and community colleges should face.

We estimate the returns to for-profit schools and public community colleges using administrative data for over 100,000 students who enrolled in for-profit schools and over 290,000 students who enrolled in public community colleges in one state between 2005 and 2012. Following the returns to schooling literature, we exclude students who transfer to a four-year school. We complement previous studies using national data by including a broader set of for-profit schools and students, specifically schools that do not receive U.S. federal aid, as well as students who do not receive federal financial aid.

Our preferred model is a two-step model that uses person fixed effects and calendar quarter in the first stage to predict earnings in the pre-enrollment period. This model addresses recent concerns with fixed-effects models (documented in de Chaisemartin and D'Haultfoeuille 2020; Borusyak, Jaravel, and Speiss 2024; and elsewhere) and allows the earnings increment resulting from program participation to vary with time since enrollment. Given that our results show that the increment in earnings varies dramatically with the time since schooling, our approach provides a more meaningful set of estimates than papers that attempt to provide a single earnings increment, which are expected to vary by the period of coverage (e.g., Cellini and Turner 2019). We follow the guidance in Elder, Goddereeis, and Haider (2010) and Słoczyński (2020) and estimate separate models by gender, school type, and credential type rather than pool these groups. The motivation for using such flexible models is that for-profit schools and community colleges have large differences in both demographics and the areas of study for students, suggesting that they serve distinct markets. However, for comparison, we also present estimates from matching estimators that compare similar students and areas of study in for-profit schools and community colleges and therefore exclude large numbers of students.

We find sizable earnings returns to for-profit schools and community college attendance as predicted from a base period 5 to 24 quarters before enrollment. For students attending for-profit schools, by the fifth year after entry (17-20 quarters after enrollment), quarterly earnings conditional on employment exceed earnings in the absence of schooling by 21 to 24 percent for students seeking certificate programs and 20 to 29 percent for associate's degree programs. For community college students, the earnings increases are 20 to 27 percent for those seeking certificates and 16 to 19 percent for associate's degrees. Estimated effects on employment are much more modest, ranging between 0 and 4 percentage points in the fifth year after entry.

We also find heterogeneous impacts on earnings over time since attendance. For both for-profit schools and community colleges, the earnings increment increases at least through 15 quarters and often to the end of our estimation period of 25 quarters. However, the pattern for community colleges shows a faster initial increase, with returns in quarters 3-10 generally exceeding those in for-profit schools. However, the growth in for-profit schools generally continues for longer, so the increment is as high or higher by 20 quarters. For students seeking associate's degrees, the increase in earnings at the end of our data is larger for attendance in for-profit schools than community colleges. For students seeking certificates, the relative returns are mixed.

We document how differences in our methods account for the differences between our estimates and previous estimates in the literature. In particular, we find that when we estimate models similar to those used in previous studies (e.g., Cellini and Turner 2019) using more restrictive models that pool data and constrain the patterns of effects over time, we replicate the findings of a larger return to community colleges. We believe our more flexible models provide a more accurate characterization of the differences in the long-run impacts of attending the two

school types. More generally, our results also demonstrate the importance of allowing treatment effects to vary over time and across individuals when trying to characterize the effect of programs designed to increase human capital.

Overall, our results suggest that students attending both for-profit schools and community colleges, on average, experience gains in earnings that outweigh the costs. Although one can certainly point out issues with both for-profit schools (high tuition) and community colleges (low completion rates), both types of schools appear to produce value for those who attend, making it difficult to argue that eliminating one type of school would lead to a Pareto-improving outcome in this market.

## II. Relation to Previous Literature

Research on the labor-market returns to for-profit schools falls into three categories: (1) studies using nationally representative data sets such as the Beginning Postsecondary Survey (BPS) (Deming, Goldin, and Katz 2012; Lang and Weinstein 2013; Cellini and Chaudhary 2014; and Liu and Belfield 2014);[2] (2) studies using administrative data (Liu and Belfield 2013; Cellini and Turner 2019) and (3) audit studies (Darolia et al. 2015; and Deming et al. 2016).[3] Cellini and Koedel (2017) and Cellini (2021) review this literature and conclude that for-profit colleges generally have lower returns than public colleges, whereas Gilpin and Stoddard (2017) interpret the findings as inconclusive.

The most prominent recent paper in this area is by Cellini and Turner (2019). They use administrative data from the U.S. Department of Education to study labor-market returns to

[2] Due to the small sample of students attending for-profit schools, Chung's (2008) analysis of the labor-market returns of attending for-profit schools using data from the National Education Longitudinal Study is inconclusive.
[3] Armona, Chakrabarti, and Lovenheim (2022) focus on financial aid and student loan debt, although they provide some estimates of the returns for students attending for-profit schools using both survey and administrative data. Results generally suggest returns are smaller for-profit schools, but most estimates are quite imprecise.

certificates in for-profit colleges among the subset of students who receive federal aid under Title IV of the Higher Education Act of 1965. Their approach matches students seeking certificates in for-profit schools with those in public community colleges and finds that earnings are lower for the former group.

Our first contribution to the existing literature using national data is that we exploit data with a much broader set of for-profit schools and students in one state. Survey datasets have the advantage of covering the entire country, but they have relatively small numbers of respondents attending for-profit schools.[4] The national data used by Cellini and Turner (2019) include only schools eligible for Title IV federal assistance, and they have data only on students who receive federal aid. Many for-profit schools offer only certificates and do not participate in federal government programs.

Second, we use a more flexible model specification to avoid the well-documented biases often present in two-way fixed-effects models, and those that pool data sets when returns are heterogeneous. We find that results are sensitive to more constrained specifications such as those that pool data for community college and for-profit students and estimate the returns to schooling as a single, time-invariant post-schooling dummy variable. These less flexible models have potential biases because they fail to account for heterogeneous returns and population differences between students attending for-profit schools and community colleges.

Third, we present results both for those seeking certificates and those seeking associate's degrees for for-profit schools and community colleges, extending the work by Cellini and Turner (2019) looking solely at certificates. Thus, we can compare returns for each award separately by

[4] BPS data, for example, include only first-time students and are limited to students attending Title IV eligible institutions. Deming, Goldin, and Katz (2012) and Lang and Weinstein (2013) acknowledge that many students in for-profit colleges have previously attended postsecondary education.

gender.

Finally, we present evidence showing that students in for-profit schools and community colleges pursue different credentials and focus on different areas of study. We undertake comparisons in returns for the small number of students who are comparable—in terms of personal characteristics and area of study—and find that although there are differences, they do not change our bottom line. However, we suggest that the two types of schools serve distinct markets, so that the comparison does not represent choices faced by many individuals.

## III. Data

Our analyses examine the enrollment and earnings returns for students who entered for-profit post-secondary schools or public community colleges located in Missouri from January 2005 to December 2012. To estimate counterfactual earnings, we also use data on pre-entry earnings for individuals who entered school as late as December 2015. For more information on the data, see Jepsen et al. (2021).

For-profit schools with a physical presence in the state must provide student-level data as part of Missouri's Proprietary School Certification Program.[5] As in most states, the set of schools includes campuses of national institutions such as the University of Phoenix as well as local institutions focusing on one or two subjects such as truck driving academies.[6] The data are not limited to schools that receive Title IV funding from the U.S. government or to students who receive federal financial aid. In total, our analysis includes data from 133 for-profit schools in the

[5] Although, in theory, nonprofit private schools could be included in the program, almost all were exempted in practice. For a discussion of the program's requirements, see http://dhe.mo.gov/psc/. We were able to identify the for-profit or not-for-profit status of all schools in the program that accepted students in 2010, and 99 percent attended private for-profit institutions; private nonprofit institutions accounted for the remainder. We refer to these schools as for-profit schools to be consistent with the literature.

[6] Although we do not have data on schools that are on-line only, discussions with state education officials suggest that very few on-line only schools exist during this time period. We found no evidence of any on-line only for-profit colleges in the Integrated Postsecondary Education Data System (IPEDS).

state, 55 of which do not receive Title IV funding. Although we know of no comprehensive listing that would allow us to identify whether all for-profit schools within the state are included, the analyses here are based on a more comprehensive listing of for-profit schools – for one state – than that used in any previous analyses. The state's Enhanced Missouri Student Achievement Study (EMSAS) records provide information about individuals who enroll in any of the state's 14 community colleges.

Our analysis focuses on spells of enrollment, where a spell is a period of participation in either a for-profit school or a community college, allowing for periods of non-enrollment of less than a year within a spell. The sample is limited to spells for students who specify that they are seeking certificates or associate's degrees, as the goal of the paper is to measure returns for students pursuing these awards rather than bachelor's or graduate degrees. Consistent with recent studies of community colleges, we exclude spells where students attend a public four-year educational institution in the state anytime between the beginning of a spell and two years following the end of the last spell of enrollment, or those who attend both for-profit schools and community colleges during the period of our study. Nearly a third of spells in community college are omitted because the credential sought is "other," often for students who plan to transfer to a four-year school; for for-profit schools, 13 percent of spells are omitted for students who were seeking other credentials, nearly all of which are bachelor's or master's degrees.[7] Approximately 16 percent of for-profit student spells and 2.2 percent of public college spells are omitted because they indicate at the time of enrollment that they are not permanent residents of Missouri or

[7] This selection implies that our analysis omits the benefits obtained by a student who chooses to attend community college as a step toward obtaining a four-year degree, a potentially important service provided by community colleges.

Kansas, the states for which we have administrative earnings data.[8] These sample exclusions are standard in the literature studying returns to community college, such as Jepsen, Troske, and Coomes (2014); Stevens, Kurlaender, and Grosz (2019); and elsewhere. Appendix Table A1 shows the impact of these sample restrictions.

For each student, the data contain the specific school attended, the Classification of Instructional Programs (CIP) code with the student's intended field of study, and—for award recipients—the type of degree or certificate received. Appendix Table A2 shows the relationship between two-digit CIP code and our aggregated field of study. Among the most popular CIP codes are 51 (Health Professionals), 49 (Transportation), and 24 (Liberal Arts).

These data are matched with administrative data on earnings from the Missouri and Kansas Unemployment Insurance (UI) programs, which provide information on quarterly earnings for the overwhelming majority of workers who live in these states.[9] We have adjusted all earnings for inflation, with 2010 as the base year. Despite excluding some types of earnings such as self-employment and federal jobs, program effects on employment and earnings based on wage records are generally comparable to those obtained in surveys, at least in the context of worker training programs (Kornfeld and Bloom 1999) and welfare programs (Wallace and Haveman 2007).

Our outcome analyses use quarterly earnings information from the first quarter of 1999 through the third quarter of 2014. Thus, we have data for at least five years prior to school attendance and at least seven quarters after initial enrollment in a for-profit school or community

[8] We keep students who do not specify a state of permanent residence (primarily community college students) because we find that the proportion of these students who have earnings reported in our UI wage record data is similar to that of students who report living in Missouri or Kansas.

[9] Although the St. Louis metropolitan area is on the border with Illinois, the proportion of Missouri residents who work in Illinois is small. Within the metropolitan area, only 16 percent of private sector jobs were in Illinois in 2012 (www.bls.gov/news.release/cewqtr.toc.htm), and these jobs were mostly held by Illinois residents.

college. The resulting data set is a panel of student entries and time periods. We exclude quarters where the individual is under the age of 18 or over the age of 60 at any time during the quarter, as well as any quarter of earnings more than 24 quarters prior to program entry or more than 25 quarters after program entry. We also exclude all observations for individuals where age or Social Security number are missing. As shown in Table A1, the number of individuals omitted for these latter reasons is modest.

Although our data pertain only to those attending for-profit schools and community colleges in Missouri, the state is typical of the U.S. The industrial structure is similar to that of the U.S. as a whole, and earnings and wages were about 10 percent below the U.S. average at the time of our observed earnings. The proportion Hispanic is substantially below the U.S. average but similar to that of most states. Because the state is representative of the nation in many respects, the results provide estimates that are plausible for many parts of the country.

## IV. Descriptive Comparisons of For-Profit Schools and Community Colleges

*Descriptive Statistics*

Table 1 contains the descriptive statistics for the analysis sample of over 100,000 entries for students into the state's for-profit schools and nearly 300,000 entries for students into the state's community colleges between January 2005 and December 2012. We provide statistics separately by gender, type of school and degree program.[10]

To summarize, the numbers in Table 1 show that students in for-profit schools differ from students in community colleges on several dimensions such as race, age, and education. For example, the mean age ranges from 24.8 for men pursuing associate's degrees in community

[10] As noted above, our analyses are based on spells of school enrollment. A student with more than one enrollment spell will appear multiple times. Table A1 indicates that the number of students with more than one spell is modest.

colleges to 32.7 for men pursuing certificates in for-profits.[11] Black students, high school dropouts, GED recipients, and students in large metro areas are over-represented in for-profit schools. The majority of for-profit students pursue certificates, whereas the vast majority of community college students pursue associate's degrees. Large differences also exist in field of study: men in community colleges are much more likely to study academic subjects than men in for-profits, and women in for-profits are much more likely to study in health fields than women in community colleges.

*Dissimilarity Index*

To look in more detail at the differences between the two school types, Table 2 provides tabulations of the index of dissimilarity between for-profit schools and community colleges by gender and credential sought for characteristics of students and field of study. This index varies from 0, implying that the distribution across categories is identical for for-profits and community colleges, to 1, in which no cases are in the same category. It can be interpreted as the proportion of for-profit students (or community college students) who would need to move between categories to make the two distributions the same.[12] We see that, for men seeking certificates, the index of dissimilarity for field of study is 0.390, implying that 39 percent of the students would have to be shifted from one field to another for the field of study distributions to be the same. Returning to Table 1, the reason for this large value is clear. The two most popular fields of study for men in certificate programs at for-profit schools are trades and transport, with around 60 percent of men seeking a certificate in one of these two fields, in contrast to certificates in community college, where these two fields together account for only 21 percent of students.

[11] Given our sample sizes, even differences as small as a percentage point and differences of a year in age are statistically significant at conventional levels. Hence, any difference of importance is statistically significant.
[12] The formal definition of the index of dissimilarity is provide in the note to Table 2.

For men seeking an associate's degree, the index of dissimilarity for field of study between for-profit and community college students is 0.673. This statistic reflects the fact that, in for-profit schools, 36 percent are in the vocational field and 28 percent are in the computer field, whereas in community colleges, 70 percent of students are in "academic/other." For women, the corresponding index is 0.736. Among women in for-profit schools, the health field is by far the most popular field of study regardless of degree program, with 77 percent of women seeking a certificate program in this field and 64 percent of women seeking an associate's degree in this field. For women in community colleges, health also is the most popular for certificates, but it is much smaller (44 percent), whereas the other/academic field is by far the most popular for associate's degrees (79 percent).

The figures in Table 2 imply that there are substantial differences by race and age in the distribution between the two types of schools, although these are smaller than differences in field. The distribution of students seeking certificates across the 13 regions of the state (based on school location) is quite discrepant for both men and women, and nearly half the students in a school type would need to be reassigned to another region to provide equal shares. In contrast, regional differences for associate's degree seekers are less extreme, with comparable figures between a fifth and a quarter.

The final line of Table 2 provides the index of dissimilarity based on a logit estimate of the probability that a student (in a given gender-credential group) is in a for-profit school rather than a community college using all the measures listed in Table 1, in addition to prior earnings and employment, and year of entry into school. The index of dissimilarity is based on deciles of the propensity score in the full sample. The index of dissimilarity is over 0.7 in every case, with a maximum value of 0.86 for women seeking associate's degrees. This value is our best overall

indicator of the extent of differences in the types of students served and type of training in for-profit schools and community colleges. It tells us for-profits and community colleges appear to operate in different parts of the higher education market, with little if any competition. Although it is of interest to consider how outcomes compare for students in for-profit schools and community colleges, this result suggests that few students can actually shift from one kind of school to the other.

## V. Methods

To estimate labor-market returns, we follow the literature and compare the post-schooling earnings of an individual with the pre-schooling earnings of the same individual (Cellini and Turner 2019; Cellini and Chaudhary 2014; Jepsen, Troske and Coomes 2014; Belfield and Bailey 2017). In effect, the comparison group and the treatment group (to use experimental terminology) consist of the same individuals, so most of the measured and unmeasured factors that influence earnings are the same. Using pre-schooling time periods as controls is plausible in our data because the vast majority of students are age 20 or older when they initially enroll.

Recently, several papers have documented limitations and biases in single-equation models that control for two-way fixed effects (e.g., de Chaisemartin and D'Haultfoeuille 2020; Borusyak, Jaravel, and Spiess 2024). Such models may produce estimates of the effect of an observed intervention that are seriously misleading where true effects are heterogeneous. It was previously assumed that estimated impacts would be within the range of true effects, but it has now been shown that estimates are not bounded by true effects. To address these concerns, we estimate a model corresponding closely to that recommended by Borusyak, Jaravel, and Spiess

(2024).[13]

Following Cellini and Chaudhary (2014) and others, we focus on quarters with positive earnings. Given the heterogeneity in demographics and field of study, we follow best practice and fit the model separately by gender, type of school (for-profit versus community college), and program (certificate versus associate's degree), yielding eight sets of estimates.[14] Although the fixed-effects model adjusts for individual differences, we also control for the effects of calendar quarter-year and age in order to predict the earnings that an individual would have obtained following enrollment if he or she had not enrolled.[15]

The model is estimated in multiple steps. In the first step, we obtain estimates of parameters using earnings for all individuals prior to enrollment. In particular, we undertake such estimation using earnings 24 to five quarters prior to enrollment for all individuals who began participation over the period 2005 through 2015. The fixed-effects model fits the following multivariate regression:

$$(1) \qquad LNEARN_{it} = \delta \cdot AGE_{it} + \eta_i + \tau_t + \varepsilon_{it}.$$

In this equation, *i* denotes a person and *t* denotes a quarter-year. *LNEARN* is the natural logarithm of total reported UI earnings across all jobs for the quarter. Quarters with no reported UI earnings are excluded. *AGE* is the individual's age in years (measured as a decimal to the nearest month), represented by a cubic. The parameter $\eta$ is a set of person fixed effects, capturing all person-specific components that are constant over time, such as race/ethnicity or innate

[13] The only difference is that our first stage estimates are based on a slightly different sample than our final stage estimates, whereas Borusyak, Jaravel, and Spiess (2024) assume the two samples are the same.
[14] See Elder, Goddeeris, and Haider (2010) and Słoczyński (2020) for arguments against pooling data in the presence of heterogeneous treatments.
[15] All time-invariant personal characteristics as well as field of study are captured in person fixed effects. However, in matching analyses reported below that compare for-profit and community college students, we match on these measures to adjust for differences in these characteristics.

ability. The model also contains a set of dichotomous variables to control for each calendar quarter-year ($\tau$). The last component ($\varepsilon$) is the error term.

Based on Equation 1, we construct counterfactual earnings for each individual for quarters beginning four quarters prior to enrollment. In particular, for an individual *i*, we specify:

(2) $$\widehat{LNEARN}_{it} = \hat{\delta} \cdot AGE_{it} + \hat{\eta}_i + \hat{\tau}_t.$$

Because we estimate Equation 1 using earnings five or more quarters prior to program entry, and the latest entry date available is at the end of 2015, the most recent earnings available are for quarter 3 in 2014. The reason for estimating this model is to use estimates of the quarter-year and age coefficients that cannot, by construction, be influenced by earnings subsequent to enrollment.

In the final step, we fit the following equation for all entries occurring from 2005 through 2012:

(3) $$LNEARN_{it} - \widehat{LNEARN}_{it} = \alpha \cdot ENROLL_{it} + \beta \cdot ENTRY_{it} + \varepsilon_{it}.$$

*ENROLL* is a variable equal to one for each internal quarter in which the individual is enrolled in school and a value of one-half for the first quarter and last quarter of school enrollment.[16] Because the school entry and exit dates are unlikely to coincide perfectly with the calendar quarter, we assume that individuals spend only half of those quarters enrolled in school.

The vector *ENTRY* contains a set of dichotomous variables measuring time relative to entry into schooling, beginning four quarters prior to the date of entry through quarter 25 following entry. Hence, we include a variable for the fourth quarter before enrolling, a variable for the third quarter before enrolling, extending through the twenty-fifth quarter after initial

[16] For approximately 18 percent of for-profit students, the exit date is missing. For these students, we assign an exit date that is 365 days after the entry date. In the results section, we discuss the robustness of the results to the exclusion of $ENROLL_{it}$, as well as of excluding students with missing exit dates. By construction, exit date is never missing for community college students, as spells are based on semesters of participation.

enrollment. The variables for the four quarters before enrollment are included to capture the possibility of an "Ashenfelter dip" in earnings in the quarters immediately before enrollment, as Jepsen, Troske, and Coomes (2014) document large dips in earnings immediately prior to community college attendance. The reference period or omitted category is the set of quarters more than four quarters before enrollment. The coefficients report the difference in earnings for the specified quarter relative to quarters more than one year before school entry, taking account of age and calendar quarter effects. Intuitively, as this approach assures that the parameters predicting earnings estimated in Equation 1 are not influenced by observed post-schooling earnings, the bias in single-equation models, which results from the relationship between the fixed effects and heterogeneity in post-schooling returns, no longer occurs.

As mentioned previously, we have earnings data from the first quarter of 1999 through the third quarter of 2015. Because we exclude observations more than 24 quarters before program entry and more than 25 quarters after program entry, we have up to 50 quarters of earnings observations per person. We look at spells of attendance rather than completion of a degree or certificate in order to avoid endogeneity concerns associated with non-random completion, as noted in Cellini and Chaudhary (2014).

The quarterly variables for the period after initial enrollment provide a flexible way to capture the returns to attendance, similar to the seminal work on returns to community colleges (Jacobson, LaLonde, and Sullivan 2005a, 2005b) as well as recent work (Jaggars and Xu 2016; Bahr 2016; Minaya and Scott-Clayton 2022). Unlike the estimator in much previous work on for-profits, we do not constrain the earnings to have any specific parametric relationship with the time since enrollment. As noted above, we do not pool the data, either by gender, school type, or program type, to avoid constraining the estimates.

Because the sample includes only individuals who attend for-profit schools or community colleges, identification of the effects in post-participation quarters derives from a comparison with earnings for quarters at least a year prior to participation and by the assumption that, given controls for age and calendar quarter, the patterns of schooling returns are similar for those beginning their attendance at different ages and in different periods. Under these 'parallel trends' assumptions, Borusyak, Jaravel, and Spiess (2024) show that this multi-step estimator is efficient, even when all observations are eventually treated. As further evidence that having data on non-treated individuals is not necessarily essential for identification, Stevens, Kurlaender, and Grosz (2019) and Jepsen, Troske, and Coomes (2012) find similar returns for community college awards in models that exclude dropouts and models that includes them.

By design, our structure is only relevant for individuals with observed earnings. In other words, our earnings estimates understate the contribution of enrollment to overall earnings if attendance increases the likelihood of employment. As a complement to these analyses, we also fit a model that predicts expected employment:

$$(4) \qquad EMP_{it} - \widehat{EMP_{it}} = \alpha \cdot ENROLL_{it} + \beta \cdot ENTRY_{it} + \varepsilon_{it}.$$

Employment (*EMP*) is a dichotomous variable equal to one for individuals with observed earnings in the quarter. Here $\widehat{EMP_{it}}$ is estimated following the structure of Equations 1 and 2 above. We estimate the model as a linear probability model.

To account for the possibility that individuals may have left Missouri and Kansas and thus have no UI earnings, we have fitted the above employment model on a sample that omits quarters if we observe no employment for an extended period through the end of our earnings data. In particular, if we observe no earnings in quarter 30 (or the last quarter for which earnings data are available, if earlier), and the continuous string of quarters with no earnings subsequent

to initial enrollment is at least 10 quarters in length, we omit this string of quarters from the analysis. Grogger (2012) recommends this approach while acknowledging the limitations. Most of our discussion will focus on earnings conditional on employment.

For all estimates listed above, we have calculated bootstrap standard errors. We repeatedly sample individuals in our analysis sample (not quarterly observations) with replacement, and for a given replication we perform each of the steps to obtain estimates for that sample. Based on 1000 replications, we report the standard deviation across effect estimates as the bootstrap standard error.

## VI. Results

*Mean Earnings Relative to College Entry*

Figures 1a and 1b (for men and women, respectively) present the trends in average earnings by quarter relative to quarter of school entry, where quarter zero denotes the quarter of initial enrollment. Individuals with no reported earnings in the states' employment records are coded as having zero earnings for the quarter, so the reported means are not conditional on employment.[17]

As Figure 1a shows, men in certificate programs have noticeably higher earnings than men in associate's degree programs for both for-profit schools and community colleges. This gap is primarily because the former are four to five years older when they begin their studies. All groups experience an "Ashenfelter dip" in earnings immediately prior to the time of school entry, as well as reduced earnings following the entry quarter, often called a "lock-in" effect, reflecting participation in school. For three of the four groups earnings growth is higher in the post-entry period than the pre-schooling period, so average earnings exceed their pre-schooling levels by

[17] However, as mentioned previously, we exclude strings of quarters of length 10 or more after initial enrollment where no earnings are observed in quarter 30, or to the end of our observation window if prior to that point.

the end of our observation period. The highest average earnings, observed 25 quarters after enrollment, are approximately $6,000 per quarter for the community college certificate program, and the lowest, $4,300, are for the for-profit associate's degree program.

For women, average earnings for those in community college programs are somewhat higher than for those in for-profit programs, both prior to enrollment and in later periods. Participants in both school types and degree programs experience large increases in average earnings during the first few post-entry quarters; the rate of growth is more modest in later periods for for-profits. Average earnings are usually highest for the community college certificate programs ($4,600 in quarter 25); earnings for the for-profit programs are lower (under $3,600).

For seven of the eight groups, these trends in average earnings suggest positive impacts of participation in both for-profit schools and community colleges. The partial exception is men seeking certificates in for-profit schools, for which earnings only recover to their prior level. Of course, these patterns do not take account of calendar time or individual age, nor do they control for individual fixed effects, so the time pattern of the enrollments and variations in the economy may well bias results. We now turn to estimates of the return to school attendance based on the model described above.

*Effects on Earnings*

Figures 2a and 2b contain the earnings regression results for the model depicted in Equation 3 for men and women, respectively, with separate regressions for each of the four lines in each figure. The figures show the earnings gains (relative to predicted earnings) to attendance for individuals pursuing certificates (dashed lines) and associate's degrees (solid lines) for each

quarter beginning four quarters before entry to 25 quarters after entry.[18] The lines indicate the estimated increment in log earnings in that quarter relative to the period from 24 to five quarters before entry (the reference period), controlling for age and year-quarter.[19] Finally, note that the calendar quarter dummies included in Equation 1 control for calendar quarter effects such as those due to variation in the growth of the economy.

Figure 2a shows a broadly similar pattern for men across school types and degree programs: slightly declining earnings in the last four quarters before entry, a large decline around entry (particularly for men in for-profit certificate programs), followed by consistent gains in earnings.[20] Given the modest standard errors (Appendix Table A3), the earnings gains are positive and significant (at the one percent level) starting two years after enrollment, if not sooner. The earnings increment continues to rise for men enrolled in for-profit associate's degree programs, but earnings gains grow more slowly beginning around four years after entry for the other three groups. By the fifth year after entry,[21] the average earnings increment relative to predicted earnings in the absence of schooling (based on earnings five to 24 quarters prior to entry) for men seeking certificates in both for-profit schools and community colleges is about 20 percent.[22] In contrast, the average return for for-profit associate's degree students is 29 percent,

[18] Earnings coefficient estimates and bootstrap standard errors are shown in Appendix Tables A3a and A3b. To avoid clutter, figures exclude confidence intervals.

[19] The figure shows the combined effect of the coefficients for the dummy variables in $ENTRY$ and the coefficient for $ENROLL$. We use the $ENROLL$ dummy variable to capture the average number of quarters in which enrollment was observed (rounded to the nearest discrete value). For example, the average number of quarters of enrollment for men in certificate programs is about three, so the dummy variable for enrollment is set to one-half for quarters 0 and 2, and one in quarter 1, i.e., identifying the first three quarters in which individuals are enrolled.

[20] These analyses omit anyone who has no earnings in a given quarter. Thus, we might expect that the relatively small declines in earnings for associate's degree seekers at the time of enrollment might be associated with withdrawal from the labor force. In fact, this is not the case, as we see below that employment rates of associate's degree seekers decline very little during enrollment.

[21] Statements of the earnings increments for five years after entry are based on the mean for coefficient estimates for quarters 17-20 after entry. We choose that time period because it corresponds roughly with the average post-schooling time period in Cellini and Chaudhary (2014), thus facilitating comparisons of our results with theirs.

[22] Coefficients for earnings reported in the figures and tables refer to effects on the natural logarithm of earnings. We have converted these to percentages in our discussion for ease of interpretation.

compared to only 19 percent for community college men seeking associate's degrees, a difference that borders on statistical significance at the 5 percent level.[23] By the end of our period, men in associate's degree programs in for-profit colleges have the largest gain in earnings, well over 30 percent, followed by men in certificate programs in for-profit colleges, with gains of slightly over 25 percent. Men in community colleges continue to experience smaller growth in earnings increments than men in for-profit schools, despite having the highest earning growth in the first 6 quarters after enrollment.

Figure 2b shows a different pattern for women. After about the seventh quarter, women in associate's degree programs in community colleges experience the lowest earnings increments of the four groups; the increment is slightly higher in for-profit associate's degree programs. Both estimates are easily statistically significant, although the difference is not (see Appendix Table A3b). The earnings increments for women in the certificate programs are somewhat higher. In the fifth year after entry, the increases in earnings (again, relative to five to 24 quarters prior to entry) average 16 percent for women pursuing associate's degrees in community colleges, compared to 20 percent for for-profit schools, although the difference is not statistically significant. Returns average from 24 to 27 percent for women in certificate programs, and there is little difference between for-profit schools and community colleges. In quarters 21 to 25, earnings growth moderates for associate's degree programs.

Overall, Figures 2a and 2b show that individuals who attend for-profit schools fare as well as, if not better than, individuals who attend community colleges. The highest gains are for men pursuing associate's degrees in for-profit schools, and the lowest gains are for women pursuing associate's degrees in community colleges. Again, it bears repeating that the results

[23] Bootstrap standard errors for the differences of means for returns in quarters 17-20 for for-profit schools and community colleges are provided in the bottom lines of Appendix Tables A3a and A3b.

exclude students who transfer to four-year schools; hence these estimates ignore the value of community colleges in providing up to two years of a four-year degree.

*Effects on Employment*

Figures 3a and 3b provide estimates of the effects of enrollment on employment for males and females.[24] Recall that this analysis omits quarters with zero earnings that are of length 10 or greater up through quarter 30 (or the end of the earnings record if prior to that) following school entry, so individuals who permanently left the state do not contribute to this analysis after their departure.

Looking first at Figure 3a, men seeking certificates have an immediate decline in employment of 6-9 percentage points, compared to no decline for men enrolled in associate's degree programs. After that, the highest short-run employment gains (compared to employment at least one year prior to enrollment) are for men in community colleges pursuing certificates. In contrast, men in certificate programs at for-profit colleges have the lowest employment gains – less than one percentage point (in some cases negative) – throughout the period. For men seeking an associate's degree at a for-profit school, employment prospects improve steadily before peaking 13 quarters after entry at around 5 percentage points. Although the employment gains decline after this point, they are generally higher than for the other three programs in the last ten quarters of our analysis. However, if we compare differences in employment in year five for for-profit and community colleges, differences are not statistically significant.[25]

Turning to Figure 3b we see that all four groups of women experience a decline in

[24] Coefficient estimates and bootstrap standard errors are provided in Appendix Table A4a and A4b. Bootstrap standard errors of the differences between average returns for quarters 17-20 for for-profit schools and community colleges are also provided.

[25] For students who entered schooling in 2005-2006, five to six years later is 2010-2012, a period of unusually high unemployment. If those with new credentials faced particular problems obtaining employment during this period, employment would decline near the latter part of our data window.

employment rates (relative to predicted employment) during the period of enrollment, with the decline ranging from 2 to 12 percentage points. Women attending certificate programs in community colleges have no more than a 2-percentage-point improvement in employment and in later quarters have lower employment relative to the pre-enrollment period. The highest gains in employment are for women attending certificate programs in for-profit schools, where women experience gains of 4 percentage points for year five after enrollment. In contrast, women in community colleges experience no improvement; the difference between for-profit schools and community colleges is statistically significant. Women pursuing associate's degrees, either in for-profit schools or in community colleges, have employment gains of up to 3 percentage points for much of the post-enrollment period.

Comparing Figures 3a and 3b we see stark differences in the patterns of employment for men and women. Certificates in for-profit schools provide the best employment outcomes for women and the worst outcomes for men, whereas certificates in community colleges provide the smallest improvement for women and the highest short-run improvement for men. Associate's degree programs provide similar gains for both men and women three to five years after enrollment.

*Sensitivity Analyses for Earnings*

Figure 4 contains a concise overview of the results for alternative models and subsamples. The figure contains the average estimated increase in earnings in quarters 17 to 20 due to school enrollment. We combine results for gender and credential type, but we show separate results for for-profit schools and community colleges. Each bar is the weighted average of the returns, where the weight is proportional to the number of students of each gender and credential type. The first bars are for the main model, summarizing the results in Figures 2a and

2b. Overall, the returns are similar across the alternative models and samples. More detailed results and explication of the models and samples underlying Figure 4 are included in Appendix B.[26] For analyses by field of study, see Jepsen et al. (2023).

One of the assumptions of our model is that there are no prior trends in earnings. Given our large sample sizes, we find statistically significant prior trends for several groups. However, in substantive terms, the violations are small, and we conclude that they do not affect our results. Results are similar in a random effects model that accounts for prior trends in earnings, as suggested by the summary in Figure 4. We provide further detail in Appendix B.

As noted earlier, in the face of heterogeneity, estimates of the effect of an intervention based on the traditional one-step model may be seriously misleading. However, such a bias is only present when heterogeneity is not accounted for and where it is associated with fixed effects. Estimates based on the traditional one-step model, but allowing for heterogeneity over time in effect estimates, are very similar to those we obtain, although the earnings increment for community colleges is about 2 percentage points smaller in this model (Figure 4). The reason the bias is small is that our model explicitly captures variation over time since entry in the estimate of the increment to earnings.

Even with our two-step model, we find that returns are smaller when we estimate a single coefficient for all post-schooling periods (Figure 4), a common but inflexible model estimated in early studies such as Jepsen, Troske, and Coomes (2014). Because this approach averages early returns immediately after participation with later returns, we view this as a seriously misleading estimate of long-run return.

[26] In addition to analyses summarized in Figure 4, Appendix B reports results of models that alter the coding of age, and of models that estimate effects on earnings levels rather than log earnings. Appendix B also reports how estimates of effects on employment are altered by the deletion of zero earnings strings at the end of the observation period.

We also fit models that exclude the enrollment dummy, and those based on the sample omitting earnings outliers, students pursuing the academic field (particularly common for community college associate's degree seekers) or those enrolled in programs that are ineligible for Title IV federal funding. We also divide students by whether they were observed enrolled for no more than five or more than five semesters. In each case, differences in estimated returns are modest, and estimated returns are always higher for for-profit schools.

Figure 4 shows that returns are smaller for students who enroll in the later period, 2009-2012, for individuals with at least 10 quarters of earnings prior to entering school, and for individuals who were at least 30 years of age at enrollment. This pattern of results is consistent with two explanations: (1) the benefits of schooling are higher for younger students and those with limited labor-force attachment, or (2) our preferred estimates slightly overstate the return because the pre-schooling earnings for these individuals are not ideal measures of the earnings trajectory of individuals in the absence of further schooling. In any case, our qualitative results are not altered.

Finally, we consider our decision not to pool our analyses of for-profit schools and community colleges. Given the stark differences in student characteristics between the two school types documented above, one might assume that separate models should be estimated by school type within credential and gender to provide the most flexible model. However, in their analyses of students seeking certificates, Cellini and Turner (2019) pool data between for-profit and community college students. We estimate pooled models on the combined sample of for-profit and community college students, finding that pooling the data affects the results, particularly for certificates. Using the pooled data, the estimated earnings gains for for-profit students seeking certificates are much lower than the estimates for community college students, a

pattern found in Cellini and Turner (2019). In contrast, our preferred estimates with separate models show little if any gap in earnings gains between school types. We conclude that pooling the data for for-profit and community college students is not appropriate. Detailed analyses are provided in Appendix B.

*Results of Schooling Completion*

So far, all the results are for attendance regardless of completion. In Figures 5a and 5b, we limit the sample to completers to facilitate comparisons with previous work such as Jepsen, Troske, and Coomes (2014), who compare community college completers to dropouts.[27] However, because completion is not random, the results should not be interpreted as causal. Not surprisingly, earnings gains are higher for completers than for the full sample of attendees (Figures 2a and 2b), particularly for women pursuing certificates in community college. With the exception of men pursuing associate's degrees, completers in community college do as well as, and often better than, completers in for-profits. But community college students are between a fifth and a third as likely to complete certificates or degrees as are for-profit students. Although quite low, these community college completion rates are similar to those reported in other states such as Kentucky and Tennessee (Jepsen, Troske, and Coomes 2014; Carruthers and Sanford 2018).

*Effect Estimates Based on Matching*

The descriptive analysis makes clear that for-profit schools and community colleges are largely serving different markets. The analysis of this section attempts to identify the returns for comparable individuals in comparable fields. We use propensity score matching to form a

[27] For those seeking a certificate, completers are students who receive a certificate or associate's degree by the end of the enrollment spell. For those seeking an associate's degree, completers are students who receive an associate's degree.

comparison group of students in each school type by gender and credential sought. We consider two alternative sets of estimates. The first is based on students who attend for-profit schools and identifies comparable students in similar fields in community colleges; in this case, for-profit school is the "treatment," and community college is the comparison group. The second set of estimates starts with community college students and asks what the return is for comparable for-profit students in the same fields of study; here, community college is the treatment, and for-profit school is the comparison.[28] As shown in the dissimilarity index analysis (Table 2), the two school types have large differences in field of study and location. As above, we perform these analyses separately by gender and credential sought. Using a combined sample of students in for-profits and community colleges, we estimate the probability that an individual enrolls in a for-profit school, using a logit. Appendix C provides more details on the matching algorithm.

Estimates of returns for men seeking certificates appear in Appendix Figure C1. We find matches for only about half of the for-profit students when for-profit is taken as the treatment (Appendix Table C1). However, the estimated returns for this more limited sample "FP Return (FP Treatment)" are quite similar to those reported in Table 2a.[29] We observe that estimated returns for the small proportion of community college students matched with for-profit students, "CC Returns (FP Treatment)," are appreciably higher, implying that the small proportion of students who have similar characteristics and find comparable training in community colleges do very well. For the matched sample, returns for for-profit students average 21 percent for the fifth year, but are 34 percent for the matched community college sample. However, the returns are not

[28] Whether the for-profits or the community colleges are the treatment, the underlying samples from these schools are the same, as they are on the common support. However, where the community college is the treatment, the sample for the for-profit schools is weighted to reproduce the distribution of student characteristics and fields in community college; where for-profits are the treatment, the sample for community college students is weighted to reproduce the distribution of student characteristics and fields in for-profits. See Appendix C for more details.
[29] Note that the treated sample is unweighted, so the return only differs from that reported earlier because not all treated cases can be matched.

uniformly higher for community college students. When community college is taken as the treatment, returns for community college students in this sample are only 18 percent in the fifth year, but the for-profit returns for the sample that is matched to it are 27 percent.

When we consider men seeking associate's degrees (Appendix Figure C2), we find somewhat different results. When for-profit schools are taken as the treatment, community college returns for the comparison cases matched to the for-profit students are appreciably lower (fifth-year average of 18 percent compared to 25 percent). Similarly, when community college is taken as the treatment, the community college returns are also lower than the for-profit returns (fifth-year average of 17 percent versus 40 percent).

The differences in returns across conditions are smaller for women. However, for those seeking certificates (Appendix Figure C3) in the case where for-profit schools are the treatment, the basic patterns are similar to those of men, implying that community college students with characteristics and fields similar to for-profit students have higher returns than the for-profit students (fifth year average 30 percent versus 21 percent). There is essentially no difference in the return in the fifth year between the two types of schools when community college is taken as the treatment. Finally, for women seeking associate's degrees (Appendix Figure C4), we find that the returns for for-profit schools and community colleges are quite similar when for-profit is taken as the treatment, but when community college is taken as the treatment, returns for for-profit schools are much higher.

Overall, these analyses do not suggest that either for-profit schools or community colleges dominate in terms of expected return. The case of certificates is notable in that the ranking of returns depends on the choice of the treatment group. Specifically, for both men and women, the select set of community college students who are similar to for-profit students and

have similar fields of study have greater returns than for-profit students, but the returns for the selected set of for-profit students who match community college students are higher than or similar to the returns for community college students.

## VII. Direct Costs of Attendance and Rate of Return

Our analyses above focus on the labor-market returns to school attendance, calculated as an increment to earnings. We have not considered any direct costs. In the absence of measures of direct cost incurred, Jepsen et al. (2021) describe the process of estimating the costs to completing an award by school type. They show that the average cost for obtaining a certificate in a for-profit school, approximately $14,000, is twice as much as the cost at a community college. For an associate's degree, the average cost at a for-profit is over $25,000, more than three times the cost at a community college. Differences in field of study do not explain these cost differentials.

Given that the direct costs of attendance for students in for-profit schools and community colleges differ, it is natural to estimate returns net of those costs. For such calculations, we use average earnings for quarter since enrollment reported in Figures 1a and 1b, in conjunction with our estimates of the effect on earnings for these same groups from Figures 2a and 2b. We combine them with employment rates, along with estimates of enrollment effects on employment (Figures 3a and 3b), to provide the dollar difference in the expected earnings (including direct costs) for the average recipient in each quarter. Estimates therefore account for both the effects on earnings for those working and on the likelihood of employment, and they account for forgone earnings as well as direct costs while in school. Our estimates of the effects of enrollment on earnings cover only 25 quarters after entry, but returns presumably are expected to accrue for a more extended period. In the face of this uncertainty, we choose to extrapolate our

data to 100 quarters (25 years), simply taking the average dollar benefit received in quarters 21-25 and extending it through quarter 100. Direct costs are assumed paid over the quarters of enrollment. Although these assumptions are arbitrary, the basic pattern of results is not sensitive to the particulars.

Although internal rates of return do not provide a comprehensive measure of the value of an investment, such measures indicate at what interest rate the net return would be positive. Table 3 provides estimates of the internal rate of return. Return estimates vary quite dramatically, although all indicate a return on investment in excess of 5 percent. For men, the internal rate of return for those seeking certificates at for-profit schools is 13.6 percent, whereas it is 39 percent for men in community colleges. For men seeking associate's degrees, the for-profit rate of return is 5.4 percent, whereas the return for men attending community colleges is 20.5 percent.

The net rates of return for for-profit schools are lower primarily because the direct costs are so much higher. Rates of returns are generally higher for certificates than for associate's degrees, with the exception of women in for-profit schools. Although the return to a certificate may seem more favorable, it is also a smaller investment; comparison of internal rates of return may be misleading where the level of investment differs. Similarly, the effective investment in for-profit schools is also greater, so the lower rate of return is associated with a greater investment. These estimates ignore the possibility that students who do not finish their chosen field may face smaller direct costs; calculated rates of return would be higher if this were taken into account. However, the basic ranking of returns would not change because a substantially smaller proportion of community college students obtains certificates or completes degrees.

## VIII. Conclusion

This paper investigates the relationship between attendance in a for-profit school or a community college and subsequent earnings and employment. We use an individual fixed-effects method, estimated in two steps, to control for time-invariant differences between students. We find positive effects of attendance on earnings for students enrolled in both types of schools and degree programs. The lowest gain in earnings is for women pursuing associate's degrees in community colleges. One caveat to our results is that, like nearly all papers involving community colleges, we exclude students who plan to transfer to a four-year school in hopes of completing a bachelor's degree. Our results therefore omit one potential role that community colleges provide, and this may be of substantial importance. Of enrollment spells we observe in community colleges in our data, about 8 percent are omitted because the student subsequently attends a four-year institution; an additional third are omitted for students who are not seeking certificates or degrees, presumably for many because they intend to transfer to a four-year institution.

How do our results compare to others in this literature? Our estimates of the earnings increments associated with school attendance are generally consistent with previous studies, although there is quite a range of reported estimates. Our earnings estimates for individuals pursuing an associate's degree at a for-profit school are very similar to those of Cellini and Chaudhary (2014).[30] Our estimates of effects for students seeking an associate's degree at a community college are somewhat lower than those for completers in Stevens, Kurlaender, and Grosz (2019) and Jepsen, Troske, and Coomes (2014).[31] Among studies that compare degree-

[30] Cellini and Chaudhary (2014) report estimates of an earnings increment for an associate's degree of about 10 percent for the four years after leaving school. Our estimates for the same period are between 11 and 13 percent.
[31] Stevens, Kurlaender, and Grosz (2019) find average earnings returns in excess of 40 percent for completing a vocational associate's degree. Jepsen, Troske, and Coomes (2014) also report returns from degree completion rather than attendance, finding an earnings increment of 56 percent for women and 24 percent for men in the period 4.5-6 years after entry, whereas our figures (for attendance) for a comparable period are 17-21 percent for women and 22-36 percent for men.

seeking students in for-profit schools to public colleges but do not provide separate estimates, Deming, Goldin and Katz (2012) report lower earnings for degree-seeking students in for-profit schools, whereas Lang and Weinstein (2013) find no statistically significant difference.

When comparing our results for students in community colleges seeking certificates with previous work on students completing certificates, we find estimates that are somewhat greater than those reported in Jepsen, Troske and Coomes (2014) but smaller than those in Stevens, Kurlaender, and Grosz (2019). Lang and Weinstein (2013) do not obtain separate estimates of earnings returns for students seeking certificates; their comparison of for-profit and public schools does not find statistically significant differences in returns.

Cellini and Turner (2019) report results for students seeking certificates that are discrepant with ours. Although they do not estimate separate returns for for-profit and public schools, they estimate earnings differentials, finding that employed students seeking certificates from for-profit schools earn, on average, 11 percent less than a matched sample seeking certificates from community colleges. There are a variety of differences between our sample and methods and theirs, but our attempts to replicate their methods show that our results are not sensitive to most differences. In particular, we do not find that use of a broader sample, which included those not obtaining federal aid, has important effects on overall results. Two factors appear to be of primary importance in explaining the difference. First, Cellini and Turner (2019) present only comparisons where community college students are matched to represent the fields and characteristics of for-profit students (for-profit schools are the "treatment"). Our results from such a matching also find higher returns for students in public schools. However, we show that the public-school advantage does not survive if for-profit students are matched to represent the characteristics of public-school students. Equally important, when we follow Cellini and Turner

(2019) in pooling for-profit and public-school students, the relative returns to for-profit students decline, indicating that their decision to pool also plays a role in producing their results.

Our results go beyond those reported by others in several important ways. We show that the earnings increment associated with school attendance grows substantially over the several years following school leaving. Estimates that focus on a shorter period, while consistent with those reported here, are seriously misleading. Similarly, even when studies include returns over a several-year period (Cellini and Chaudhary 2014), if they estimate an average for the period that begins with school leaving, this will underestimate lifetime returns, because the higher relative earnings that would be observed near the end of the estimation period are likely to better reflect returns that would accrue over most of the work life of the student.

Our estimates of direct costs confirm the general view that for-profit schools are much more expensive than community colleges, whether students are seeking certificates or associate's degrees. As a result, the effective rate of return for students attending for-profit schools is lower. However, our estimates suggest that, over the long run, the average for-profit student receives an earnings increment that ultimately covers those direct costs.

Overall, our results suggest that attending for-profit schools and community colleges gain valuable labor-market skills. We have also shown that the two types of schools serve different types of students, offer different degrees and fields of study, and are located in different places. This observation suggests that direct comparison of the costs and returns for for-profit schools and community colleges may be misleading. If a student who wishes to pursue a given area of study can attend only a for-profit school, the returns that could be obtained at a community

college are not relevant.[32] The more appropriate question is whether those returns are sufficient to justify attendance.

Our results do not allow us to reject the reality that some students complete expensive or time-consuming programs and obtain minimal labor-market returns. However, these results suggest that those who enroll in for-profit schools and community colleges experience substantial earnings benefits that, in aggregate, exceed forgone earnings and even substantial direct tuition and other direct costs. For this reason, there is little basis for restructuring policy in favor of either the for-profit schooling sector or public community colleges.

[32] In the face of cutbacks in funding for public postsecondary education, the availability of public alternatives is not likely to improve.

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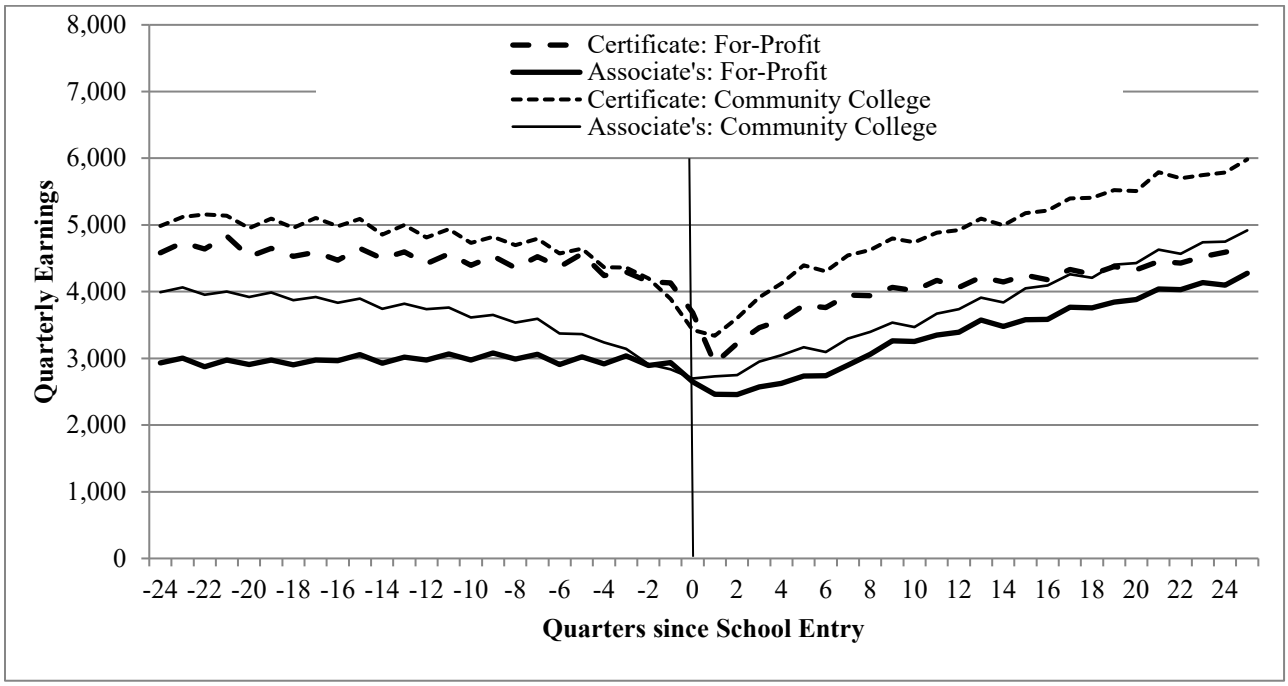
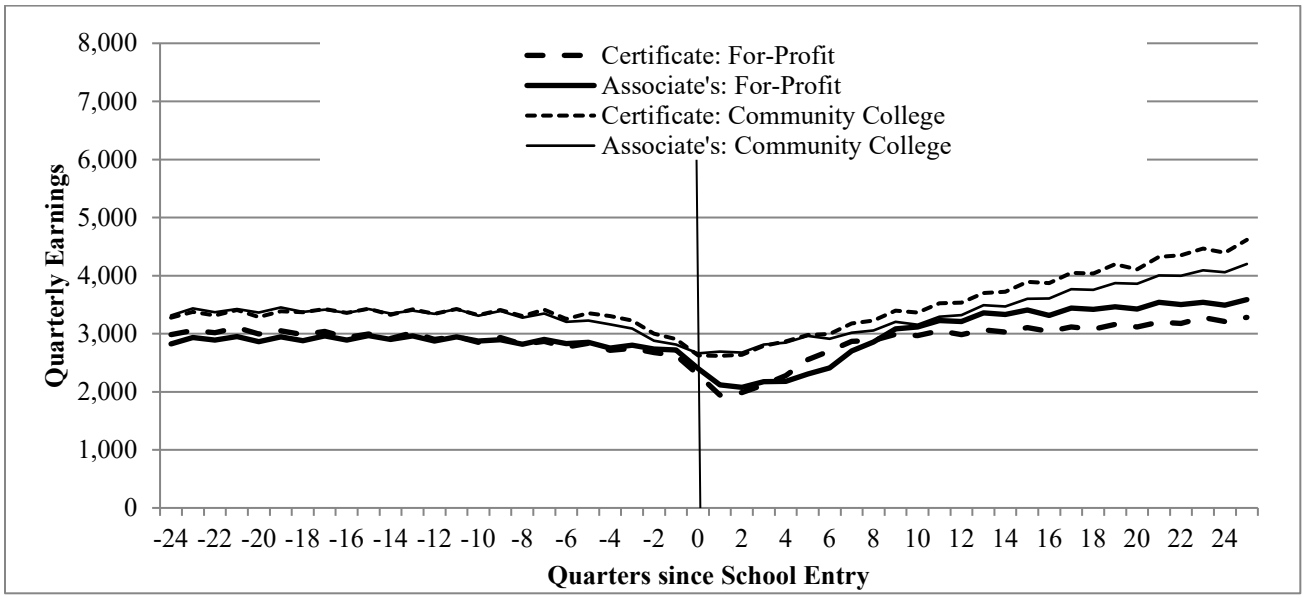
**Figure 1a – Quarterly Earnings by School, Program Type and Quarters since School Entry, Men**



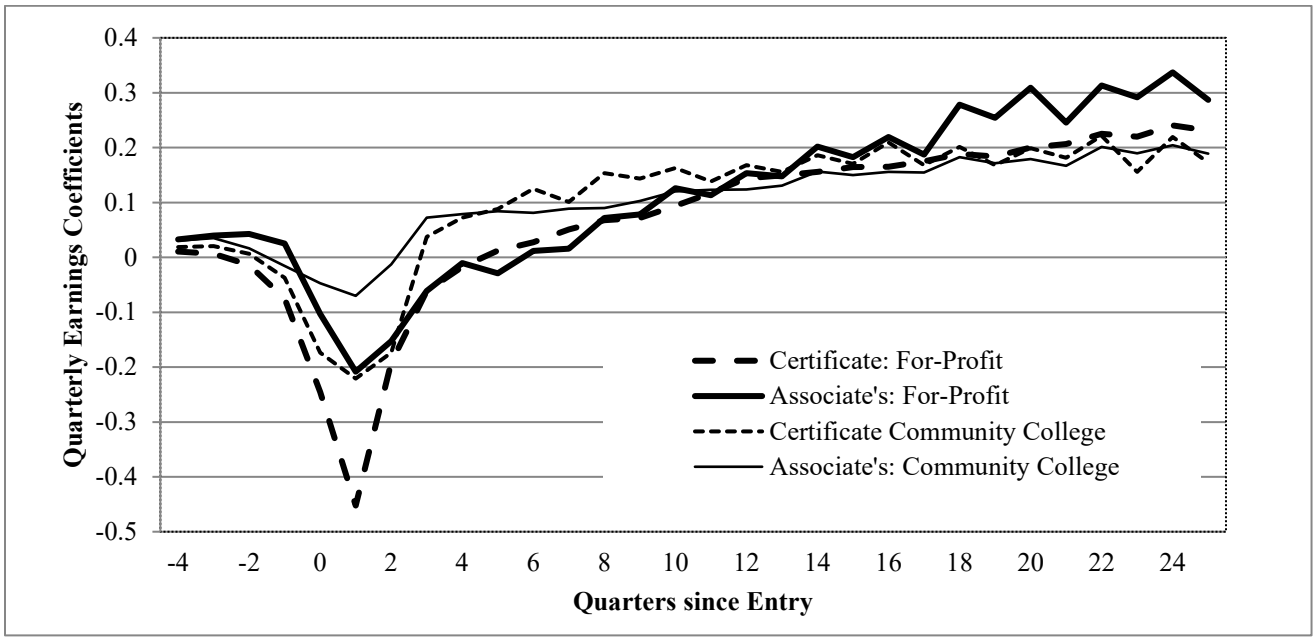

Note: The figure shows the average quarterly earnings for men pursuing associate's degrees and men pursuing certificates. Earnings are not conditional on employment, except for the exclusion of strings of quarters of zero earnings of length 10 or more through quarter 30 following initial enrollment. Earnings are measured in first quarter 2010 dollars.

**Figure 1b – Quarterly Earnings by School, Program Type and Quarters since School Entry, Women**



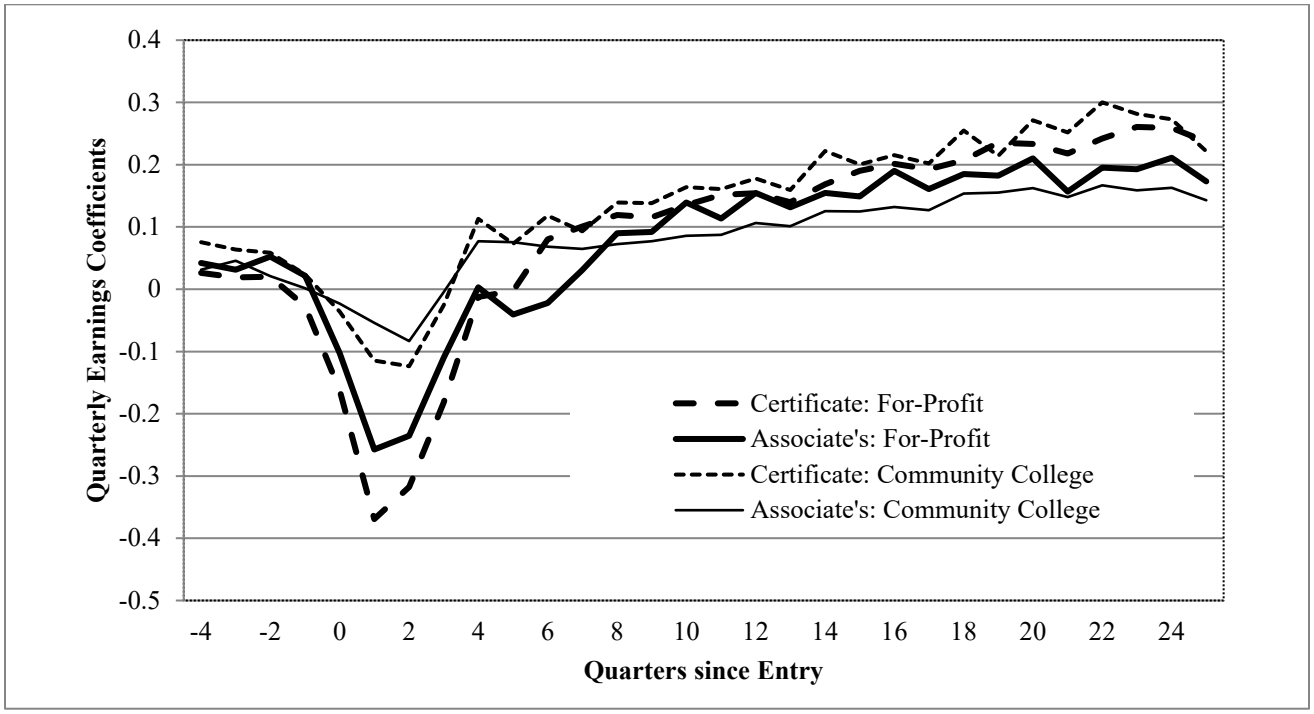

Note: The figure shows the average quarterly earnings for women pursuing associate's degrees and women pursuing certificates. Earnings are not conditional on employment, except for the exclusion of strings of quarters of zero earnings of length 10 or more through quarter 30 following initial enrollment. Earnings are measured in first quarter 2010 dollars.

**Figure 2a – Effect of Attendance on Earnings by Quarter and School and Program Type, Men**



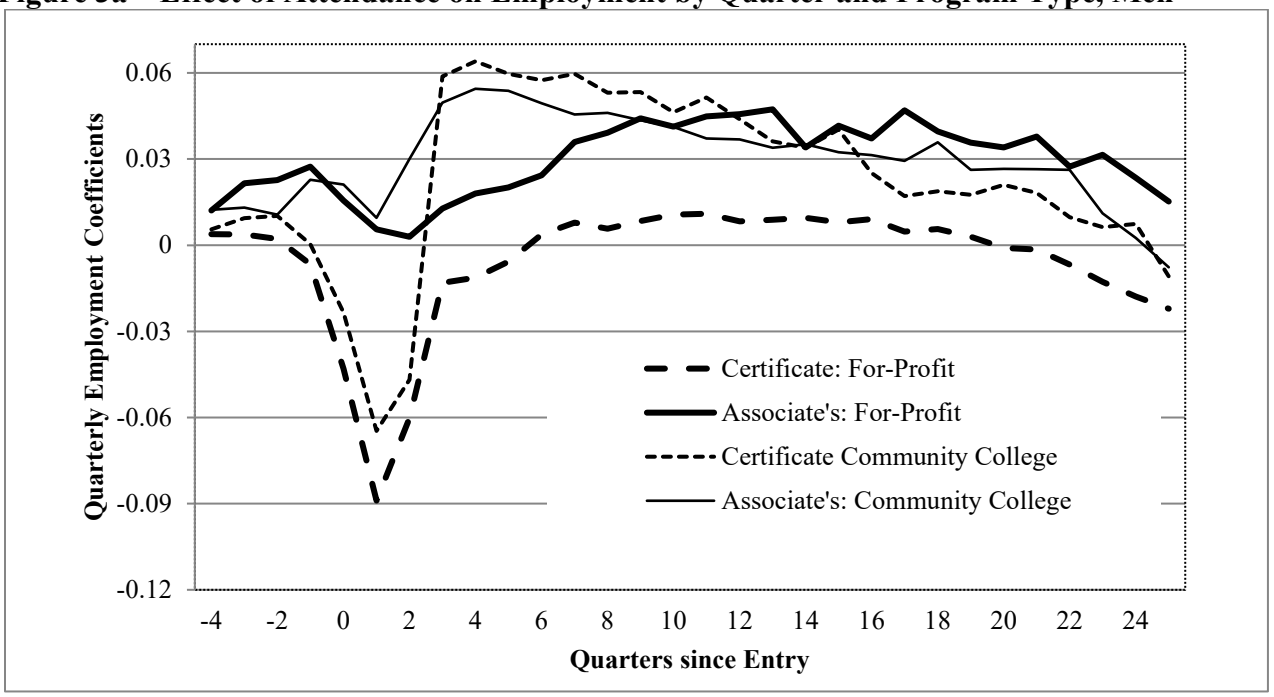

Note: Each data point is the effect of attendance on the natural log of quarterly earnings based on estimates presented in Appendix Table A3a. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for the group. See text. Standard errors (shown in Appendix Table A3a) vary from 0.004 to 0.025 in the first 10 quarters after enrollment, and 0.01 to 0.05 in later periods.

**Figure 2b – Effect of Attendance on Earnings by Quarter and School and Program Type, Women**



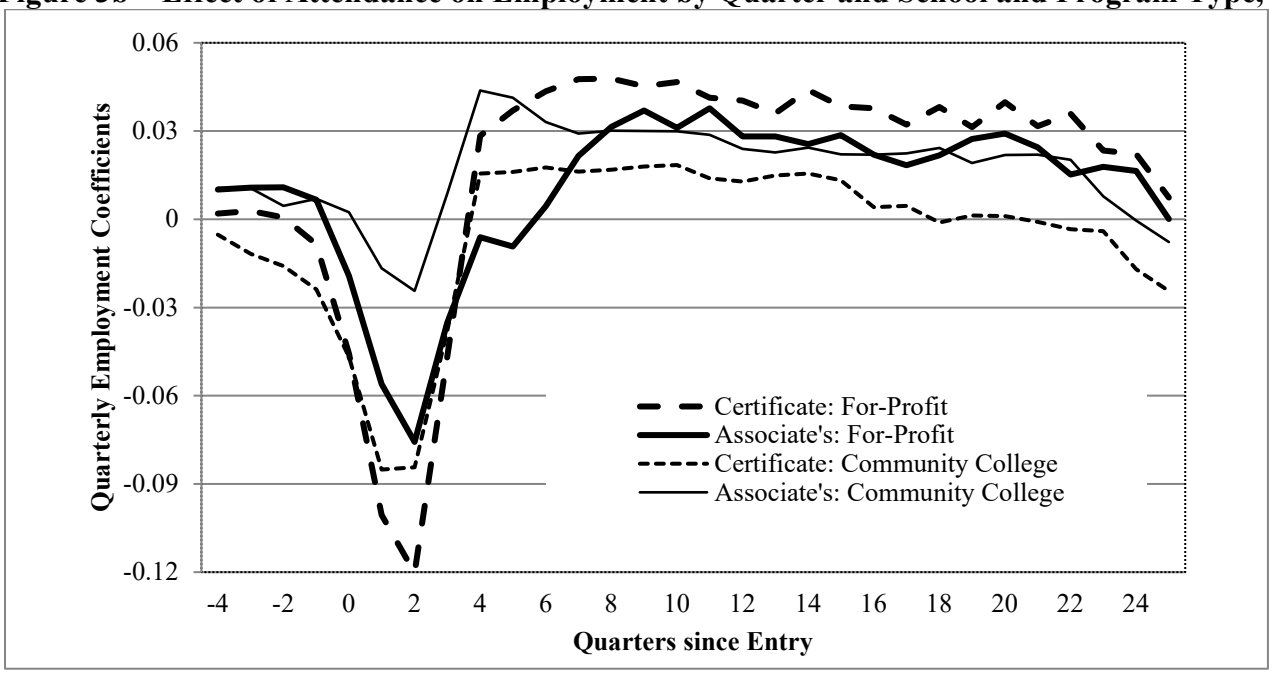

Note: Each data point is the effect of the attendance on the natural log of quarterly earnings based on estimates presented in Appendix Table A3b. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group. See text. Standard errors (shown in Appendix Table A3b) vary from 0.003 to 0.02 in the first 10 quarters after enrollment, and 0.005 to 0.04 in later periods.

**Figure 3a – Effect of Attendance on Employment by Quarter and Program Type, Men**



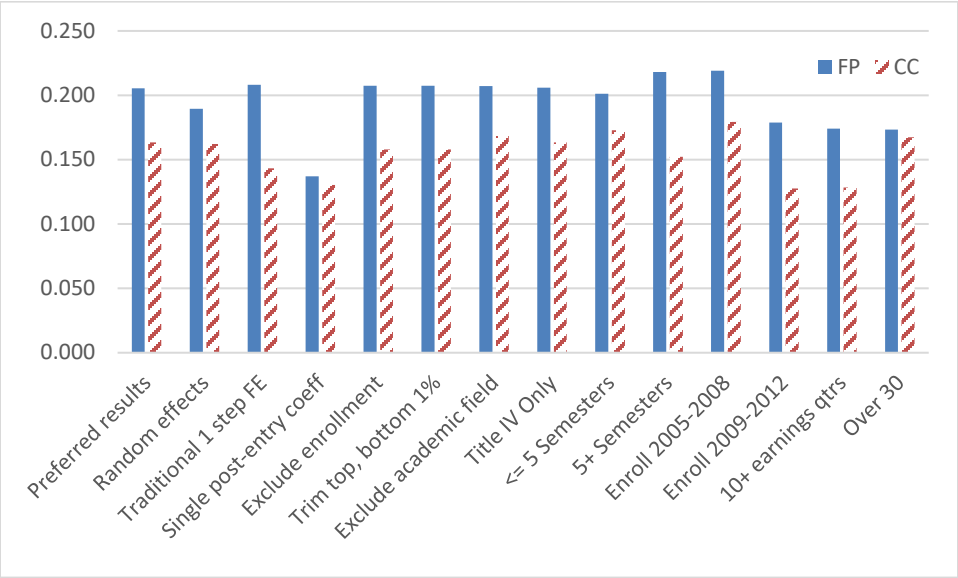

Note: Each data point is the effect on quarterly employment based on estimates presented in Appendix Table A4a. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for the group (see text). Standard errors are reported in Appendix Table A4a.

**Figure 3b – Effect of Attendance on Employment by Quarter and School and Program Type, Women**



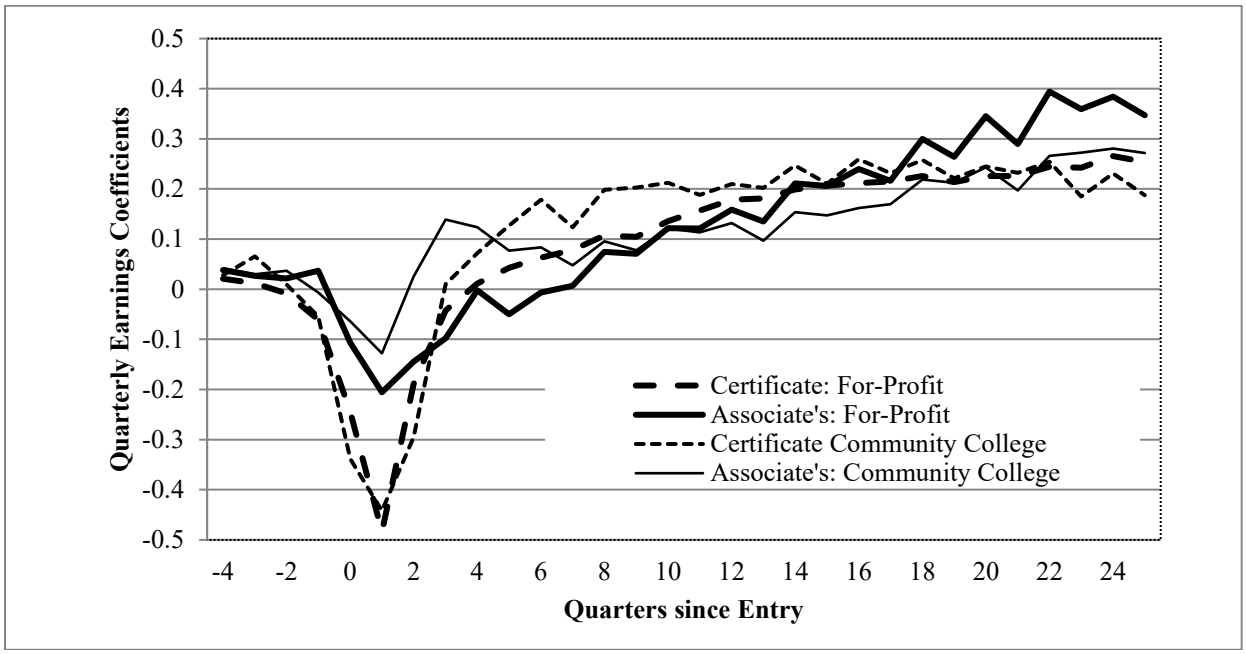

Note: Each data point is the effect on quarterly employment based on estimates presented in Appendix Table A4b. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for the group (see text). Standard errors are reported in Appendix Table A4b.

**Figure 4 – Overview of Results: Estimates of Increase in Earnings, Quarters 17-20.**



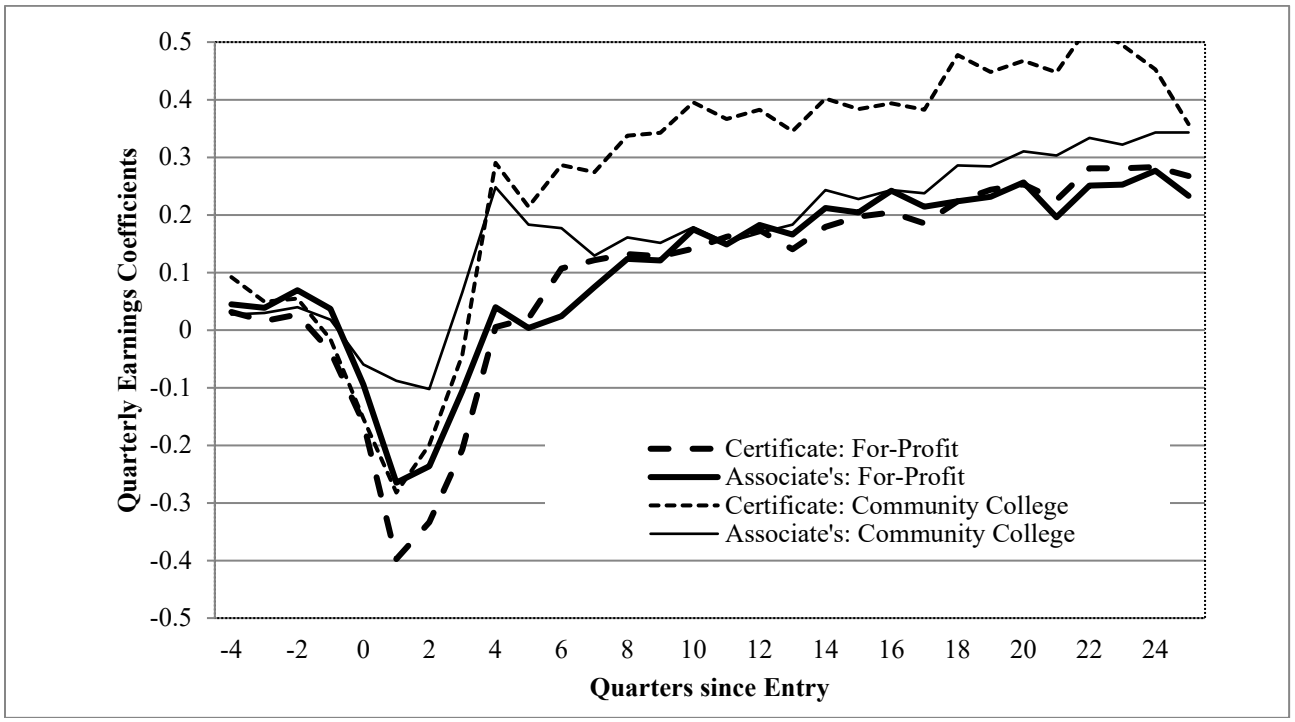

Note: Reported returns are constructed as the weighted average of the average return for quarters 17-20 for each of the four gender-credential groups, weighted by the number of cases in each group. Details of model specification and disaggregated estimates are presented in Appendix B and Appendix Figures B1 – B13.

**Figure 5a – Effect of Completion on Earnings by Quarter, School and Program Type, Men**




Note: Each data point is the effect estimate for the natural log of quarterly earnings based on Equation 3, estimated for the sample of students who complete the credential they pursued.

**Figure 5b – Effect of Completion on Earnings by Quarter, School and Program Type, Women**




Note: Each data point is the effect estimate for the natural log of quarterly earnings based on Equation 3, estimated for the sample of students who complete the credential they pursued.

**Table 1 - Descriptive Statistics by Gender and Program Type**

| | For-Profit | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Men | | Women | | Men | | Women | |
| | Certificate | Associate's | Certificate | Associate's | Certificate | Associate's | Certificate | Associate's |
| Variable | Mean | Mean | Mean | Mean | Mean | Mean | Mean | Mean |
| *Demographics and Educational Background* | | | | | | | | |
| White | 0.642 | 0.601 | 0.544 | 0.626 | 0.764 | 0.697 | 0.768 | 0.673 |
| Black | 0.279 | 0.263 | 0.374 | 0.281 | 0.090 | 0.141 | 0.100 | 0.179 |
| Other/missing race | 0.079 | 0.136 | 0.082 | 0.093 | 0.146 | 0.163 | 0.132 | 0.148 |
| Age at time of entry | 32.7 | 27.6 | 29.5 | 28.1 | 28.5 | 24.8 | 28.9 | 26.8 |
| | (10.7) | (8.3) | (10.0) | (8.8) | (10.5) | (8.4) | (10.5) | (9.6) |
| Less than high school | 0.063 | 0.013 | 0.067 | 0.021 | 0.002 | 0.003 | 0.003 | 0.004 |
| High school | 0.718 | 0.775 | 0.766 | 0.806 | 0.727 | 0.799 | 0.802 | 0.812 |
| GED | 0.199 | 0.205 | 0.154 | 0.164 | 0.043 | 0.050 | 0.051 | 0.056 |
| Missing education | 0.020 | 0.008 | 0.014 | 0.010 | 0.229 | 0.148 | 0.145 | 0.128 |
| Major urban | 0.496 | 0.802 | 0.667 | 0.735 | 0.522 | 0.626 | 0.406 | 0.651 |
| Small metro | 0.141 | 0.127 | 0.163 | 0.161 | 0.282 | 0.187 | 0.309 | 0.147 |
| Nonmetro | 0.357 | 0.071 | 0.170 | 0.104 | 0.196 | 0.187 | 0.285 | 0.202 |
| Missing metro | 0.006 | 0.000 | 0.001 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| *Schooling Information* | | | | | | | | |
| Studying business | 0.039 | 0.099 | 0.069 | 0.119 | 0.065 | 0.038 | 0.086 | 0.057 |
| Studying computers | 0.044 | 0.283 | 0.015 | 0.047 | 0.063 | 0.050 | 0.021 | 0.013 |
| Studying health | 0.154 | 0.180 | 0.763 | 0.635 | 0.159 | 0.024 | 0.438 | 0.070 |
| Studying trades | 0.310 | 0.033 | 0.012 | 0.002 | 0.151 | 0.057 | 0.005 | 0.003 |
| Studying transport | 0.294 | 0.000 | 0.023 | 0.000 | 0.063 | 0.000 | 0.004 | 0.000 |
| Studying vocational | 0.143 | 0.356 | 0.103 | 0.147 | 0.380 | 0.134 | 0.226 | 0.072 |
| Studying academic/other | 0.016 | 0.049 | 0.014 | 0.051 | 0.120 | 0.697 | 0.220 | 0.786 |
| Completed certificate | 0.630 | 0.012 | 0.525 | 0.024 | 0.196 | 0.011 | 0.097 | 0.010 |
| Completed associate's | 0.007 | 0.416 | 0.013 | 0.470 | 0.041 | 0.099 | 0.089 | 0.123 |
| No certificate or degree | 0.363 | 0.572 | 0.462 | 0.506 | 0.764 | 0.891 | 0.814 | 0.867 |
| Number of entries | 32,117 | 12,979 | 39,830 | 21,115 | 9,789 | 113,259 | 14,371 | 153,533 |

Note: The standard deviation for age is in parentheses.

**Table 2 - Index of Dissimilarity between For-Profit and Community College Students**

| | Men | | Women | |
|---|---|---|---|---|
| | Certificates | Associate's Degrees | Certificates | Associate's Degrees |
| Field of study (7 areas) | 0.390 | 0.673 | 0.352 | 0.736 |
| Race | 0.190 | 0.122 | 0.274 | 0.102 |
| Age (4 categories) | 0.197 | 0.197 | 0.038 | 0.099 |
| Region within the state | 0.467 | 0.233 | 0.453 | 0.202 |
| Propensity score (deciles) | 0.762 | 0.797 | 0.703 | 0.863 |

Note: The index of dissimilarity is defined as $(\frac{1}{2}) \sum_{i=1}^{n} |FP_i - CC_i|$, where $FP_i$ and $CC_i$ are the proportions of for-profit students and of community college students, respectively, in category $i$, and $n$ is the number of categories. We also calculated the Gini coefficient, which many have argued is a preferable measure of dissimilarity, which also varies between 0 and 1. Although numerical values were always higher (by as much as 0.15), the rankings were essentially the same. See Duncan and Duncan (1955).

**Table 3 - Internal Rate of Return**

| | For-Profit | | Community Colleges | |
|---|---|---|---|---|
| | Certificates | Associate's | Certificates | Associate's |
| Men | 13.6% | 5.4% | 39.2% | 20.5% |
| Women | 12.2% | 17.6% | 29.1% | 24.9% |

Note: Internal rate of return is calculated as the interest rate that equalizes the present value of earnings and direct program costs for profiles with and without enrollment, based on 25 years following entry. See text.

# For Online Publication

## Appendix A: Supplemental Tables and Figures

**Table A1 – Reasons for Omitting Observations from Regression Sample**

| | For-Profit | | | Community College | | |
|---|---|---|---|---|---|---|
| | Sample Size | Number Change | Percent Change | Sample Size | Number Change | Percent Change |
| Number of individuals in sample | 170,190 | | | 496,716 | | |
| Number of spells | 184,481 | 14,291 | 8.4% | 565,119 | 68,403 | 13.8% |
| Omit spells for students without valid Social Security number | 176,680 | -7,801 | -4.2% | 507,957 | -57,162 | -10.1% |
| Omit spells for students who enroll in 4-year public university | 174,685 | -1,995 | -1.1% | 465,674 | -42,283 | -8.3% |
| Omit spells for students over 60 or under 18 | 161,701 | -12,984 | -7.4% | 454,516 | -11,158 | -2.4% |
| Omit spells for students with missing or invalid gender code | 157,529 | -4,172 | -2.6% | 454,424 | -92 | 0.0% |
| Omit spells for students not seeking certificate or degree | 137,668 | -19,861 | -12.6% | 308,648 | -145,776 | -32.1% |
| Omit spells for students not resident in Missouri or Kansas[a] | 115,187 | -22,481 | -16.3% | 301,850 | -6,798 | -2.2% |
| Omit spells for students in both for-profit and community college | 106,041 | -9,146 | -7.9% | 290,952 | -10,898 | -3.6% |

NOTES: The exact change in sample size depends on the order in which observations are omitted, but in the case at hand the percent change is largely insensitive to the order.

[a] Students with unknown residence are retained.

**Table A2 – Field of Study and Classification of Instructional Program**

| | | | Students Seeking Certificates | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Men | | | | Women | | | |
| Field | CIP | | For-Profit | | Community College | | For-Profit | | Community College | |
| | | | N | Percent | N | Percent | N | Percent | N | Percent |
| Business | 52 | Business | 1,094 | 3.4% | 628 | 6.4% | 2,673 | 6.7% | 1,230 | 8.6% |
| | 09 | Journalism | 172 | 0.5% | 4 | 0.0% | 81 | 0.2% | 2 | 0.0% |
| Computers | 10 | Communications | 35 | 0.1% | 73 | 0.7% | 37 | 0.1% | 60 | 0.4% |
| | 11 | Computer Sciences | 1,367 | 4.3% | 542 | 5.5% | 550 | 1.4% | 247 | 1.7% |
| Health | 34 | Health-Related Skills | 55 | 0.2% | 0 | 0.0% | 36 | 0.1% | 0 | 0.0% |
| | 51 | Health Professions | 4,895 | 15.2% | 1,555 | 15.9% | 30,368 | 76.2% | 6,289 | 43.8% |
| Trades | 46 | Construction Trades | 1,615 | 5.0% | 227 | 2.3% | 82 | 0.2% | 22 | 0.2% |
| | 47 | Mechanic | 6,558 | 20.4% | 793 | 8.1% | 343 | 0.9% | 34 | 0.2% |
| | 48 | Precision Production | 1,795 | 5.6% | 460 | 4.7% | 57 | 0.1% | 20 | 0.1% |
| Transport | 49 | Transportation | 9,432 | 29.4% | 612 | 6.3% | 923 | 2.3% | 57 | 0.4% |
| Vocational | 13 | Education | 257 | 0.8% | 879 | 9.0% | 441 | 1.1% | 2,288 | 15.9% |
| | 14 | Engineering | 0 | 0.0% | 44 | 0.4% | 0 | 0.0% | 5 | 0.0% |
| | 15 | Engineering Tech | 1,872 | 5.8% | 1,534 | 15.7% | 274 | 0.7% | 155 | 1.1% |
| | 22 | Legal Studies | 23 | 0.1% | 51 | 0.5% | 155 | 0.4% | 291 | 2.0% |
| | 36 | Leisure Studies | 129 | 0.4% | 0 | 0.0% | 30 | 0.1% | 0 | 0.0% |
| | 03 | Natural Resources | 0 | 0.0% | 0 | 0.0% | 1 | 0.0% | 0 | 0.0% |
| | 31 | Parks and Recreation | 118 | 0.4% | 0 | 0.0% | 48 | 0.1% | 0 | 0.0% |
| | 12 | Services | 2,185 | 6.8% | 106 | 1.1% | 3,162 | 7.9% | 217 | 1.5% |
| | 44 | Public Admin | 0 | 0.0% | 28 | 0.3% | 0 | 0.0% | 51 | 0.4% |
| | 41 | Science Tech | 0 | 0.0% | 17 | 0.2% | 0 | 0.0% | 26 | 0.2% |
| | 43 | Security | 5 | 0.0% | 1,064 | 10.9% | 7 | 0.0% | 214 | 1.5% |
| | 04 | Architecture | 0 | 0.0% | 2 | 0.0% | 0 | 0.0% | 1 | 0.0% |
| Academic | 01 | Agriculture | 244 | 0.8% | 111 | 1.1% | 205 | 0.5% | 118 | 0.8% |
| or Other | 26 | Biological Sciences | 9 | 0.0% | 11 | 0.1% | 4 | 0.0% | 32 | 0.2% |
| | 19 | Family Sciences | 2 | 0.0% | 17 | 0.2% | 36 | 0.1% | 534 | 3.7% |
| | 16 | Foreign Languages | 0 | 0.0% | 15 | 0.2% | 0 | 0.0% | 80 | 0.6% |
| | 24 | Liberal Arts | 9 | 0.0% | 714 | 7.3% | 21 | 0.1% | 1,931 | 13.4% |
| | 50 | Performing Arts | 104 | 0.3% | 173 | 1.8% | 161 | 0.4% | 231 | 1.6% |
| | | Other Acad. Fields | 142 | 0.4% | 129 | 1.3% | 135 | 0.3% | 236 | 1.6% |
| Total | | | 32,117 | 100.0% | 9,789 | 100.0% | 39,830 | 100.0% | 14,371 | 100.0% |

| | | | Students Seeking Associate's Degrees | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Men | | | | Women | | | |
| Field | CIP | | For-Profit | | Community College | | For-Profit | | Community College | |
| | | | N | Percent | N | Percent | N | Percent | N | Percent |
| Business | 52 | Business | 1,285 | 9.9% | 4,228 | 3.7% | 2,506 | 11.9% | 8,706 | 5.7% |
| | 09 | Journalism | 0 | 0.0% | 98 | 0.1% | 0 | 0.0% | 90 | 0.1% |
| Computers | 10 | Communications | 133 | 1.0% | 828 | 0.7% | 14 | 0.1% | 592 | 0.4% |
| | 11 | Computer Sciences | 3,542 | 27.3% | 4,826 | 4.3% | 971 | 4.6% | 1,367 | 0.9% |
| Health | 34 | Health-Related Skills | 30 | 0.2% | 0 | 0.0% | 11 | 0.1% | 0 | 0.0% |
| | 51 | Health Professions | 2,310 | 17.8% | 2,715 | 2.4% | 13,402 | 63.5% | 10,670 | 6.9% |
| Trades | 46 | Construction Trades | 101 | 0.8% | 768 | 0.7% | 9 | 0.0% | 35 | 0.0% |
| | 47 | Mechanic | 293 | 2.3% | 4,577 | 4.0% | 21 | 0.1% | 294 | 0.2% |
| | 48 | Precision Production | 36 | 0.3% | 1,114 | 1.0% | 3 | 0.0% | 81 | 0.1% |
| Transport | 49 | Transportation | 0 | 0.0% | 6 | 0.0% | 0 | 0.0% | 0 | 0.0% |
| Vocational | 13 | Education | 0 | 0.0% | 1,372 | 1.2% | 1 | 0.0% | 4,280 | 2.8% |
| | 14 | Engineering | 40 | 0.3% | 2,433 | 2.1% | 7 | 0.0% | 325 | 0.2% |
| | 15 | Engineering Tech | 3,088 | 23.8% | 4,964 | 4.4% | 352 | 1.7% | 631 | 0.4% |
| | 22 | Legal Studies | 176 | 1.4% | 141 | 0.1% | 986 | 4.7% | 1,033 | 0.7% |
| | 36 | Leisure Studies | 0 | 0.0% | 0 | 0.0% | 0 | 0.0% | 0 | 0.0% |
| | 03 | Natural Resources | 0 | 0.0% | 61 | 0.1% | 0 | 0.0% | 8 | 0.0% |
| | 31 | Parks and Recreation | 178 | 1.4% | 0 | 0.0% | 69 | 0.3% | 0 | 0.0% |
| | 12 | Services | 328 | 2.5% | 1,251 | 1.1% | 457 | 2.2% | 1,296 | 0.8% |
| | 44 | Public Admin | 0 | 0.0% | 239 | 0.2% | 0 | 0.0% | 1,186 | 0.8% |
| | 41 | Science Tech | 0 | 0.0% | 125 | 0.1% | 0 | 0.0% | 127 | 0.1% |
| | 43 | Security | 808 | 6.2% | 4,555 | 4.0% | 1,226 | 5.8% | 2,196 | 1.4% |
| | 04 | Architecture | 0 | 0.0% | 168 | 0.1% | 0 | 0.0% | 92 | 0.1% |
| Academic | 01 | Agriculture | 0 | 0.0% | 853 | 0.8% | 0 | 0.0% | 435 | 0.3% |
| or Other | 26 | Biological Sciences | 3 | 0.0% | 286 | 0.3% | 0 | 0.0% | 401 | 0.3% |
| | 19 | Family Sciences | 0 | 0.0% | 229 | 0.2% | 0 | 0.0% | 5,556 | 3.6% |
| | 16 | Foreign Languages | 0 | 0.0% | 38 | 0.0% | 0 | 0.0% | 99 | 0.1% |
| | 24 | Liberal Arts | 137 | 1.1% | 75,065 | 66.3% | 804 | 3.8% | 111,089 | 72.4% |
| | 50 | Performing Arts | 490 | 3.8% | 1,236 | 1.1% | 269 | 1.3% | 1,385 | 0.9% |
| | | Other Acad. Fields | 1 | 0.0% | 1,083 | 1.0% | 7 | 0.0% | 1,559 | 1.0% |
| Total | | | 12,979 | 100.0% | 113,259 | 100.0% | 21,115 | 100.0% | 153,533 | 100.0% |

Notes: Other Academic Fields includes CIP codes: 5 (Ethnic and Gender Studies), 23 (English), 27 (Mathematics), 30 (Interdisciplinary Studies), 32 (Basic Skills), 37 (Personal Awareness), 38 (Philosophy), 39 (Theology), 40 (Physical Sciences), 42 (Psychology), 45 (Social Studies), 53 (High School Diplomas and Certificates), and 54 (History).

**Table A3a – Effect of School Attendance on Log Quarterly Earnings: Men**

| | For-profit | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Certificate | | Associate's | | Certificates | | Associate's | |
| | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. |
| 4 quarters prior to entry | 0.011 | (0.007) | 0.032 | (0.011) | 0.019 | (0.013) | 0.031 | (0.004) |
| 3 quarters prior to entry | 0.006 | (0.007) | 0.040 | (0.012) | 0.020 | (0.013) | 0.035 | (0.004) |
| 2 quarters prior to entry | -0.014 | (0.007) | 0.043 | (0.013) | 0.007 | (0.014) | 0.016 | (0.004) |
| 1 quarter prior to entry | -0.075 | (0.008) | 0.025 | (0.014) | -0.037 | (0.015) | -0.016 | (0.004) |
| Quarter of entry | -0.178 | (0.012) | -0.051 | (0.018) | -0.082 | (0.021) | 0.026 | (0.005) |
| 1 quarter after entry | -0.318 | (0.015) | -0.104 | (0.024) | -0.038 | (0.025) | 0.076 | (0.006) |
| 2 quarters after entry | -0.125 | (0.013) | -0.048 | (0.023) | 0.010 | (0.021) | 0.060 | (0.005) |
| 3 quarters after entry | -0.061 | (0.012) | -0.009 | (0.021) | 0.037 | (0.019) | 0.073 | (0.005) |
| 4 quarters after entry | -0.018 | (0.011) | -0.010 | (0.021) | 0.072 | (0.018) | 0.079 | (0.005) |
| 5 quarters after entry | 0.012 | (0.011) | -0.029 | (0.020) | 0.088 | (0.018) | 0.084 | (0.005) |
| 6 quarters after entry | 0.028 | (0.012) | 0.012 | (0.024) | 0.125 | (0.019) | 0.081 | (0.005) |
| 7 quarters after entry | 0.051 | (0.012) | 0.016 | (0.024) | 0.101 | (0.019) | 0.089 | (0.006) |
| 8 quarters after entry | 0.067 | (0.012) | 0.072 | (0.023) | 0.153 | (0.020) | 0.090 | (0.006) |
| 9 quarters after entry | 0.072 | (0.012) | 0.078 | (0.023) | 0.144 | (0.019) | 0.103 | (0.006) |
| 10 quarters after entry | 0.094 | (0.013) | 0.126 | (0.025) | 0.163 | (0.020) | 0.120 | (0.006) |
| 11 quarters after entry | 0.117 | (0.013) | 0.113 | (0.025) | 0.138 | (0.022) | 0.123 | (0.006) |
| 12 quarters after entry | 0.144 | (0.014) | 0.154 | (0.029) | 0.168 | (0.023) | 0.123 | (0.007) |
| 13 quarters after entry | 0.149 | (0.014) | 0.148 | (0.029) | 0.156 | (0.023) | 0.131 | (0.007) |
| 14 quarters after entry | 0.156 | (0.015) | 0.202 | (0.038) | 0.186 | (0.024) | 0.156 | (0.007) |
| 15 quarters after entry | 0.165 | (0.016) | 0.182 | (0.036) | 0.171 | (0.025) | 0.150 | (0.007) |
| 16 quarters after entry | 0.165 | (0.016) | 0.220 | (0.040) | 0.209 | (0.025) | 0.156 | (0.008) |
| 17 quarters after entry | 0.175 | (0.017) | 0.187 | (0.039) | 0.168 | (0.027) | 0.155 | (0.008) |
| 18 quarters after entry | 0.189 | (0.018) | 0.278 | (0.047) | 0.201 | (0.029) | 0.183 | (0.008) |
| 19 quarters after entry | 0.184 | (0.019) | 0.255 | (0.045) | 0.168 | (0.030) | 0.172 | (0.008) |
| 20 quarters after entry | 0.200 | (0.019) | 0.309 | (0.045) | 0.199 | (0.031) | 0.179 | (0.009) |
| 21 quarters after entry | 0.206 | (0.020) | 0.245 | (0.042) | 0.182 | (0.030) | 0.167 | (0.009) |
| 22 quarters after entry | 0.225 | (0.019) | 0.313 | (0.054) | 0.222 | (0.032) | 0.201 | (0.009) |
| 23 quarters after entry | 0.220 | (0.021) | 0.292 | (0.051) | 0.156 | (0.033) | 0.190 | (0.010) |
| 24 quarters after entry | 0.240 | (0.021) | 0.337 | (0.051) | 0.219 | (0.035) | 0.204 | (0.010) |
| 25 quarters after entry | 0.231 | (0.023) | 0.287 | (0.050) | 0.173 | (0.035) | 0.189 | (0.010) |
| Enrolled | -0.135 | (0.014) | -0.105 | (0.020) | -0.183 | (0.025) | -0.146 | (0.006) |
| Constant | 0.001 | (0.000) | 0.002 | (0.001) | 0.000 | (0.004) | 0.002 | (0.000) |
| Observations | 761,300 | | 263,191 | | 194,341 | | 1,746,373 | |
| Quarters 17-20 Mean | 0.187 | (0.016) | 0.257 | (0.043) | 0.184 | (0.026) | 0.172 | (0.008) |
| Diff: For-Profit – Community College | Cert. | 0.003 | (0.003) | | Assoc. | 0.085 | (0.044) | |

Note: Coefficient estimates are from Equation 3, corresponding to Figure 2a. Standard errors are bootstrapped based on 1000 replications. Bottom two lines report means for quarters 17-20, standard error, and the difference between estimates for for-profit schools and community colleges, and the bootstrapped standard errors of the differences, by credential.

**Table A3b – Effect of School Attendance on Log Quarterly Earnings: Women**

| | For-profit | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Certificates | | Associate's | | Certificates | | Associate's | |
| | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. |
| 4 quarters prior to entry | 0.026 | (0.007) | 0.042 | (0.008) | 0.075 | (0.014) | 0.031 | (0.003) |
| 3 quarters prior to entry | 0.019 | (0.007) | 0.031 | (0.009) | 0.064 | (0.014) | 0.046 | (0.003) |
| 2 quarters prior to entry | 0.020 | (0.007) | 0.052 | (0.010) | 0.058 | (0.015) | 0.021 | (0.003) |
| 1 quarter prior to entry | -0.027 | (0.008) | 0.022 | (0.010) | 0.024 | (0.015) | 0.001 | (0.003) |
| Quarter of entry | -0.014 | (0.011) | 0.028 | (0.014) | 0.090 | (0.017) | 0.059 | (0.004) |
| 1 quarter after entry | -0.073 | (0.015) | 0.004 | (0.018) | 0.138 | (0.021) | 0.110 | (0.005) |
| 2 quarters after entry | -0.021 | (0.013) | 0.027 | (0.017) | 0.129 | (0.019) | 0.081 | (0.004) |
| 3 quarters after entry | -0.034 | (0.012) | 0.021 | (0.016) | 0.100 | (0.019) | 0.077 | (0.004) |
| 4 quarters after entry | -0.012 | (0.011) | 0.003 | (0.016) | 0.113 | (0.019) | 0.077 | (0.004) |
| 5 quarters after entry | -0.003 | (0.011) | -0.041 | (0.016) | 0.073 | (0.020) | 0.075 | (0.004) |
| 6 quarters after entry | 0.080 | (0.011) | -0.022 | (0.017) | 0.118 | (0.019) | 0.068 | (0.004) |
| 7 quarters after entry | 0.101 | (0.015) | 0.031 | (0.017) | 0.093 | (0.020) | 0.065 | (0.004) |
| 8 quarters after entry | 0.119 | (0.013) | 0.090 | (0.017) | 0.139 | (0.021) | 0.072 | (0.004) |
| 9 quarters after entry | 0.115 | (0.015) | 0.092 | (0.018) | 0.138 | (0.021) | 0.077 | (0.004) |
| 10 quarters after entry | 0.135 | (0.013) | 0.139 | (0.019) | 0.164 | (0.022) | 0.086 | (0.004) |
| 11 quarters after entry | 0.152 | (0.017) | 0.114 | (0.019) | 0.161 | (0.023) | 0.087 | (0.005) |
| 12 quarters after entry | 0.154 | (0.016) | 0.154 | (0.021) | 0.178 | (0.024) | 0.106 | (0.005) |
| 13 quarters after entry | 0.140 | (0.018) | 0.132 | (0.023) | 0.159 | (0.024) | 0.101 | (0.005) |
| 14 quarters after entry | 0.169 | (0.017) | 0.155 | (0.028) | 0.222 | (0.025) | 0.125 | (0.005) |
| 15 quarters after entry | 0.190 | (0.025) | 0.149 | (0.031) | 0.200 | (0.026) | 0.125 | (0.005) |
| 16 quarters after entry | 0.202 | (0.021) | 0.190 | (0.028) | 0.215 | (0.028) | 0.132 | (0.005) |
| 17 quarters after entry | 0.193 | (0.028) | 0.161 | (0.032) | 0.202 | (0.027) | 0.127 | (0.006) |
| 18 quarters after entry | 0.207 | (0.021) | 0.185 | (0.029) | 0.255 | (0.029) | 0.154 | (0.006) |
| 19 quarters after entry | 0.235 | (0.032) | 0.182 | (0.032) | 0.215 | (0.030) | 0.155 | (0.006) |
| 20 quarters after entry | 0.233 | (0.026) | 0.210 | (0.030) | 0.271 | (0.031) | 0.162 | (0.006) |
| 21 quarters after entry | 0.218 | (0.034) | 0.157 | (0.033) | 0.252 | (0.030) | 0.148 | (0.006) |
| 22 quarters after entry | 0.242 | (0.025) | 0.195 | (0.033) | 0.300 | (0.032) | 0.167 | (0.007) |
| 23 quarters after entry | 0.261 | (0.037) | 0.193 | (0.038) | 0.281 | (0.033) | 0.159 | (0.007) |
| 24 quarters after entry | 0.259 | (0.029) | 0.211 | (0.035) | 0.273 | (0.035) | 0.163 | (0.007) |
| 25 quarters after entry | 0.237 | (0.040) | 0.173 | (0.040) | 0.222 | (0.035) | 0.143 | (0.008) |
| Enrolled | -0.296 | (0.013) | -0.262 | (0.015) | -0.252 | (0.017) | -0.164 | (0.005) |
| Constant | 0.001 | (0.000) | 0.002 | (0.000) | -0.022 | (0.009) | 0.002 | (0.000) |
| Observations | 887,934 | | 473,377 | | 284,890 | | 2,893,457 | |
| Quarters 17-20 Mean | 0.217 | (0.024) | 0.185 | (0.028) | 0.236 | (0.028) | 0.150 | (0.005) |
| Diff: For-Profit – Community College | Cert. | -0.019 | (0.037) | | Assoc. | 0.035 | (0.037) | |

Note: Coefficient estimates are from Equation 3, corresponding to Figure 2b. Standard errors are bootstrapped based on 1000 replications. Bottom two lines report means for quarters 17-20, standard error, and the difference between estimates for for-profit schools and community colleges, and the bootstrapped standard errors of the differences, by credential.

**Table A4a – Effect of School Attendance on Quarterly Employment: Men**

| | For-profit | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Certificate | | Associate's | | Certificate | | Associate's | |
| | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. |
| 4 quarters prior to entry | 0.004 | (0.002) | 0.012 | (0.004) | 0.006 | (0.005) | 0.012 | (0.001) |
| 3 quarters prior to entry | 0.004 | (0.002) | 0.022 | (0.004) | 0.009 | (0.005) | 0.013 | (0.001) |
| 2 quarters prior to entry | 0.002 | (0.003) | 0.023 | (0.005) | 0.010 | (0.005) | 0.011 | (0.001) |
| 1 quarter prior to entry | -0.007 | (0.003) | 0.027 | (0.005) | 0.000 | (0.006) | 0.023 | (0.002) |
| Quarter of entry | -0.009 | (0.004) | 0.024 | (0.007) | 0.023 | (0.008) | 0.039 | (0.002) |
| 1 quarter after entry | -0.022 | (0.006) | 0.023 | (0.010) | 0.027 | (0.010) | 0.045 | (0.003) |
| 2 quarters after entry | -0.026 | (0.005) | 0.021 | (0.009) | 0.045 | (0.009) | 0.048 | (0.002) |
| 3 quarters after entry | -0.013 | (0.005) | 0.022 | (0.009) | 0.059 | (0.008) | 0.050 | (0.002) |
| 4 quarters after entry | -0.011 | (0.004) | 0.018 | (0.008) | 0.064 | (0.008) | 0.054 | (0.002) |
| 5 quarters after entry | -0.006 | (0.004) | 0.020 | (0.008) | 0.060 | (0.008) | 0.054 | (0.002) |
| 6 quarters after entry | 0.004 | (0.004) | 0.024 | (0.008) | 0.057 | (0.008) | 0.049 | (0.002) |
| 7 quarters after entry | 0.008 | (0.005) | 0.036 | (0.009) | 0.060 | (0.008) | 0.045 | (0.002) |
| 8 quarters after entry | 0.006 | (0.005) | 0.039 | (0.009) | 0.053 | (0.009) | 0.046 | (0.002) |
| 9 quarters after entry | 0.008 | (0.005) | 0.044 | (0.009) | 0.053 | (0.009) | 0.043 | (0.002) |
| 10 quarters after entry | 0.011 | (0.005) | 0.041 | (0.009) | 0.046 | (0.010) | 0.041 | (0.002) |
| 11 quarters after entry | 0.011 | (0.005) | 0.045 | (0.010) | 0.051 | (0.010) | 0.037 | (0.003) |
| 12 quarters after entry | 0.008 | (0.006) | 0.046 | (0.011) | 0.044 | (0.010) | 0.037 | (0.003) |
| 13 quarters after entry | 0.009 | (0.006) | 0.047 | (0.011) | 0.036 | (0.011) | 0.034 | (0.003) |
| 14 quarters after entry | 0.010 | (0.006) | 0.034 | (0.012) | 0.034 | (0.011) | 0.035 | (0.003) |
| 15 quarters after entry | 0.008 | (0.006) | 0.042 | (0.014) | 0.040 | (0.011) | 0.032 | (0.003) |
| 16 quarters after entry | 0.009 | (0.007) | 0.037 | (0.014) | 0.025 | (0.012) | 0.031 | (0.003) |
| 17 quarters after entry | 0.005 | (0.007) | 0.047 | (0.015) | 0.017 | (0.012) | 0.029 | (0.003) |
| 18 quarters after entry | 0.006 | (0.007) | 0.040 | (0.015) | 0.019 | (0.013) | 0.036 | (0.003) |
| 19 quarters after entry | 0.003 | (0.007) | 0.036 | (0.018) | 0.018 | (0.013) | 0.026 | (0.004) |
| 20 quarters after entry | -0.001 | (0.007) | 0.034 | (0.016) | 0.021 | (0.013) | 0.027 | (0.004) |
| 21 quarters after entry | -0.001 | (0.008) | 0.038 | (0.017) | 0.018 | (0.014) | 0.026 | (0.004) |
| 22 quarters after entry | -0.007 | (0.008) | 0.027 | (0.017) | 0.010 | (0.014) | 0.026 | (0.004) |
| 23 quarters after entry | -0.013 | (0.008) | 0.031 | (0.021) | 0.006 | (0.015) | 0.011 | (0.004) |
| 24 quarters after entry | -0.018 | (0.009) | 0.023 | (0.019) | 0.007 | (0.016) | 0.003 | (0.004) |
| 25 quarters after entry | -0.022 | (0.009) | 0.015 | (0.020) | -0.011 | (0.016) | -0.008 | (0.004) |
| Enrolled | -0.067 | (0.006) | -0.018 | (0.008) | -0.092 | (0.011) | -0.036 | (0.003) |
| Constant | 0.000 | (0.000) | 0.000 | (0.000) | 0.000 | (0.002) | 0.000 | (0.000) |
| Observations | 1,200,008 | | 431,969 | | 282,902 | | 2,721,522 | |
| Quarters 17-20 Mean | 0.003 | (0.007) | 0.039 | (0.012) | 0.019 | (0.016) | 0.030 | (0.003) |
| Diff: For-Profit – Community College | | Cert | -0.016 | (0.014) | | Assoc. | 0.010 | (0.016) |

Note: Coefficient estimates are from Equation 4, corresponding to Figure 3a. Standard errors are bootstrapped based on 1000 replications. Bottom two lines report means for quarters 17-20, standard error, and the difference between estimates for for-profit schools and community colleges, and the bootstrapped standard errors of the differences, by credential.

**Table A4b – Effect of School Attendance on Quarterly Employment: Women**

| | For-profit | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Certificate | | Associate's | | Certificate | | Associate's | |
| | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. | Coeff. | Std. Err. |
| 4 quarters prior to entry | 0.002 | (0.002) | 0.010 | (0.003) | -0.005 | (0.008) | 0.011 | (0.001) |
| 3 quarters prior to entry | 0.003 | (0.002) | 0.011 | (0.003) | -0.012 | (0.008) | 0.011 | (0.001) |
| 2 quarters prior to entry | 0.001 | (0.002) | 0.011 | (0.003) | -0.016 | (0.008) | 0.005 | (0.001) |
| 1 quarter prior to entry | -0.008 | (0.002) | 0.007 | (0.004) | -0.024 | (0.008) | 0.007 | (0.001) |
| Quarter of entry | 0.030 | (0.004) | 0.029 | (0.005) | 0.007 | (0.009) | 0.038 | (0.001) |
| 1 quarter after entry | 0.051 | (0.005) | 0.040 | (0.007) | 0.023 | (0.011) | 0.054 | (0.002) |
| 2 quarters after entry | 0.031 | (0.005) | 0.021 | (0.007) | 0.024 | (0.010) | 0.046 | (0.002) |
| 3 quarters after entry | 0.029 | (0.004) | 0.013 | (0.006) | 0.016 | (0.010) | 0.044 | (0.002) |
| 4 quarters after entry | 0.028 | (0.004) | -0.006 | (0.006) | 0.016 | (0.010) | 0.044 | (0.002) |
| 5 quarters after entry | 0.037 | (0.004) | -0.009 | (0.006) | 0.016 | (0.010) | 0.041 | (0.002) |
| 6 quarters after entry | 0.044 | (0.004) | 0.004 | (0.006) | 0.018 | (0.010) | 0.033 | (0.002) |
| 7 quarters after entry | 0.048 | (0.005) | 0.022 | (0.007) | 0.016 | (0.010) | 0.029 | (0.002) |
| 8 quarters after entry | 0.048 | (0.005) | 0.031 | (0.007) | 0.017 | (0.010) | 0.030 | (0.002) |
| 9 quarters after entry | 0.045 | (0.005) | 0.037 | (0.007) | 0.018 | (0.011) | 0.030 | (0.002) |
| 10 quarters after entry | 0.047 | (0.005) | 0.031 | (0.007) | 0.018 | (0.011) | 0.030 | (0.002) |
| 11 quarters after entry | 0.041 | (0.005) | 0.038 | (0.008) | 0.014 | (0.011) | 0.029 | (0.002) |
| 12 quarters after entry | 0.040 | (0.005) | 0.028 | (0.009) | 0.013 | (0.011) | 0.024 | (0.002) |
| 13 quarters after entry | 0.036 | (0.006) | 0.028 | (0.010) | 0.015 | (0.011) | 0.023 | (0.002) |
| 14 quarters after entry | 0.044 | (0.006) | 0.026 | (0.010) | 0.016 | (0.012) | 0.024 | (0.002) |
| 15 quarters after entry | 0.038 | (0.007) | 0.029 | (0.013) | 0.013 | (0.012) | 0.022 | (0.002) |
| 16 quarters after entry | 0.038 | (0.007) | 0.022 | (0.012) | 0.004 | (0.012) | 0.022 | (0.002) |
| 17 quarters after entry | 0.032 | (0.008) | 0.018 | (0.013) | 0.005 | (0.013) | 0.022 | (0.002) |
| 18 quarters after entry | 0.038 | (0.008) | 0.022 | (0.011) | -0.001 | (0.013) | 0.024 | (0.003) |
| 19 quarters after entry | 0.031 | (0.009) | 0.027 | (0.013) | 0.001 | (0.013) | 0.019 | (0.003) |
| 20 quarters after entry | 0.040 | (0.008) | 0.029 | (0.012) | 0.001 | (0.013) | 0.022 | (0.003) |
| 21 quarters after entry | 0.032 | (0.009) | 0.024 | (0.013) | -0.001 | (0.014) | 0.022 | (0.003) |
| 22 quarters after entry | 0.036 | (0.009) | 0.015 | (0.012) | -0.003 | (0.015) | 0.020 | (0.003) |
| 23 quarters after entry | 0.023 | (0.010) | 0.018 | (0.015) | -0.004 | (0.015) | 0.008 | (0.003) |
| 24 quarters after entry | 0.022 | (0.010) | 0.016 | (0.014) | -0.017 | (0.015) | 0.000 | (0.003) |
| 25 quarters after entry | 0.007 | (0.011) | 0.000 | (0.016) | -0.024 | (0.016) | -0.008 | (0.003) |
| Enrolled | -0.152 | (0.005) | -0.096 | (0.006) | -0.108 | (0.008) | -0.070 | (0.002) |
| Constant | 0.001 | (0.000) | 0.000 | (0.000) | 0.021 | (0.007) | 0.001 | (0.000) |
| Observations | 1,391,966 | | 717,489 | | 419,884 | | 4,227,248 | |
| Quarter 17-20 Mean | 0.035 | (0.007) | 0.024 | (0.012) | 0.002 | (0.013) | 0.022 | (0.002) |
| Diff: For-Profit – Community College | | Cert. | 0.034 | (0.014) | | Assoc. | 0.002 | (0.012) |

Note: Coefficient estimates are from Equation 4, corresponding to Figure 3b. Standard errors are bootstrapped based on 1000 replications. Bottom two lines report means for quarters 17-20, standard error, and the difference between estimates for for-profit schools and community colleges, and the bootstrapped standard errors of the differences, by credential.

**For Online Publication – Appendix B: Sensitivity Analysis**

The first sensitivity test explores the plausibility of the parallel trends assumption. In our context, this assumption implies that, when calendar quarter and age are controlled, there is no systematic difference in the earnings between quarters prior to the year before entry into schooling. We implement the test of this assumption suggested in Borusyak, Jaravel, and Spiess (2024) by estimating the effects of time until entry into schooling on the subsample of observations from 5 to 24 quarters before entry. The omitted time period is 21 to 24 quarters before entry. Table B1 shows the coefficients, as well as the test statistic for the joint significance of these pre-enrollment trend variables.[33] In five of the eight samples, we reject the null hypothesis of no difference between earnings in prior quarters against the alternative of joint significance. Borusyak, Jaravel, and Spiess (2024) interpret this result as suggestive that pre-enrollment trends exist. When the sample size is under 200,000 observations, we cannot reject the hypothesis that the pre-enrollment trends are zero, but when the sample size is over 400,000 we always reject the hypothesis at the 1-percent level. Given our large sample sizes, the critical issue is whether the observed differences are of substantive importance. Pre-enrollment trends may be so small that they do not affect our conclusions even if they are statistically significant.

Wing, Simon, and Bello-Gomez (2018) and Dynarski, Jacob, and Kriesman (2018) suggest that researchers consider including a linear time trend to control explicitly for differential time trends between groups. In our preferred fixed-effects model, such a linear time trend would be collinear with the calendar quarter and time until enrollment. Thus, to fit our model with such a time trend, we would have to omit controls for certain time periods, and our estimates would be identified by the choice of the reference period. If, instead, we use person random effects rather than fixed effects, the model can identify a linear time trend without excluding additional time periods. However, the underlying

[33] For this single-equation model, we report analytical standard errors, clustered at the individual level, and standard F-tests to identify joint significance.

assumptions implicit in a random-effects model differ from those of a fixed-effects model. Whereas the fixed-effects model allows individual fixed effects to vary over time and across age, the random-effects model assumes that the average individual effect does not change over time. It therefore identifies the model by assuming that once age and time effects are accounted for, there is no time trend in the mean person-specific effects used to identify the counterfactual earnings outcome.

Figures B1a and B1b contain the results from the random-effects model with a linear time trend. This framework differs in three respects from the fixed-effects model framework: (1) the first stage includes individual random effects (instead of fixed effects), (2) the first stage includes a linear time trend for quarters relative to school entry, and (3) this linear trend is used, along with estimates of age and calendar quarter effects, to predict subsequent counterfactual earnings. This approach assumes that the trend observed in prior earnings (up through five quarters before entry) would continue in the absence of schooling. Although not shown in the figure, the first-stage results show that the coefficient for the time trend is modest in size, always less than 0.004. For both men and women, the results in these figures are nearly identical to those from the fixed-effects framework. The similarity of results suggests that any deviation from parallel trends is sufficiently small that assuming parallel trends – as is done throughout the literature on for-profits and community colleges – will not bias the results in any meaningful way.

Next, we consider the traditional fixed-effects model estimated in one step, the model used in nearly all previous studies on community colleges and for-profits.[34] The results, in Figures B2a and B2b, show more similarity between sectors and degrees than the preferred model, with the exception of notably lower returns for associate's degrees in community colleges. As noted in the text, the similarity between results based on the single-step model and our two-step approach reflects the fact that our

[34] This model allows for the earnings increment to vary by time since enrollment, as in the pioneering work on community colleges by Jacobson, LaLonde and Sullivan (2005a,b).

model largely accounts for relevant heterogeneity, which is the cause of bias in the former model. Notwithstanding the similarity in results, we assume remaining differences reflect biases inherent in the one-step approach.

Our next sensitivity analysis estimates returns using a single post-entry dichotomous variable, which is the model used in several of the previous papers in this literature (e.g., Jepsen, Troske, Coomes 2014; Cellini and Chaudhary 2014; Cellini and Turner 2019). There are several possible reasons why comparing returns to schooling using a single post-school dichotomous variable in a fixed-effects model could produce misleading results. First, as noted above, in the presence of heterogeneous treatment effects, single-equation two-way fixed-effects models can generate estimates that are outside the range of actual effects that occur (de Chaisemartin and D'Haultfoeuille 2020; Borusyak, Jaravel, and Speiss 2024). However, our two-step method should produce an average effect estimate that weights all returns positively. A second issue is that if patterns of returns differ by time since school attendance, an average will be misleading insofar as early returns are a poor measure of the lifetime payoff of schooling. Finally, if groups vary in the timing of when they enrolled in school and therefore the number of post-school periods in the data, these groups' returns will appear to differ even if actual expected returns are the same.

To focus on the difference in using a series of dichotomous variables versus a single dichotomous variable, we continue to use the two-stage estimation routine described in Equations 1 through 3, keeping the variables for the four quarters before entry and quarter of entry in the model, but then collapsing the variables starting in the quarter after entry into a single post-entry dichotomous

variable.[35] As in our preferred specification, we estimate separate models by gender, type of school, and credential being pursued.

The results from estimating our model using a single post-entry dichotomous variable are presented in Table B2. Looking first at the returns for both men and women seeking certificates, the coefficients of the post-entry variable are smaller than our estimates of ultimate return, and they suggest for-profit schools have nearly a 3-percentage-point-lower return than community colleges, which is consistent with the findings reported in Cellini and Turner (2019). In the case of men, the single post-entry dichotomous variable hides the fact that returns to attending for-profit schools tend to grow faster over time than the returns to attending a community college, and by the sixth year after enrollment returns to attending a for-profit school often substantially exceed the returns to attending a community college. For those seeking associate's degrees, returns reported in Table B2 for for-profit schools appear higher than those for community colleges, but the relative advantage in returns for for-profit schools is generally smaller than that reported above for the earnings effect in the fifth and sixth years after enrollment.

The next model excludes controls for enrollment, providing a combined estimate of the returns to attendance and enrollment (Figures B3a and B3b).[36] These results are nearly the same as those from the preferred model.[37] Figures B4a and B4b present results of a model that includes categorical variables for age (<=20 years; 20<=years<25; 25<=years<30; 30<=years<40; 40<=years<50; >50 years) rather than a

[35] Some papers in the literature, such as Cellini and Turner (2019), measure the returns to schooling starting after a student leaves schools. However, since we include separate indicator variables for quarters when someone is enrolled in school, our model with a single post-entry dichotomous variable produces very similar estimates to such models.
[36] When we drop observations during enrollment, as in Cellini and Turner (2019), results – available upon request – are virtually identical starting nine quarters after entry (when nearly all students are no longer enrolled).
[37] The only notable change is that, if we exclude for-profit students who have missing exit dates (as no community college student has a missing exit date), the returns for men seeking associate's degrees are higher. These results are available from authors upon request.

cubic polynomial in age. The only substantial difference in this specification is a lower return for women pursuing certificates in community colleges.

In Figures B5a and B5b, the dependent variable is the earnings level in dollars, with values of zero for individuals with no reported earnings. Thus, the coefficients represent a combination of earnings and employment returns. The figures indicate substantial increases in earnings for all groups. For men, the highest gains in the second and third post-enrollment years are for certificates in community colleges. After that, men pursuing associate's degrees in for-profits have a steep increase, so that this group has the highest gains in the final year. The highest gains for women are for those pursuing certificates in community colleges, with notably higher returns starting in the fourth or fifth year after enrollment. These returns are similar in size to those for men. The other three groups of women have broadly similar returns, although the returns are slightly lower than those for men.

Next, we explore how the returns vary among different regression samples. First, we trim the sample of the top 1 percent and the bottom 1 percent of the observations, a robustness test reported in Cellini and Chaudhary (2014). Results are essentially unchanged for men, although women obtaining associate's degrees in for-profit schools experience larger earnings reductions in the first two years after entry and have slightly higher returns in later years in the trimmed sample (see Figures B6a and B6b). Second, we exclude students who study the academic/other field. Whereas more than half of students in community colleges are in such fields, in for-profit schools the figure is below 5 percent. The similarity in results with the preferred model implies that the difference in earnings and employment gains between school types are not driven by students pursuing academic fields of study in community colleges (Figures B7a and B7b). In Figures B8a and B8b, the returns for for-profit Title IV eligible schools[38] (as in Cellini and Turner 2019) are again similar to the full sample of eligible and ineligible

[38] All the community colleges are Title IV eligible. The percentages of students in for-profit schools attending Title IV eligible schools are 66.5 percent for students pursuing certificates and 92.4 percent for students pursuing associate's degrees.

schools (Figures 2a and 2b), with one notable exception. The returns for men pursuing certificates in for-profit schools are several percentage points smaller until the fifth year after entry in the subsample of Title IV eligible schools than in our full sample.

We also considered differences by length of time enrolled and year of enrollment. When looking at length of enrollment (Figures B9a through B9d), the returns are usually larger for students who enroll for longer periods, although in many cases differences are inconsistent. The pattern of returns is similar by year of entry (Figures B10a through B10d), although the returns for students who enrolled in the later period (2008 to 2012) are generally smaller and are imprecisely estimated at the end of the sample period.

Estimates might suffer bias if there were very few quarters of earnings before entry. Figures B11a and B11b provide estimates on the sample limited to students with at least ten earnings quarters in the period five to 24 quarters before entry. The patterns of estimates for both males and females across the types of returns are very similar to those for the full sample, although return estimates are slightly lower. Figures B12a and B12b present results for students who are more than 30 years of age at the time of enrollment. Although the relative returns are similar to those for the full sample, earnings decrements during the period immediately after entry are somewhat greater and returns increase by more over our follow-up period.

As noted above, the samples used to examine employment omit any string of zero earnings of length 10 or more quarters at the end of the observed earnings data or 30 quarters after entry for an individual.[39] Figures B13a through B13d show that the employment coefficients are sensitive to this choice, with higher estimated employment effects for the model that excludes strings of five or more

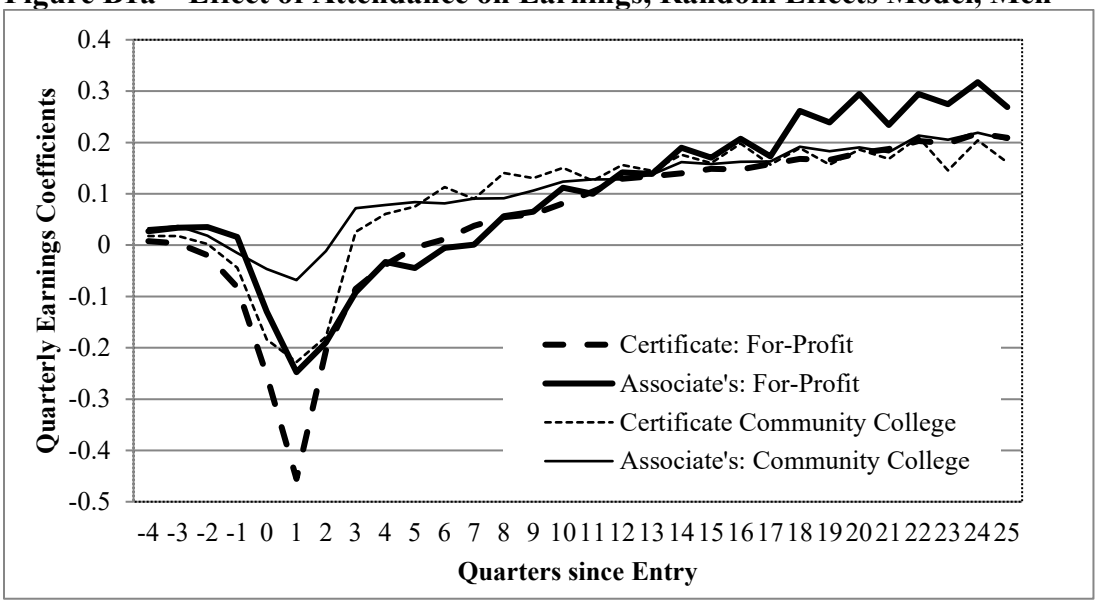
[39] This omission has no effect on estimates for log earnings, because these estimates are based on quarters with positive earnings.

zeros and lower estimates for the model that excludes strings of 15 or more zeros. However, the ranking of coefficients is stable across these changes.

The final sensitivity analysis considers whether to pool data across school type within gender-credential groups. We estimate pooled models that combine for-profit and community college students, with interaction terms between for-profit attendance and each control variable in the second-stage equation estimated in Equation 3.[40] We conduct separate regressions by gender and credential sought. Table B3 clearly shows that the results of the pooled analyses differ substantially from those of our preferred model based on analyses that separate for-profit and community college students. The formal assumption implied by pooling is that the income attainment processes for the two groups of students, prior to entry into schooling, are the same. In practice, researchers often pool data to improve precision as long as the processes are not "too different." In the case at hand, the very different results show that the prior processes differ substantially, meaning that the results of the pooled model are likely to be misleading (Goddereeis, and Haider 2010; and Słoczyński 2020).

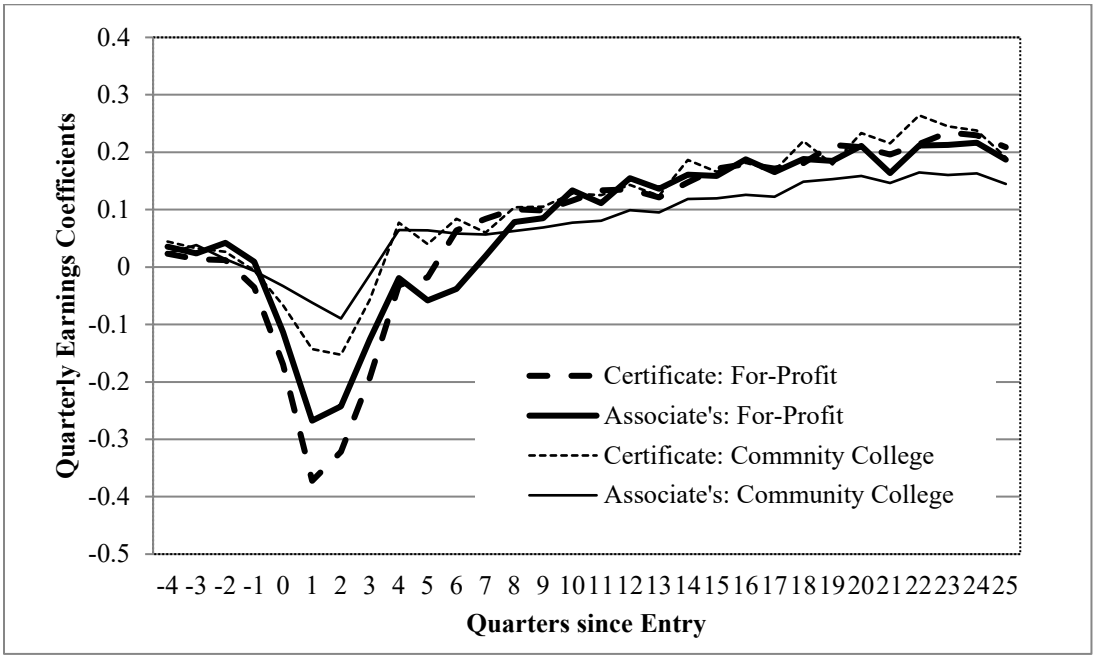
[40] Such interaction terms are not included in the first stage because they are absorbed by the person fixed effects.

**Figure B1a – Effect of Attendance on Earnings, Random Effects Model, Men**



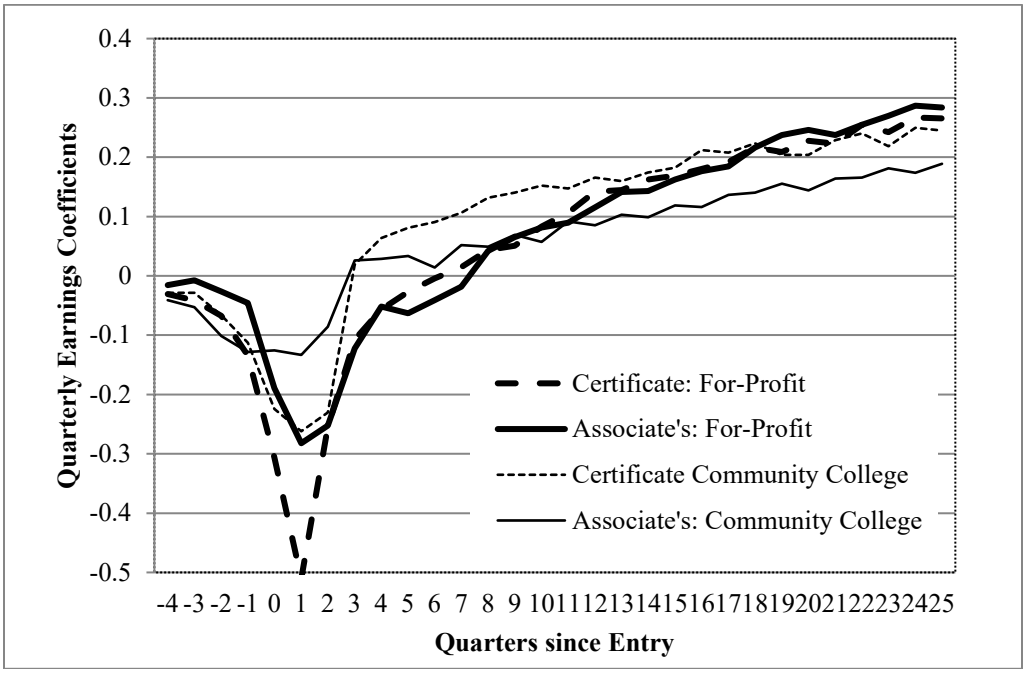

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regressions depicted in Equation 3, except that random effects are used rather than fixed effects, and prior earnings are modeled with a linear trend. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group. See text.

**Figure B1b – Effect of Attendance on Earnings, Random Effects Model, Women**



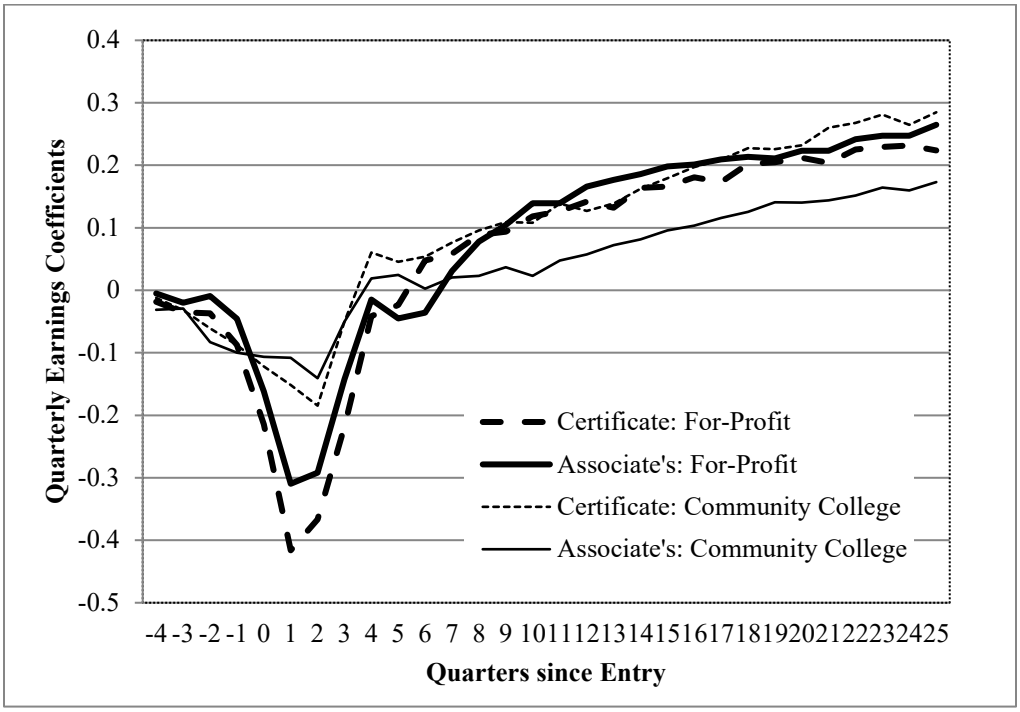

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regressions depicted in Equation 3, except that random effects are used rather than fixed effects, and prior earnings are modeled with a linear trend. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group. See text.

**Figure B2a – Effect of Attendance on Earnings, One-Step Fixed-Effects Model, Men**



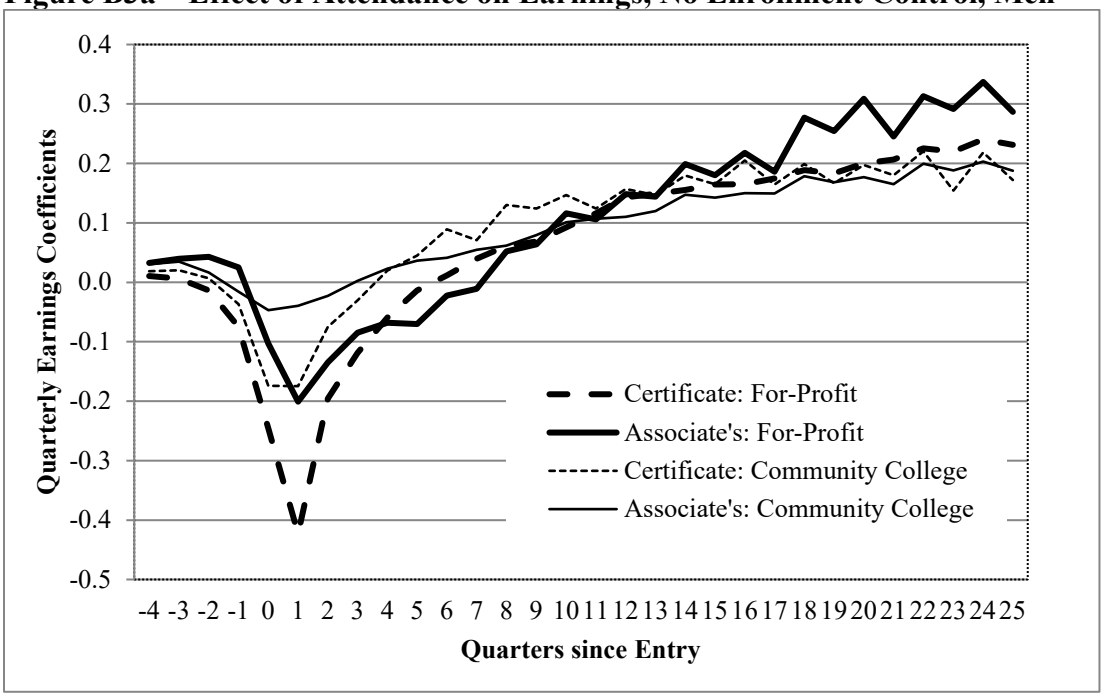

Note: Each data point is the effect estimate for the natural log of quarterly earnings from a single-equation model with person fixed effects. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B2b – Effect of Attendance on Earnings, One-Step Fixed-Effects Model, Women**



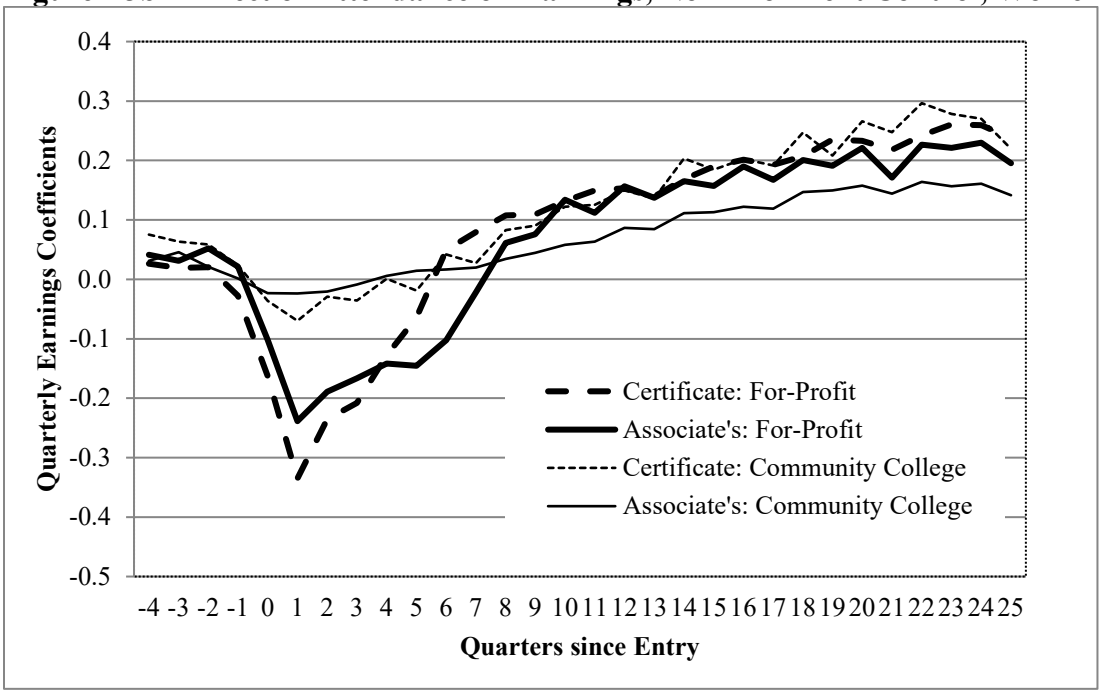

Note: Each data point is the effect estimate for the natural log of quarterly earnings from a single-equation model with person fixed effects. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B3a – Effect of Attendance on Earnings, No Enrollment Control, Men**



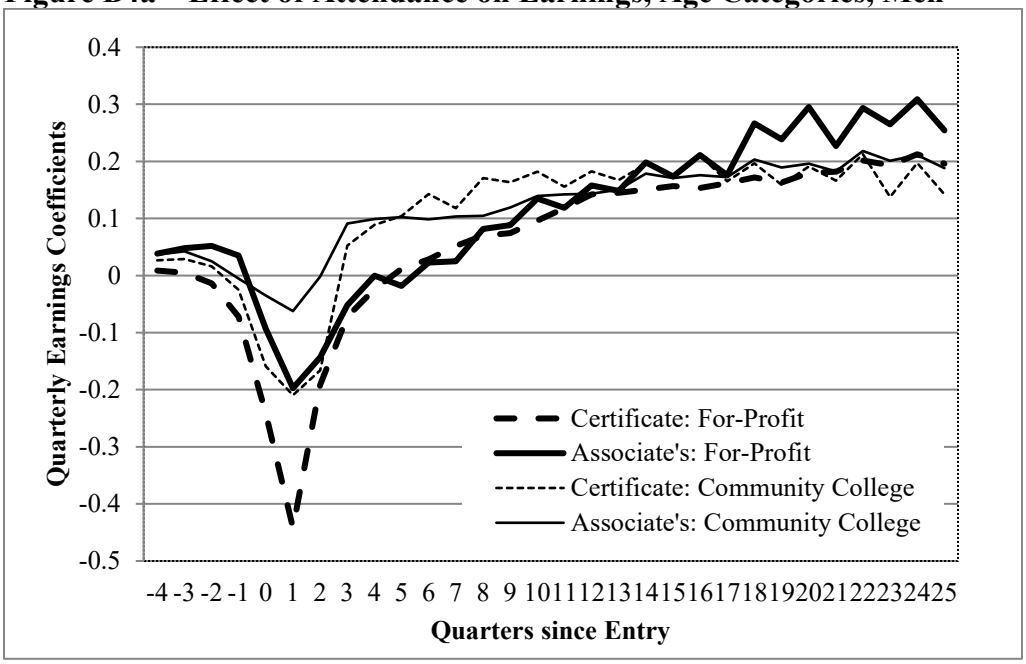

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3, excluding a control variable for enrollment.

**Figure B3b – Effect of Attendance on Earnings, No Enrollment Control, Women**



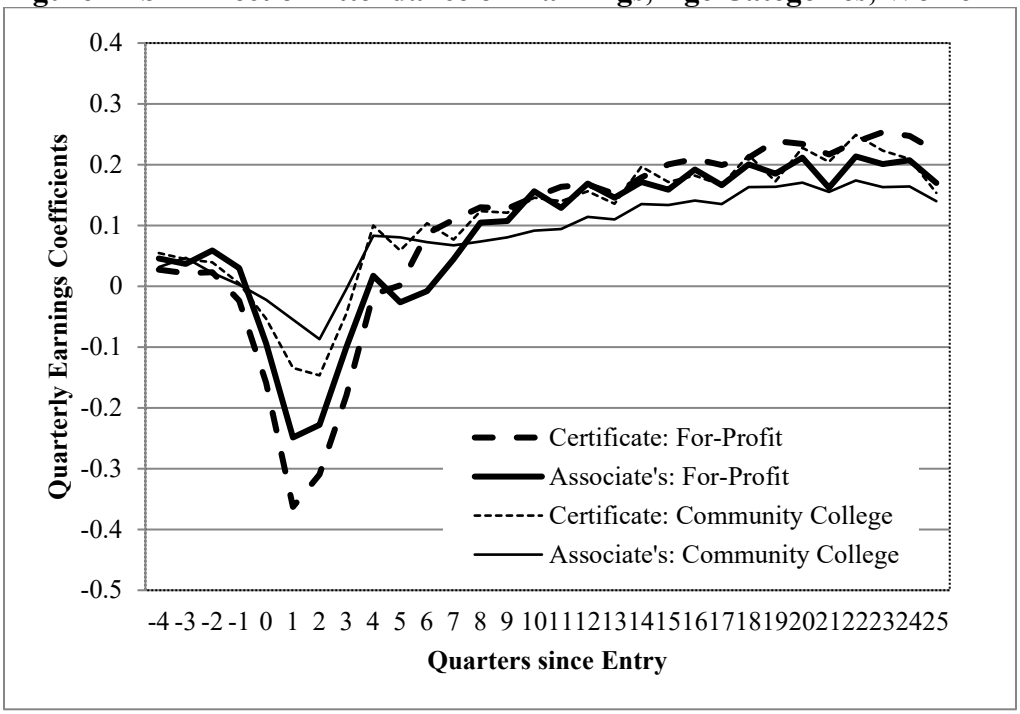

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3, excluding a control variable for enrollment.

**Figure B4a – Effect of Attendance on Earnings, Age Categories, Men**



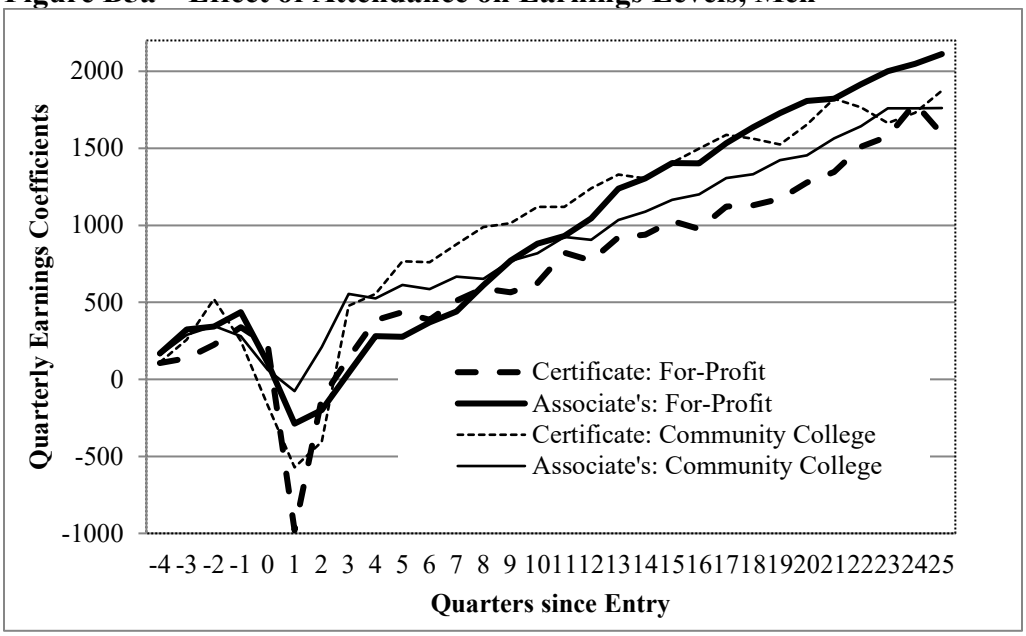

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3, except that age is modeled as a set of dummy variables (six categories) rather than as a cubic. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B4b – Effect of Attendance on Earnings, Age Categories, Women**



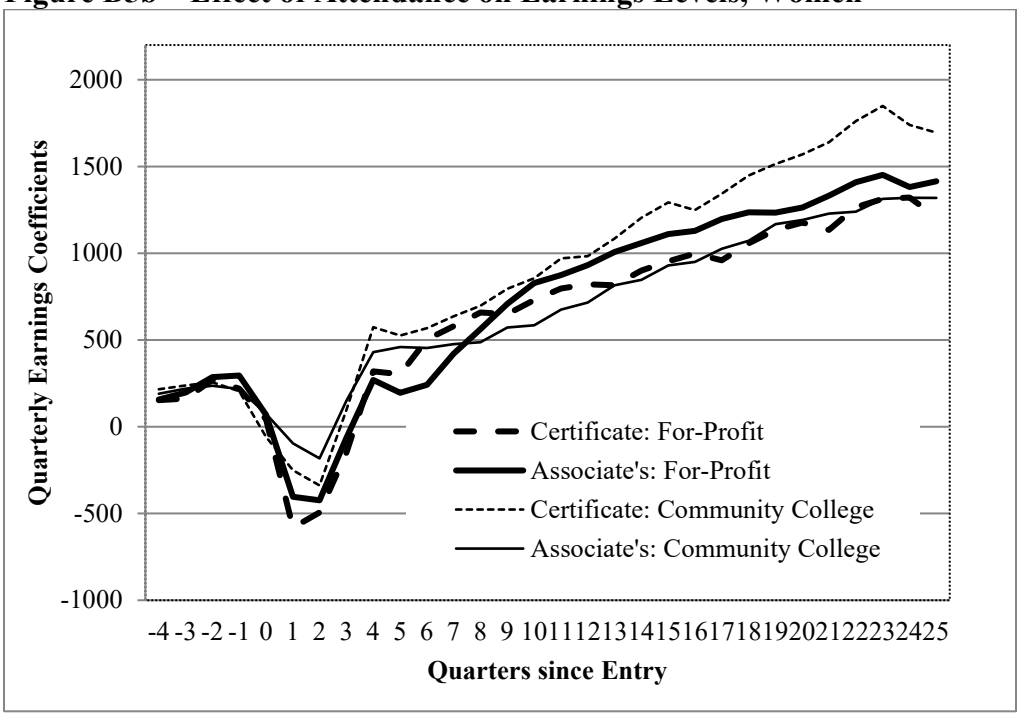

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3, except that age is modeled as set of dummy variables (six categories) rather than as a cubic. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B5a – Effect of Attendance on Earnings Levels, Men**



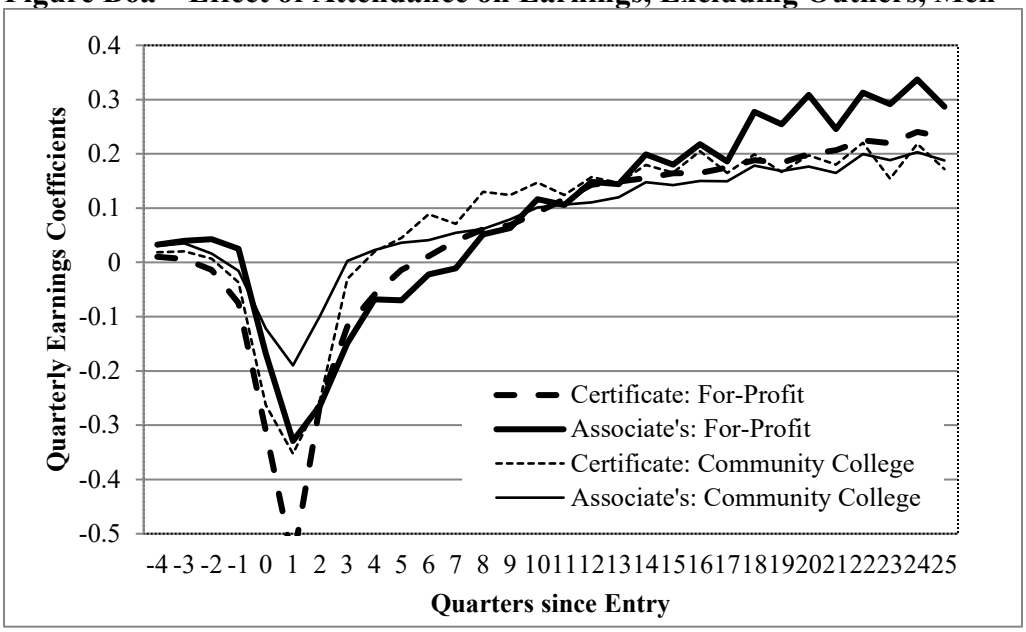

Note: Each data point is the effect estimate for quarterly earnings from the earnings regression in Equation 3 but with the dependent variable as earnings in dollars, not logs, including zeros. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B5b – Effect of Attendance on Earnings Levels, Women**



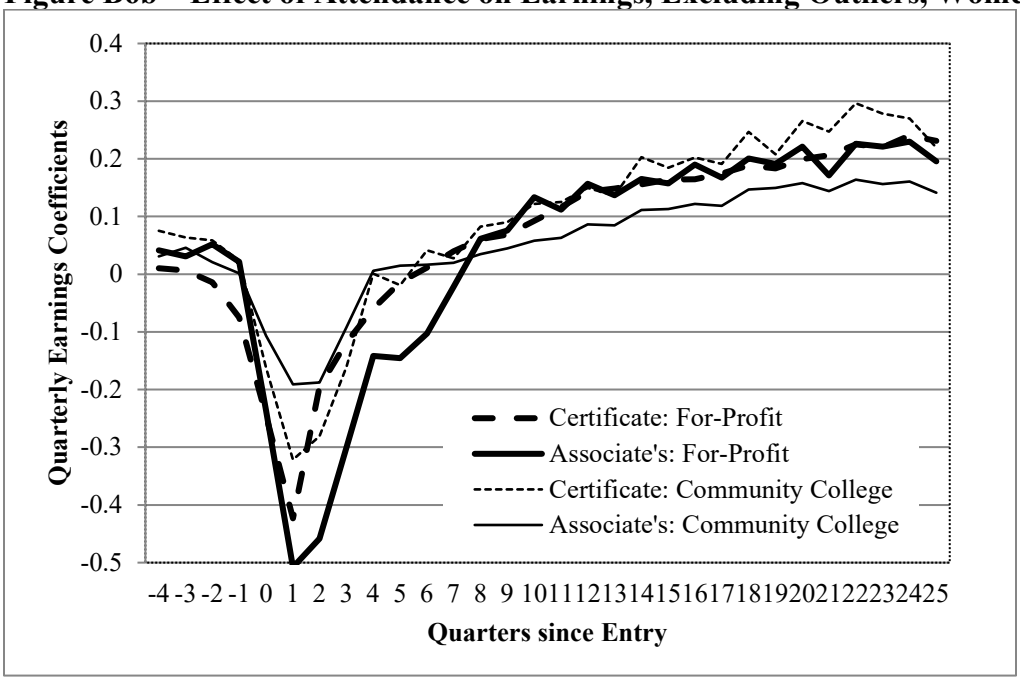

Note: Each data point is the effect estimate for quarterly earnings from the earnings regression in Equation 3 but with the dependent variable as earnings in dollars, not logs, and including zeros. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B6a – Effect of Attendance on Earnings, Excluding Outliers, Men**



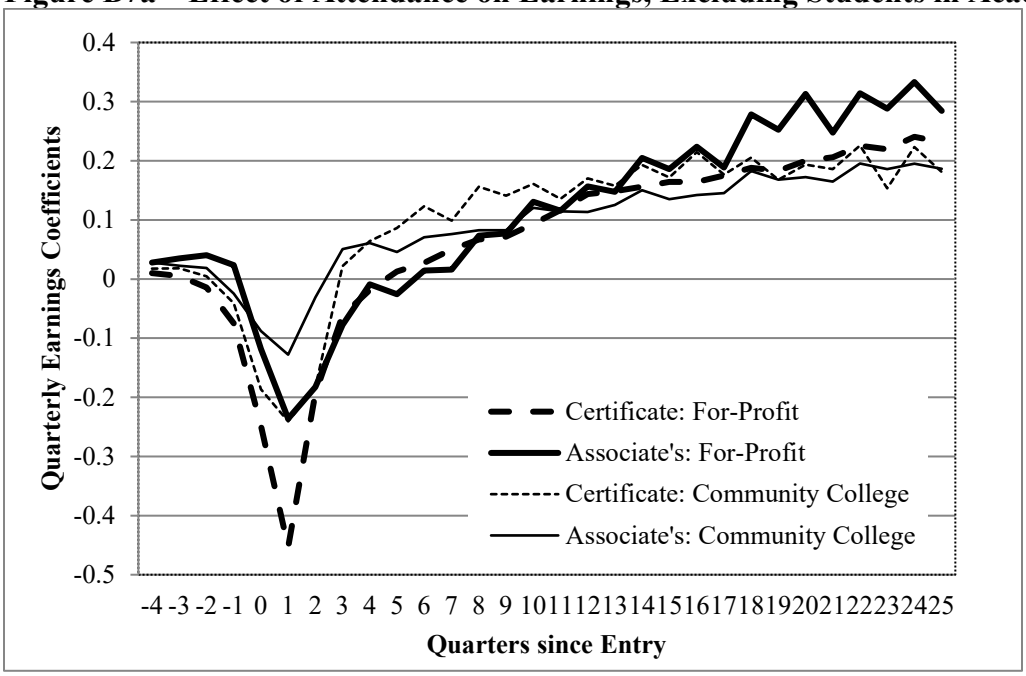

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Sample excludes earnings outliers. See text. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B6b – Effect of Attendance on Earnings, Excluding Outliers, Women**



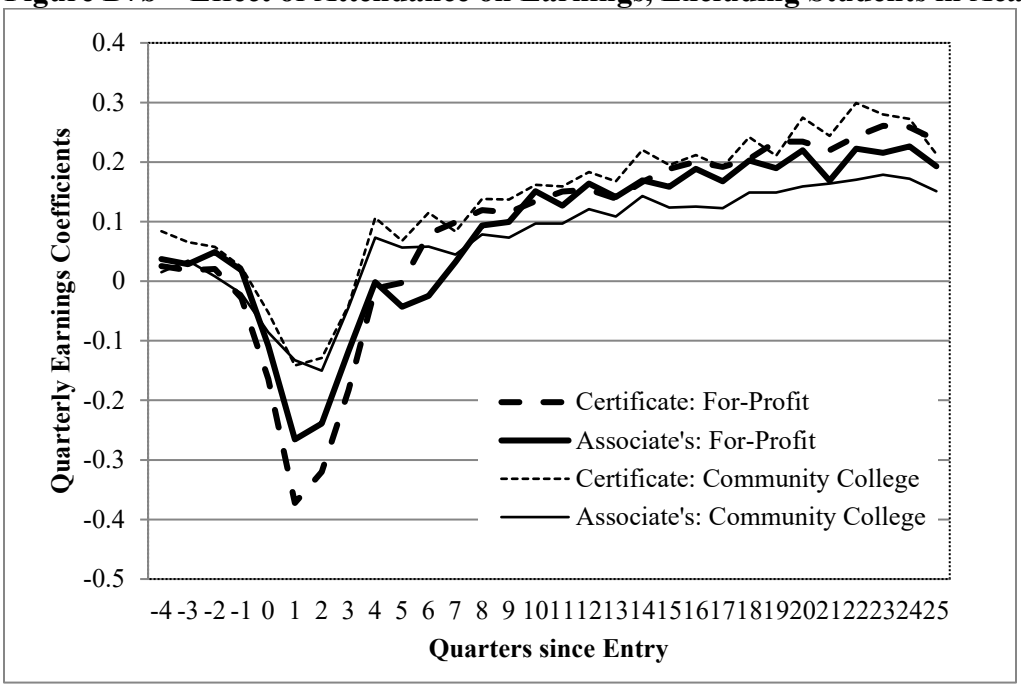

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Sample excludes earnings outliers. See text. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B7a – Effect of Attendance on Earnings, Excluding Students in Academic Subjects, Men**



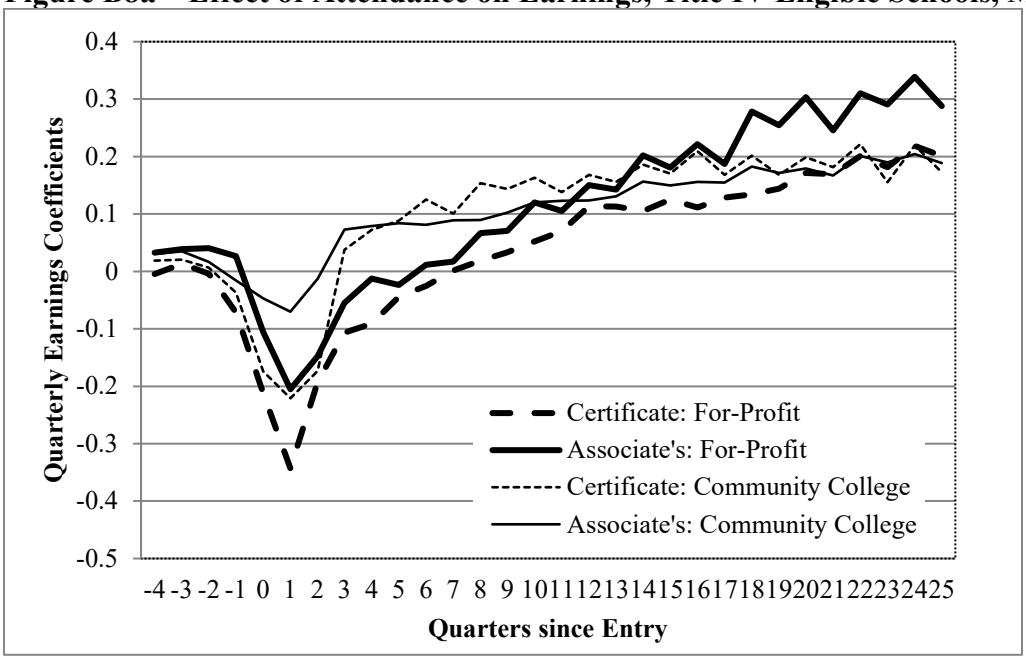

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Excludes students in academic fields of study. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B7b – Effect of Attendance on Earnings, Excluding Students in Academic Subjects, Women**



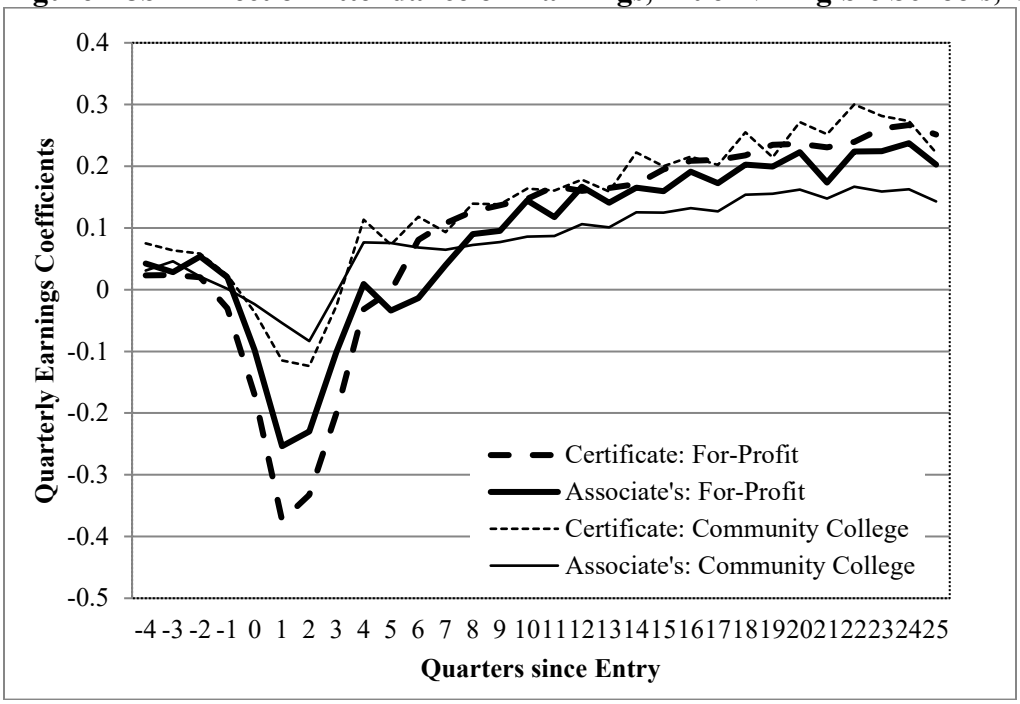

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Excludes students in academic fields of study. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B8a – Effect of Attendance on Earnings, Title IV Eligible Schools, Men**



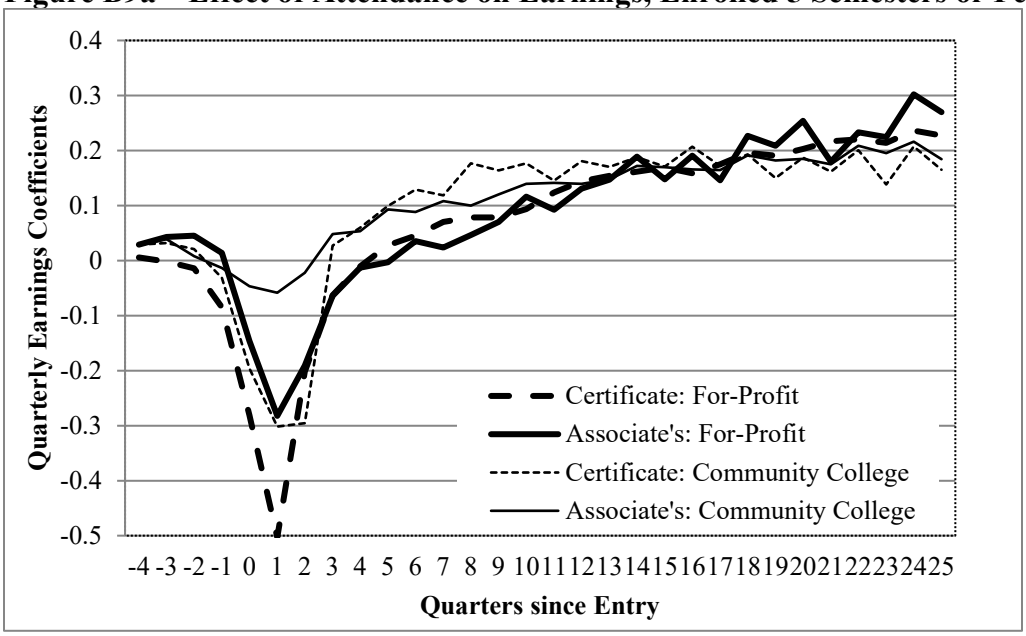

Note: Each data point is the effect estimate for the natural log of quarterly earnings based on Equation 3, estimated for the sample of students who attend Title IV eligible schools. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B8b – Effect of Attendance on Earnings, Title IV Eligible Schools, Women**



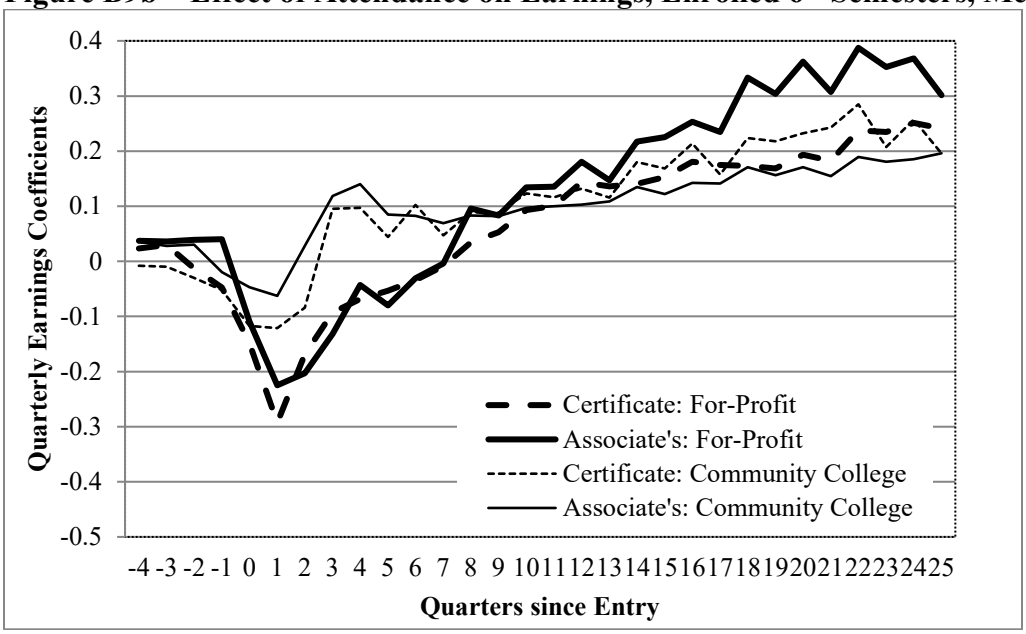

Note: Each data point is the effect estimate for the natural log of quarterly earnings based on Equation 3, estimated for the sample of students who attend Title IV eligible schools. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B9a – Effect of Attendance on Earnings, Enrolled 5 Semesters or Fewer, Men**



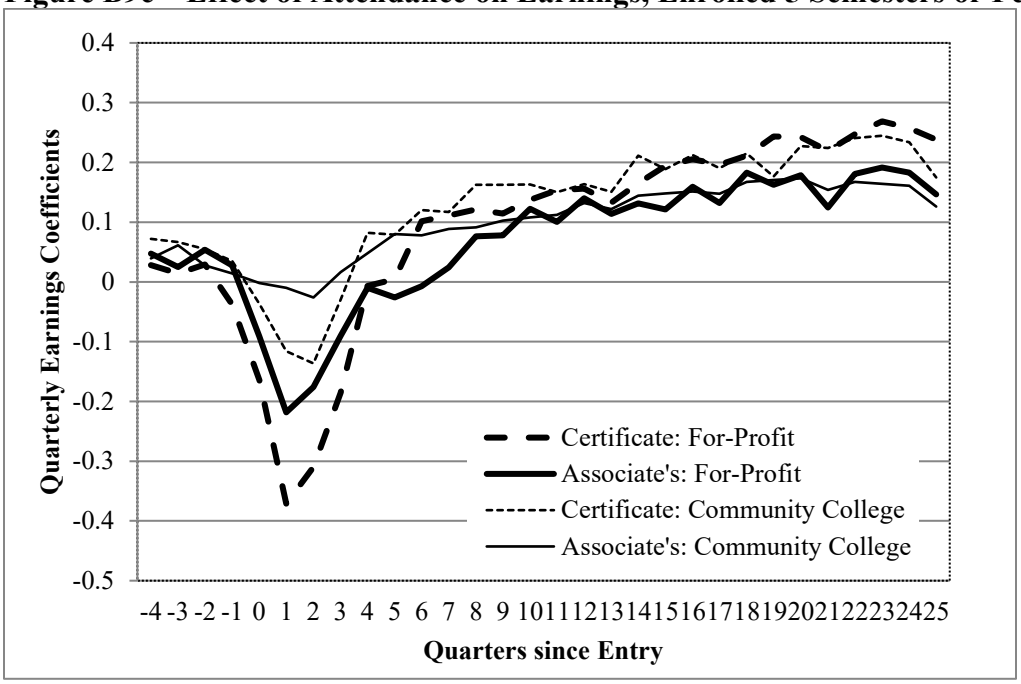

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who enrolled for five or fewer semesters. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B9b – Effect of Attendance on Earnings, Enrolled 6+ Semesters, Men**



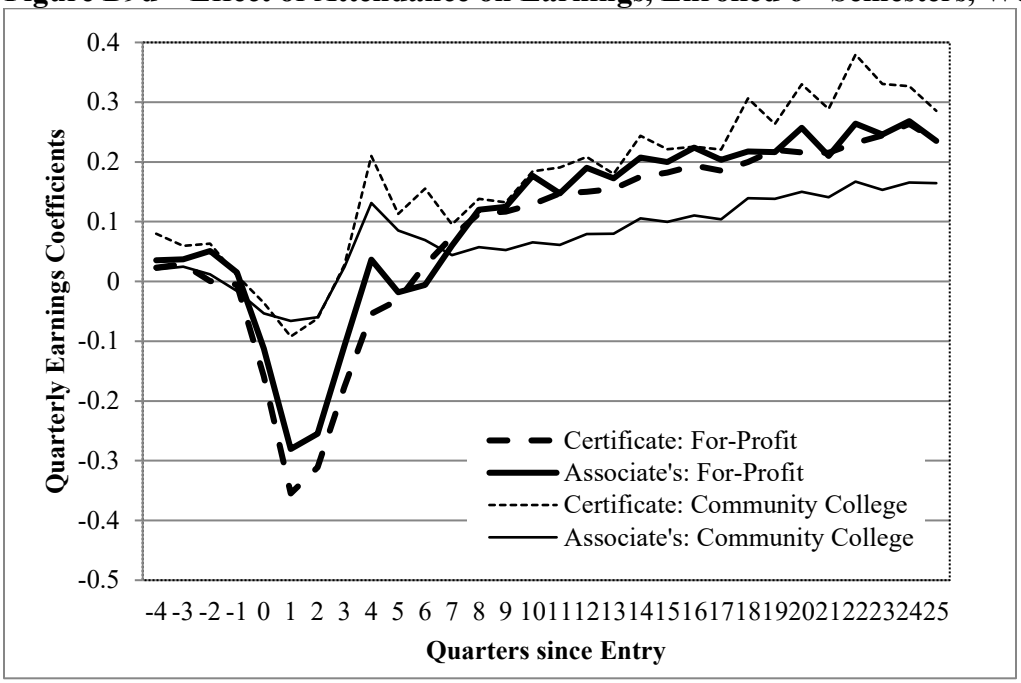

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who enrolled for six or more semesters. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B9c – Effect of Attendance on Earnings, Enrolled 5 Semesters or Fewer, Women**



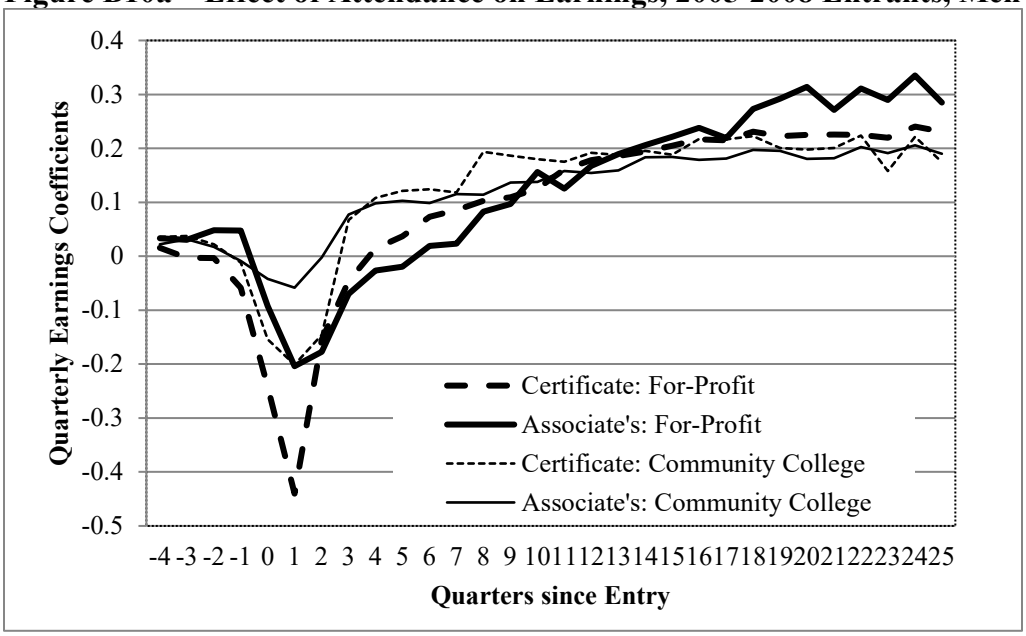

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who enrolled for five or fewer semesters. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B9d – Effect of Attendance on Earnings, Enrolled 6+ Semesters, Women**



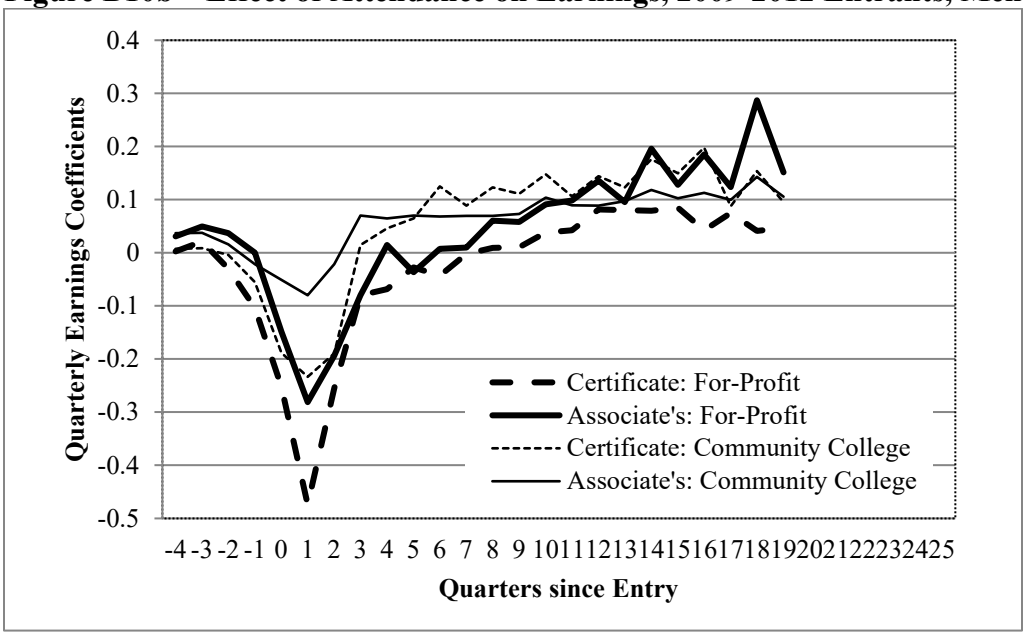

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who enrolled for six or more semesters. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B10a – Effect of Attendance on Earnings, 2005-2008 Entrants, Men**



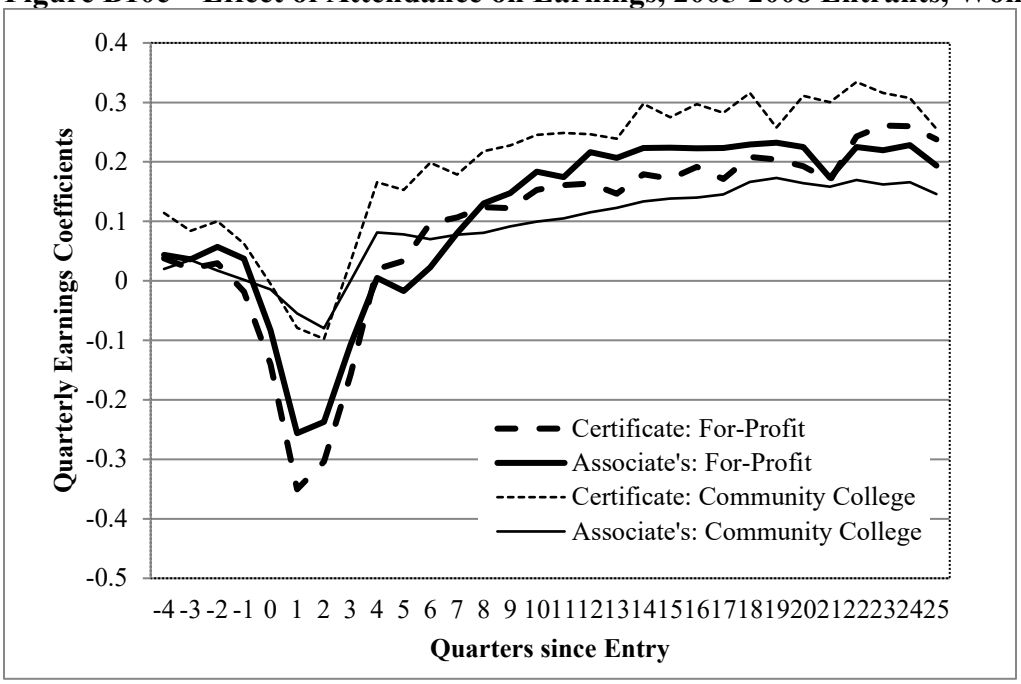

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who entered in 2005 to 2008. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B10b – Effect of Attendance on Earnings, 2009-2012 Entrants, Men**



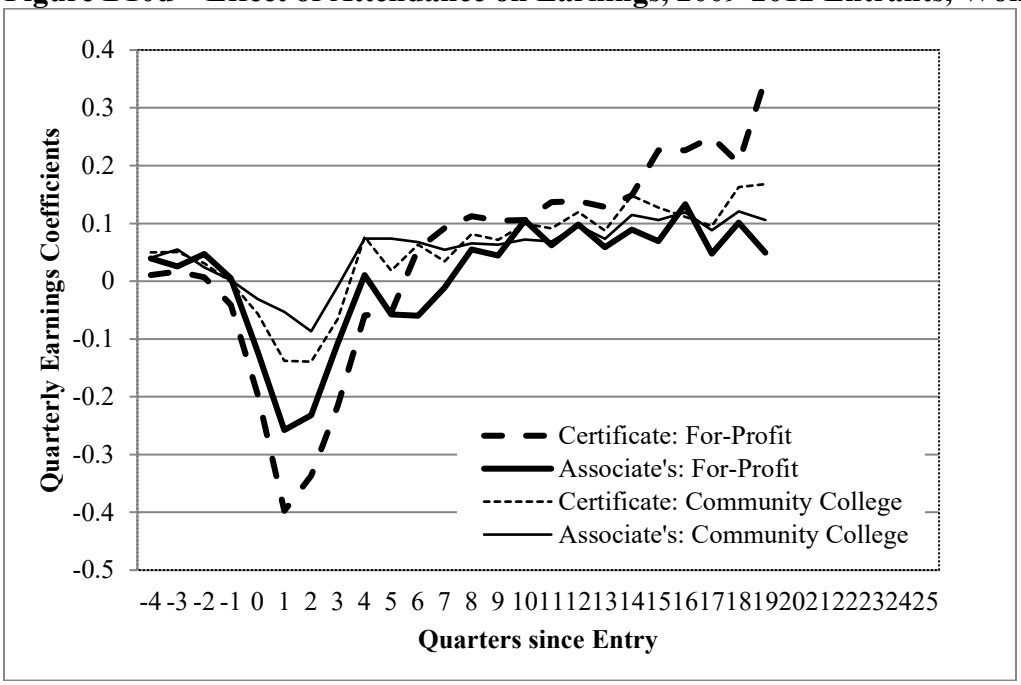

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who entered in 2009 to 2012. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B10c – Effect of Attendance on Earnings, 2005-2008 Entrants, Women**



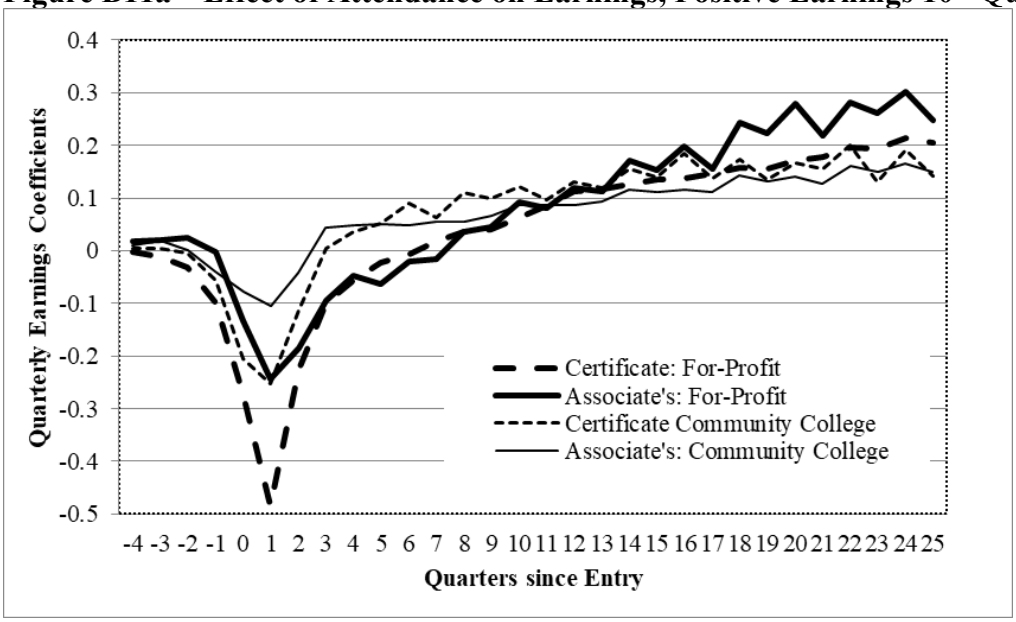

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who entered in 2005 to 2008. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B10d – Effect of Attendance on Earnings, 2009-2012 Entrants, Women**



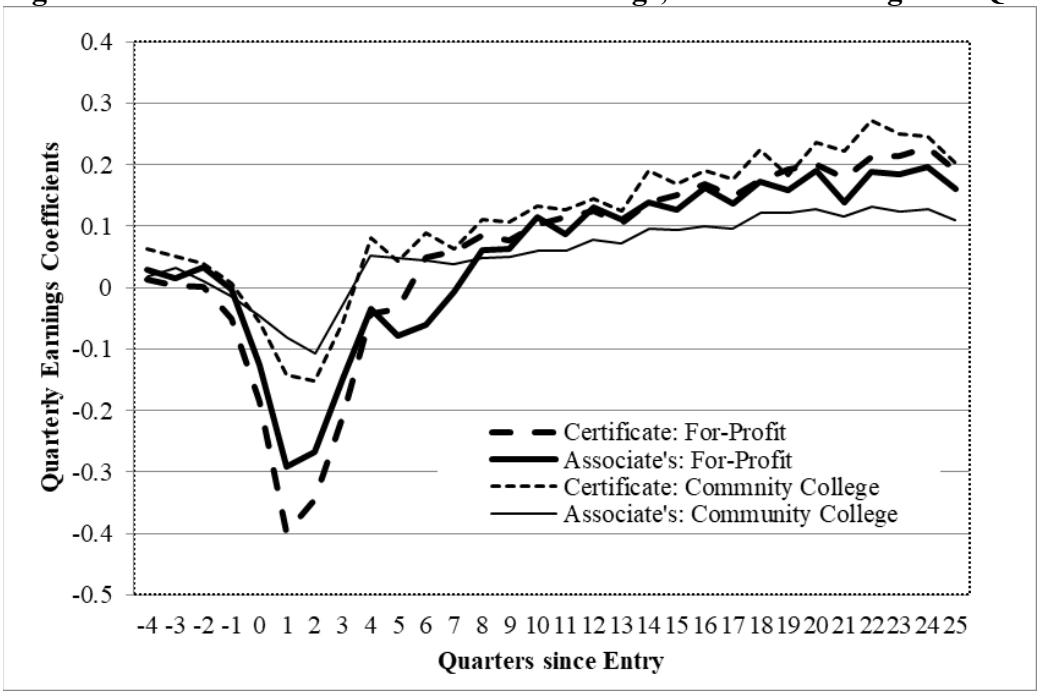

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students who entered in 2009 to 2012. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B11a – Effect of Attendance on Earnings, Positive Earnings 10+ Quarters, Men**



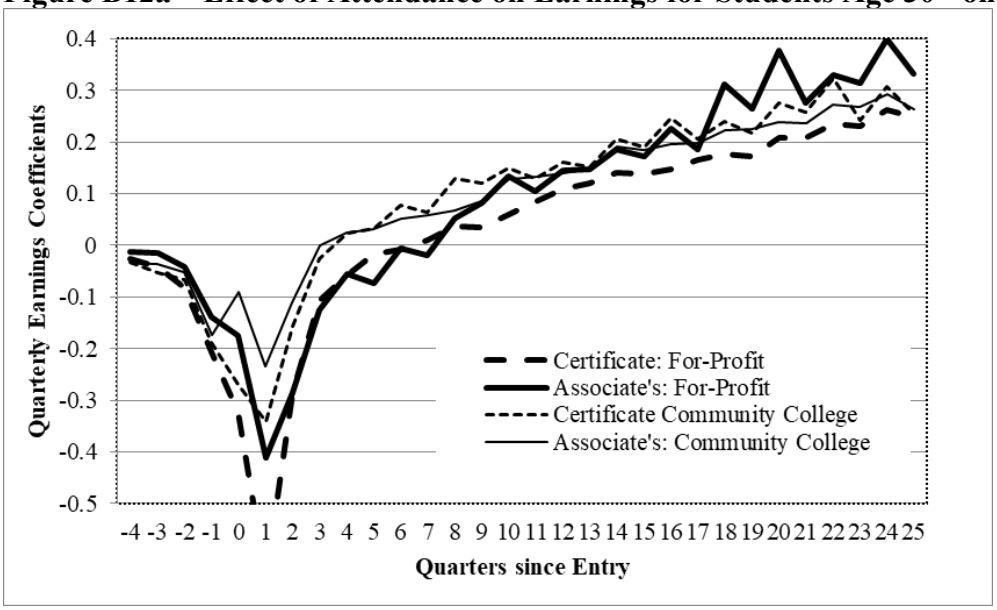

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students with positive earnings in at least 10 quarters five to 24 quarters before entry. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B11b – Effect of Attendance on Earnings, Positive Earnings 10+ Quarters, Women**



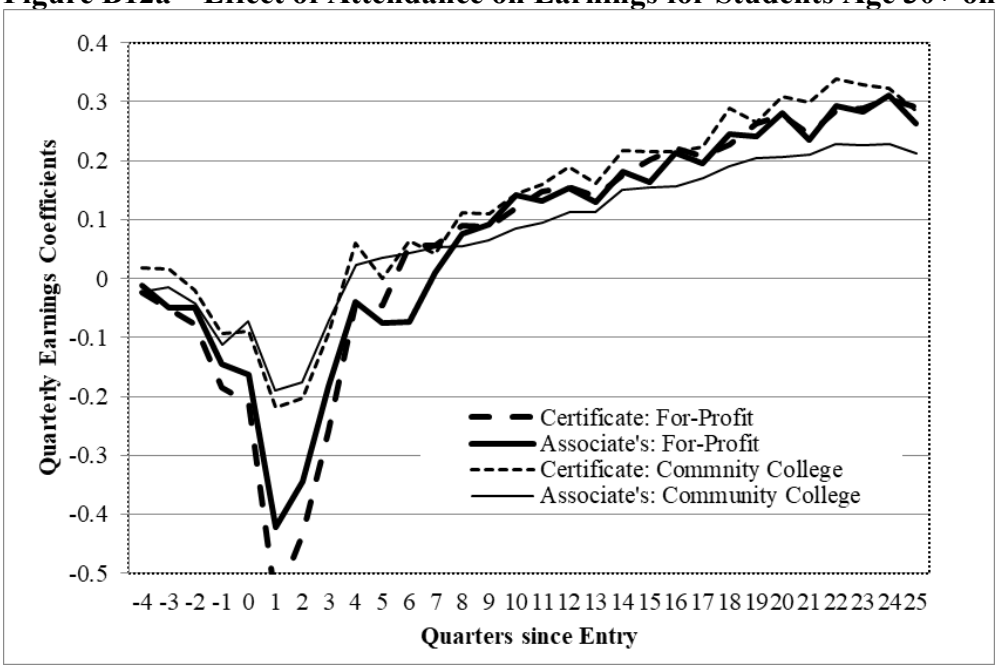

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students with positive earnings in at least 10 quarters five to 24 quarters before entry. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B12a – Effect of Attendance on Earnings for Students Age 30+ on Entry, Men**



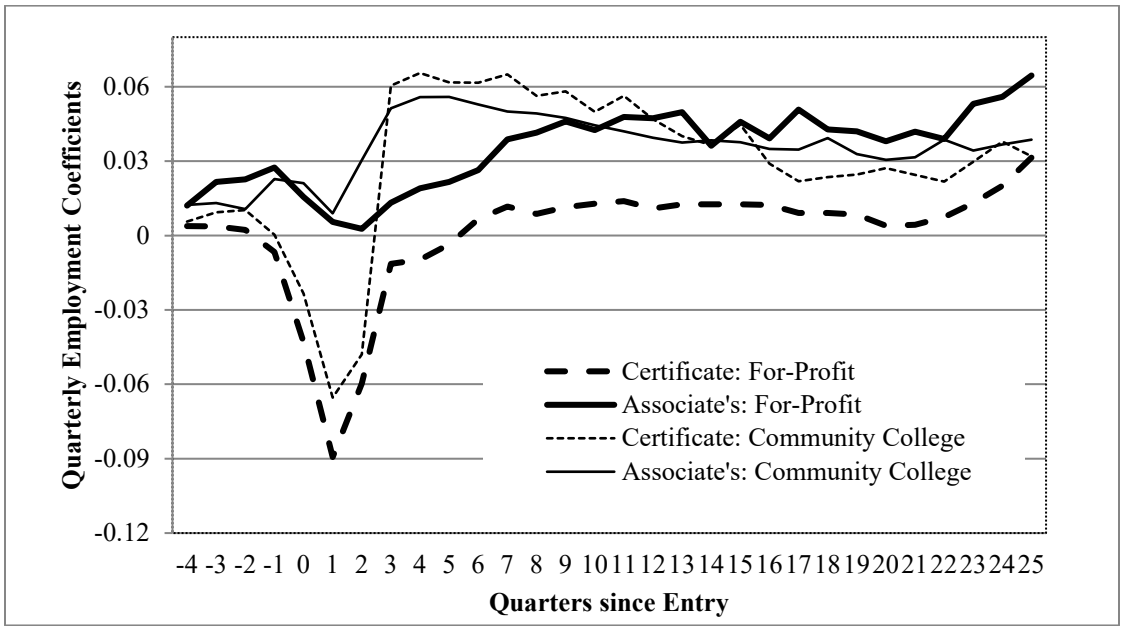

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students age at least 30 on entry. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B12a – Effect of Attendance on Earnings for Students Age 30+ on Entry, Women**



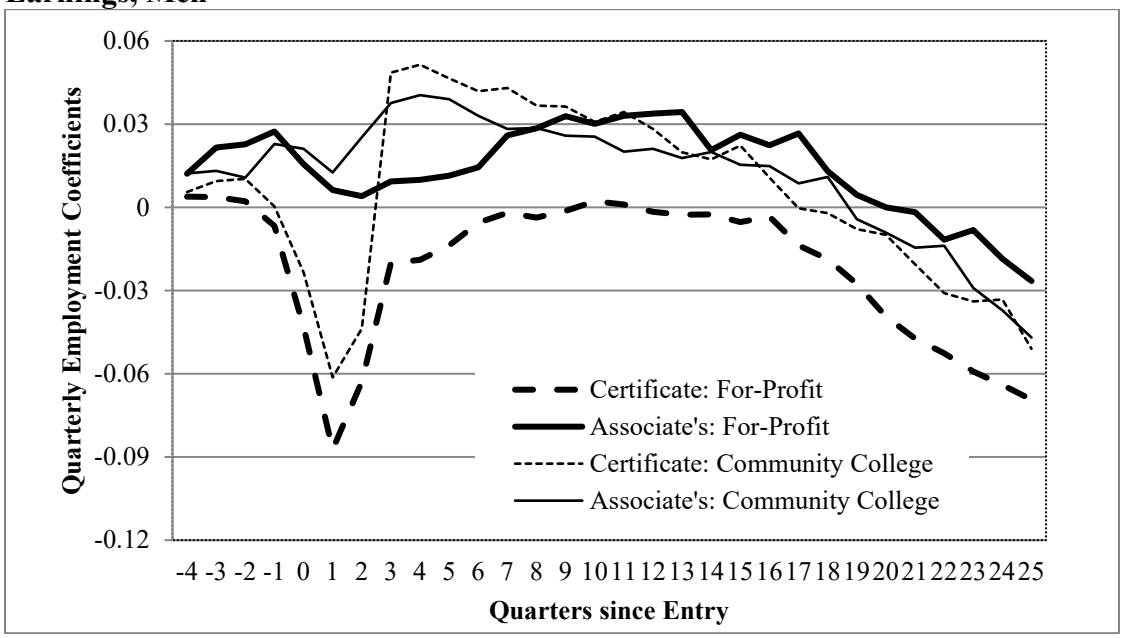

Note: Each data point is the effect estimate for the natural log of quarterly earnings from the earnings regression in Equation 3. Includes students age at least 30 on entry. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable, with the period of enrollment taken as the mean for each group.

**Figure B13a – Effect of Attendance on Employment, Excluding Strings of 5+ Quarters of Zero Earnings, Men**



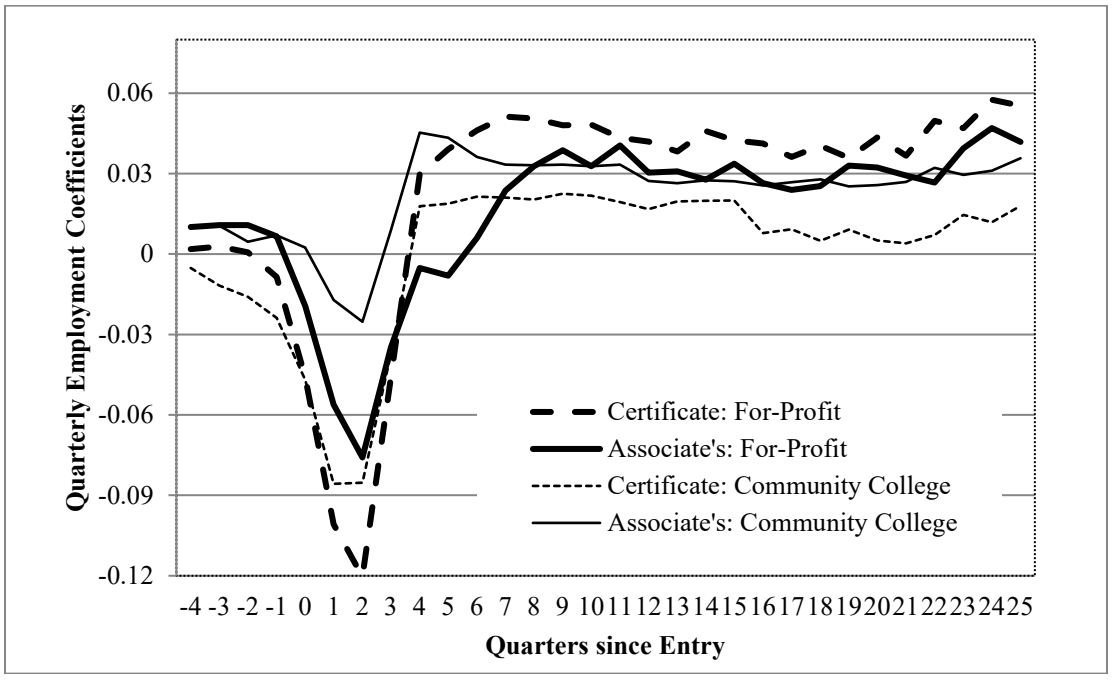

Note: Each data point is the effect estimate for quarterly employment based on Equation 4. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. Strings of zero earnings of 5+ quarters at the end of the sample period are excluded.

**Figure B13b – Effect of Attendance on Employment, Excluding Strings of 15+ Quarters of Zero Earnings, Men**



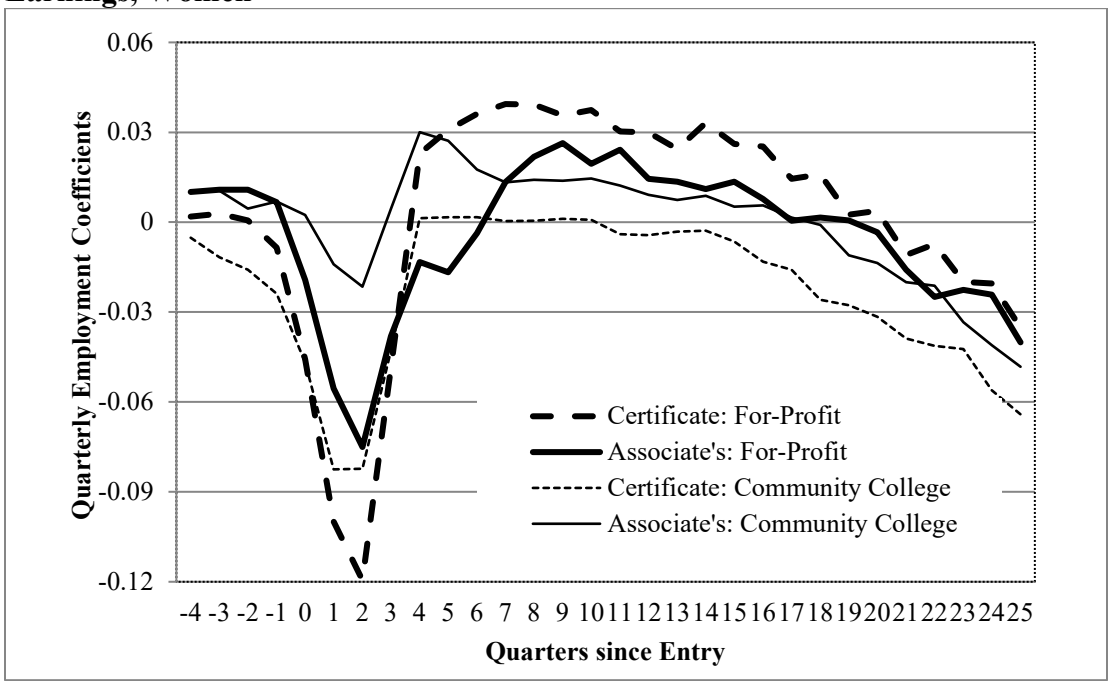

Note: Each data point is the effect estimate for quarterly employment based on Equation 4. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. Strings of zero earnings of 15+ quarters at the end of the sample period are excluded.

**Figure B13c – Effect of Attendance on Employment, Excluding Strings of 5+ Quarters of Zero Earnings, Women**




Note: Each data point is the effect estimate for quarterly employment based on Equation 4. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. Strings of zero earnings of 5+ quarters at the end of the sample period are excluded.

**Figure B13d – Effect of Attendance on Employment, Excluding Strings of 15+ Quarters of Zero Earnings, Women**




Note: Each data point is the effect estimate for quarterly employment based on Equation 4. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. Strings of zero earnings of 15+ quarters at the end of the sample period are excluded.

**Table B1 – Pre-Enrollment Coefficients in Log Earnings Regression, Quarters 5-24 before Enrollment**

| | For Profits | | | | Community College | | | |
|---|---|---|---|---|---|---|---|---|
| | Men | | Women | | Men | | Women | |
| | Certificate | Associate's | Certificate | Associate's | Certificate | Associate's | Certificate | Associate's |
| 20 quarters before entry | -0.004 | -0.017 ** | -0.020 * | -0.017 ** | -0.023 *** | -0.01 | -0.013 *** | -0.003 |
| 19 quarters before entry | -0.017 | -0.022 *** | -0.017 | -0.030 *** | -0.021 *** | -0.004 | -0.013 *** | -0.004 |
| 18 quarters before entry | -0.017 | -0.028 *** | -0.015 | -0.019 ** | -0.029 *** | -0.0004 | -0.023 *** | -0.01 |
| 17 quarters before entry | -0.039 ** | -0.036 *** | -0.018 | -0.019 ** | -0.028 *** | -0.005 | -0.020 *** | -0.008 |
| 16 quarters before entry | -0.023 | -0.031 *** | -0.016 | -0.024 ** | -0.044 *** | 0.010 | -0.020 *** | -0.004 |
| 15 quarters before entry | -0.029 | -0.032 *** | -0.013 | -0.034 *** | -0.033 *** | -0.014 | -0.025 *** | -0.006 |
| 14 quarters before entry | -0.027 | -0.04 *** | -0.022 | -0.026 ** | -0.049 *** | 0.006 | -0.028 *** | 0.006 |
| 13 quarters before entry | -0.033 | -0.035 *** | -0.007 | -0.020 | -0.041 *** | -0.004 | -0.025 *** | 0.000 |
| 12 quarters before entry | -0.024 | -0.034 ** | -0.031 | -0.024 * | -0.037 *** | -0.012 | -0.021 *** | -0.004 |
| 11 quarters before entry | -0.031 | -0.04 ** | -0.01 | -0.033 ** | -0.036 *** | -0.007 | -0.009 ** | 0.009 |
| 10 quarters before entry | -0.027 | -0.047 *** | -0.01 | -0.027 * | -0.046 *** | -0.004 | -0.019 *** | 0.020 |
| 9 quarters before entry | -0.034 | -0.038 ** | 0.007 | -0.018 | -0.034 *** | 0.012 | -0.014 *** | 0.011 |
| 8 quarters before entry | -0.046 | -0.039 ** | -0.007 | -0.016 | -0.039 *** | 0.003 | -0.008 | 0.020 |
| 7 quarters before entry | -0.018 | -0.035 * | 0.005 | -0.014 | -0.026 *** | 0.014 | 0.005 | 0.037 |
| 6 quarters before entry | -0.011 | -0.035 | -0.001 | -0.003 | -0.039 *** | 0.025 | -0.002 | 0.041 |
| 5 quarters before entry | -0.014 | -0.024 | 0.018 | 0.015 | -0.02 ** | 0.052 | 0.016 *** | 0.055 * |
| F stat of joint signific. | 1.36 | 2.31 | 1.42 | 3.92 | 8.08 | 1.44 | 12.99 | 1.71 |
| Prob > F | 0.149 | 0.002 | 0.123 | 0 | 0 | 0.111 | 0 | 0.037 |
| Observations | 116,973 | 403,882 | 159,880 | 425,445 | 868,702 | 131,715 | 1,477,519 | 229,342 |
| R-squared | 0.233 | 0.036 | 0.114 | 0.129 | 0.122 | 0.048 | 0.119 | 0.03 |

NOTES: The regressions only contain observations for 5 to 24 quarters before enrollment. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively, for a two-sided test. Each column is from a separate regression. All regressions include a cubic in age, as well as calendar quarter and person fixed effects. Standard errors are clustered at the person level.

**Table B2 – Estimated Returns for Single Post-Enrollment Dummy Variable, by Credential Type and Gender**

| | Certificate | | Associate's Degree | |
|---|---|---|---|---|
| | For Profits | Community College | For Profits | Community College |
| Men | Coef./SE | Coef./SE | Coef./SE | Coef./SE |
| Post Enrollment | 0.116 *** | 0.143 *** | 0.156 *** | 0.129 *** |
| | 0.014 | 0.022 | 0.030 | 0.007 |
| 4 quarters prior | 0.011 | 0.019 | 0.032 ** | 0.031 *** |
| | 0.007 | 0.014 | 0.013 | 0.004 |
| 3 quarters prior | 0.006 | 0.020 | 0.040 *** | 0.035 *** |
| | 0.007 | 0.015 | 0.013 | 0.005 |
| 2 quarters prior | -0.014 * | 0.007 | 0.043 *** | 0.016 *** |
| | 0.008 | 0.016 | 0.014 | 0.005 |
| 1 quarter prior | -0.075 * | -0.037 ** | 0.025 * | -0.016 * |
| | 0.009 | 0.017 | 0.015 | 0.005 |
| Entry Quarter | -0.044 * | -0.034 | 0.041 * | 0.050 * |
| | 0.012 | 0.022 | 0.022 | 0.006 |
| Enrollment | -0.404 *** | -0.280 *** | -0.290 *** | -0.193 *** |
| | 0.013 | 0.021 | 0.024 | 0.006 |
| Observations | 761,300 | 194,341 | 263,191 | 1,746,373 |
| Adj. R-squared | 0.011 | 0.010 | 0.009 | 0.006 |
| Women | Coef. /SE | Coef./SE | Coef/SE | Coef/SE |
| Post Enrollment | 0.151 *** | 0.177 *** | 0.135 *** | 0.109 *** |
| | 0.018 | 0.023 | 0.022 | 0.006 |
| 4 quarters prior | 0.026 *** | 0.075 *** | 0.041 *** | 0.031 *** |
| | 0.007 | 0.016 | 0.009 | 0.003 |
| 3 quarters prior | 0.019 *** | 0.064 *** | 0.031 *** | 0.046 *** |
| | 0.007 | 0.017 | 0.010 | 0.003 |
| 2 quarters prior | 0.020 *** | 0.058 *** | 0.052 *** | 0.021 *** |
| | 0.008 | 0.018 | 0.010 | 0.004 |
| 1 quarter prior | -0.027 *** | 0.024 | 0.022 ** | 0.001 |
| | 0.008 | 0.018 | 0.011 | 0.004 |
| Entry Quarter | 0.082 *** | 0.116 *** | 0.090 *** | 0.070 *** |
| | 0.013 | 0.021 | 0.016 | 0.005 |
| Enrollment | -0.488 *** | -0.305 *** | -0.384 *** | -0.187 *** |
| | 0.017 | 0.016 | 0.018 | 0.005 |
| Observations | 887,934 | 284,890 | 473,377 | 2,893,457 |
| Adj. R-squared | 0.015 | 0.011 | 0.012 | 0.005 |

Note: Standard errors are based on a bootstrap with 1000 replications.

**Table B3 – Difference between For-Profit and Community College Coefficients, by Credential Type and Gender, Estimated Separately and Pooled across School Type**

| | Men | | | | Women | | | |
|---|---|---|---|---|---|---|---|---|
| | Certificate | | Associate's | | Certificate | | Associate's | |
| | Separate | Pooled | Separate | Pooled | Separate | Pooled | Separate | Pooled |
| 4 quarters prior to entry | -0.008 | -0.045 | 0.002 | -0.032 | -0.049 | -0.048 | 0.001 | -0.024 |
| 3 quarters prior to entry | -0.014 | -0.046 | 0.005 | -0.023 | -0.045 | -0.050 | 0.000 | -0.046 |
| 2 quarters prior to entry | -0.020 | -0.066 | 0.026 | -0.012 | -0.038 | -0.040 | 0.000 | -0.008 |
| 1 quarter prior to entry | -0.038 | -0.076 | 0.041 | 0.001 | -0.051 | -0.062 | 0.000 | -0.019 |
| Quarter of entry | -0.096 | -0.124 | -0.077 | -0.108 | -0.104 | -0.130 | 0.001 | -0.126 |
| 1 quarter after entry | -0.280 | -0.349 | -0.180 | -0.223 | -0.211 | -0.270 | 0.003 | -0.183 |
| 2 quarters after entry | -0.134 | -0.217 | -0.108 | -0.161 | -0.150 | -0.196 | 0.002 | -0.130 |
| 3 quarters after entry | -0.098 | -0.168 | -0.081 | -0.134 | -0.134 | -0.185 | 0.001 | -0.132 |
| 4 quarters after entry | -0.090 | -0.162 | -0.089 | -0.155 | -0.126 | -0.160 | -0.001 | -0.150 |
| 5 quarters after entry | -0.076 | -0.139 | -0.113 | -0.171 | -0.075 | -0.109 | -0.002 | -0.174 |
| 6 quarters after entry | -0.097 | -0.166 | -0.069 | -0.140 | -0.038 | -0.077 | -0.003 | -0.155 |
| 7 quarters after entry | -0.050 | -0.118 | -0.073 | -0.137 | 0.008 | -0.053 | -0.003 | -0.094 |
| 8 quarters after entry | -0.086 | -0.159 | -0.018 | -0.101 | -0.020 | -0.064 | -0.003 | -0.051 |
| 9 quarters after entry | -0.072 | -0.146 | -0.024 | -0.099 | -0.023 | -0.075 | -0.006 | -0.042 |
| 10 quarters after entry | -0.069 | -0.146 | 0.006 | -0.081 | -0.029 | -0.071 | -0.008 | -0.004 |
| 11 quarters after entry | -0.021 | -0.097 | -0.010 | -0.094 | -0.009 | -0.078 | -0.009 | -0.028 |
| 12 quarters after entry | -0.024 | -0.101 | 0.030 | -0.075 | -0.024 | -0.078 | -0.010 | -0.017 |
| 13 quarters after entry | -0.006 | -0.085 | 0.017 | -0.071 | -0.019 | -0.085 | -0.011 | -0.012 |
| 14 quarters after entry | -0.030 | -0.106 | 0.046 | -0.064 | -0.053 | -0.112 | -0.014 | -0.027 |
| 15 quarters after entry | -0.006 | -0.088 | 0.032 | -0.059 | -0.010 | -0.106 | -0.011 | -0.033 |
| 16 quarters after entry | -0.044 | -0.123 | 0.064 | -0.063 | -0.014 | -0.094 | -0.002 | -0.033 |
| 17 quarters after entry | 0.007 | -0.080 | 0.033 | -0.063 | -0.009 | -0.106 | -0.008 | -0.031 |
| 18 quarters after entry | -0.013 | -0.094 | 0.095 | -0.031 | -0.048 | -0.122 | -0.016 | -0.037 |
| 19 quarters after entry | 0.016 | -0.073 | 0.083 | -0.021 | 0.021 | -0.100 | -0.009 | -0.048 |
| 20 quarters after entry | 0.001 | -0.086 | 0.130 | -0.011 | -0.038 | -0.140 | -0.012 | -0.045 |
| 21 quarters after entry | 0.025 | -0.070 | 0.078 | -0.034 | -0.034 | -0.147 | -0.015 | -0.055 |
| 22 quarters after entry | 0.004 | -0.083 | 0.112 | -0.030 | -0.058 | -0.145 | -0.031 | -0.036 |
| 23 quarters after entry | 0.064 | -0.034 | 0.102 | -0.018 | -0.021 | -0.157 | -0.028 | -0.024 |
| 24 quarters after entry | 0.021 | -0.068 | 0.133 | -0.023 | -0.014 | -0.131 | -0.019 | -0.042 |
| 25 quarters after entry | 0.058 | -0.038 | 0.098 | -0.029 | 0.016 | -0.110 | -0.022 | -0.030 |
| Enrolled | 0.049 | -0.230 | 0.041 | -0.146 | -0.044 | -0.282 | -0.004 | -0.166 |

Note: Pooled estimates combine students from for-profit schools and community colleges within gender-credential groups.

**For Online Publication – Appendix C: Matching Algorithm and Results**

We undertake analyses by gender-credential group, using a logit to estimate the probability that a student enrolls in a for-profit school rather than a community college. The model includes controls for age,[41] dummy variables indicating prior education (less than high school, GED, high school graduate), dummy variables for race (white, black, missing/other), a series of dummy variables indicating the location of the school (13 regions), dummy variables indicating the field of study (academic/other, business, computers, health, trades, transportation, vocational) as well as dummy variables controlling for the year-quarter of entry into school. We also interact all the variables with race and education except the year-quarter of entry variables. We do not include prior earnings in the matching measures given the potential bias that matching on prior values of the dependent variable may produce in difference-in-difference models (Daw and Hatfield 2018).

We impose three sample restrictions in order to make the two groups of students as similar as possible. First, we omit any student whose characteristics or field of study are unique to either for-profit schools or community college, as such characteristics/fields perfectly predict school type. Once we estimate the logit, we impose a common support condition by dropping for-profit students whose estimated probability of enrollment in a for-profit school lies above the maximum estimated probability among community college students and dropping community college students whose estimated probability lies below the smallest estimated probability among for-profit students. Finally, we drop cases with probability ranges where one of the school types has very low density.[42] [43]

[41] We control for age at time of entry into school by including both age and age squared, along with a series of dummy variables for: less than 20 years; 20 years but less than 25; 25 years but less than 30; 30 years but less than 40; and 40 years and older.

[42] Where the density of treated cases is low, we might have retained these cases because matches for such cases are likely to be available. The advantage of our approach is that it permits us to use the same propensity scores for both treatments, and to avoid complications in the inverse probability weighting process. In fact, fewer than 1 percent of treated cases were omitted by this rule.

[43] The procedure described here was modified in the case of women seeking certificates because the balancing test indicated that matches were poor. For this group, after eliminating the cases as described above, we re-estimated the logit model, and

Our preferred matching technique is inverse probability weighting. We use estimated propensity scores to calculate probability weights for the community college sample that reproduces the observed characteristics and field of study for for-profit students. The returns for enrollment are then estimated separately for for-profit students and the weighted sample of community college students using our fixed-effects model. The latter is our best estimate of what the return would be for community college students if they had the same characteristics and fields of study as for-profit students, and it therefore provides a comparison with the for-profit school taken as the treatment. We also use the propensity scores to construct weights for the for-profit student sample so it reproduces the characteristics and field of the community college sample, producing estimates that take the community college as the treatment.[44] The proportion of treated cases retained in the matched samples are presented in Table C1.

The success of any matching technique depends on details of specification. In order to determine whether the matching methods were successful, we undertook balancing tests on all the independent variables used in the matching process, testing whether differences in variable means were reduced by the matching. This involved eight comparisons between treatment and matched comparison samples (four gender-credential combinations, by treatment defined by type of school). We calculated the standardized difference before and after the inverse probability weighting. Prior to such weighting, the average absolute value of the standardized difference of the 60-65 variables was between 0.19 and 0.21. When weights were used to produce matched samples, the average difference was between 0.02 and 0.09. Of significance, the maximum standardized differences are substantially greater in the unmatched comparison, in each case. Although even with matching there are differences in the means of these measures, it is clear that the matching is successful at producing much more similar samples.

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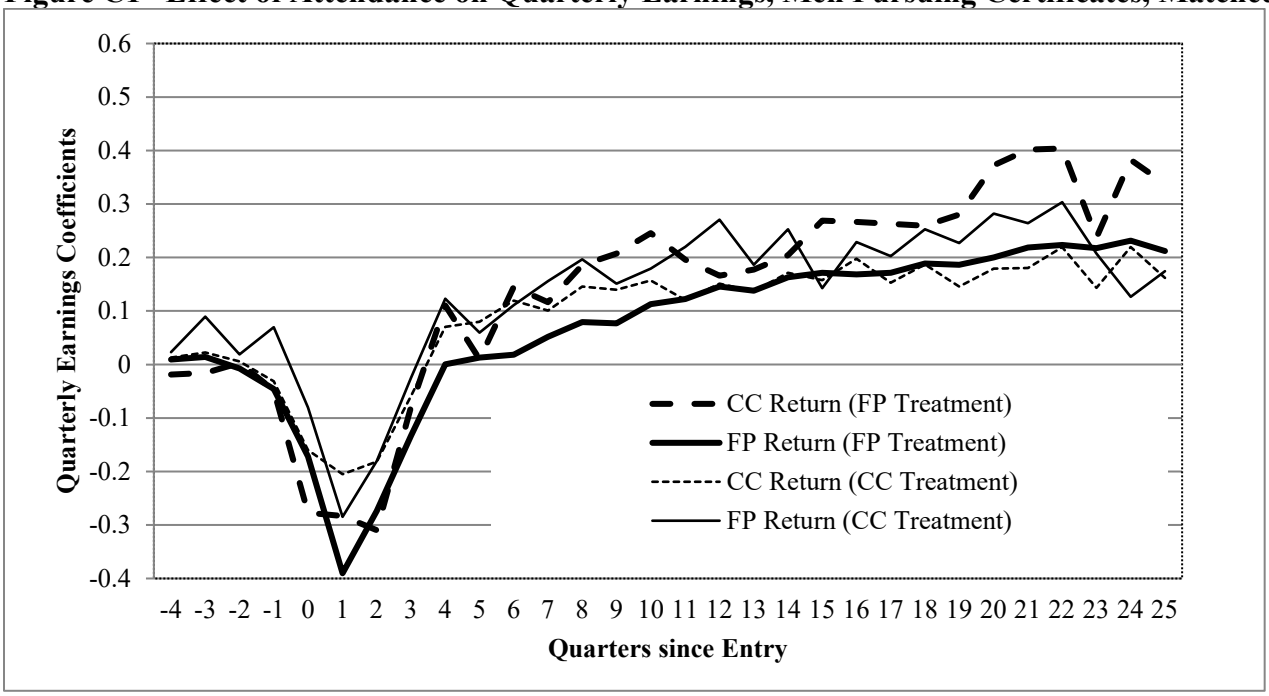
again eliminated cases off the common support or with very low density before using the propensity scores to calculate probability weights.

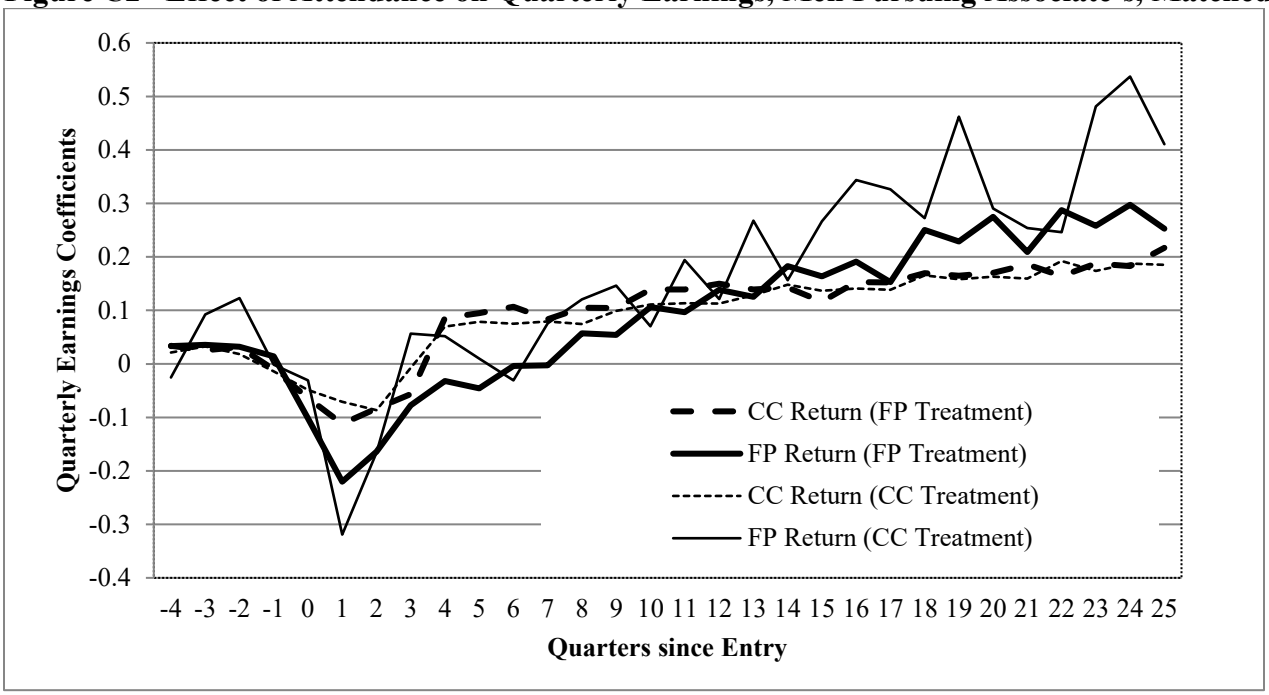
[44] Each retained case in the comparison sample is weighted by $p(X)/(1-p(X))$, where $p(X)$ is the estimated probability (as a function of case characteristics $X$) that the case in question would be a treated, based on the logit regression.

**Figure C1 –Effect of Attendance on Quarterly Earnings, Men Pursuing Certificates, Matched Samples**



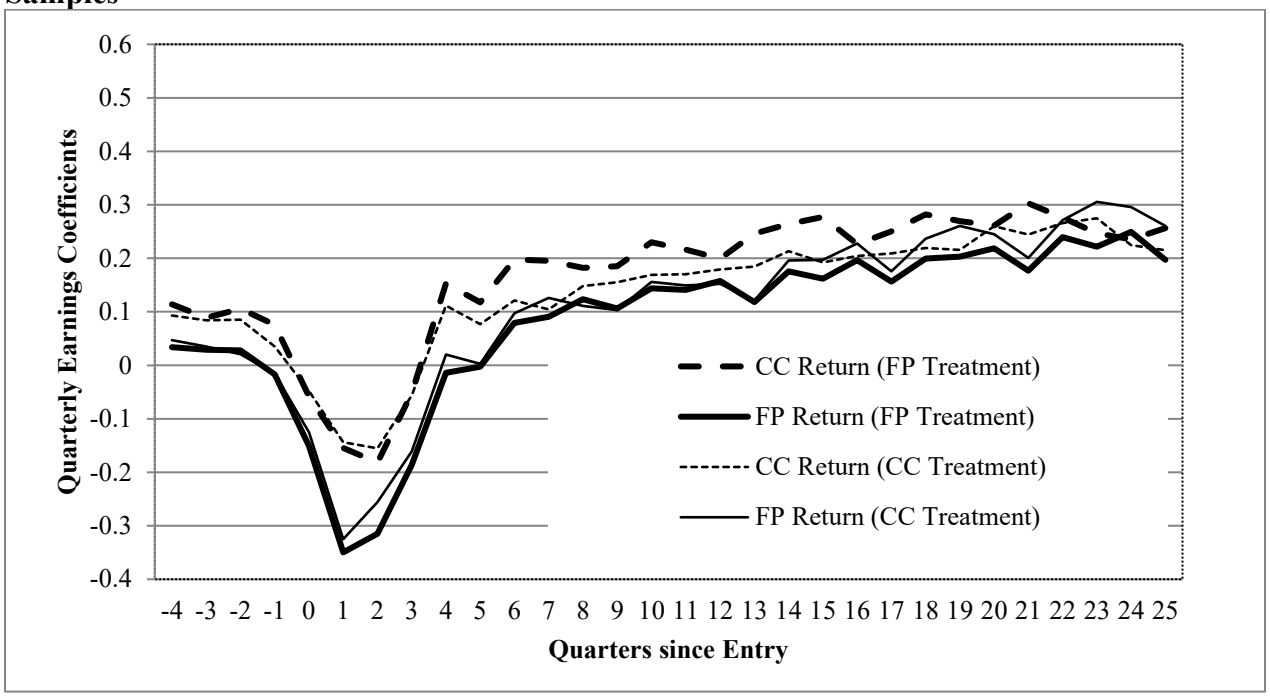

Notes: Each data point is the estimated log quarterly earnings gain based on Equation 3 for the matched sample. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. FP=for-profit students; CC=community college students. See the main text and Appendix C for further details.

**Figure C2 –Effect of Attendance on Quarterly Earnings, Men Pursuing Associate's, Matched Samples**



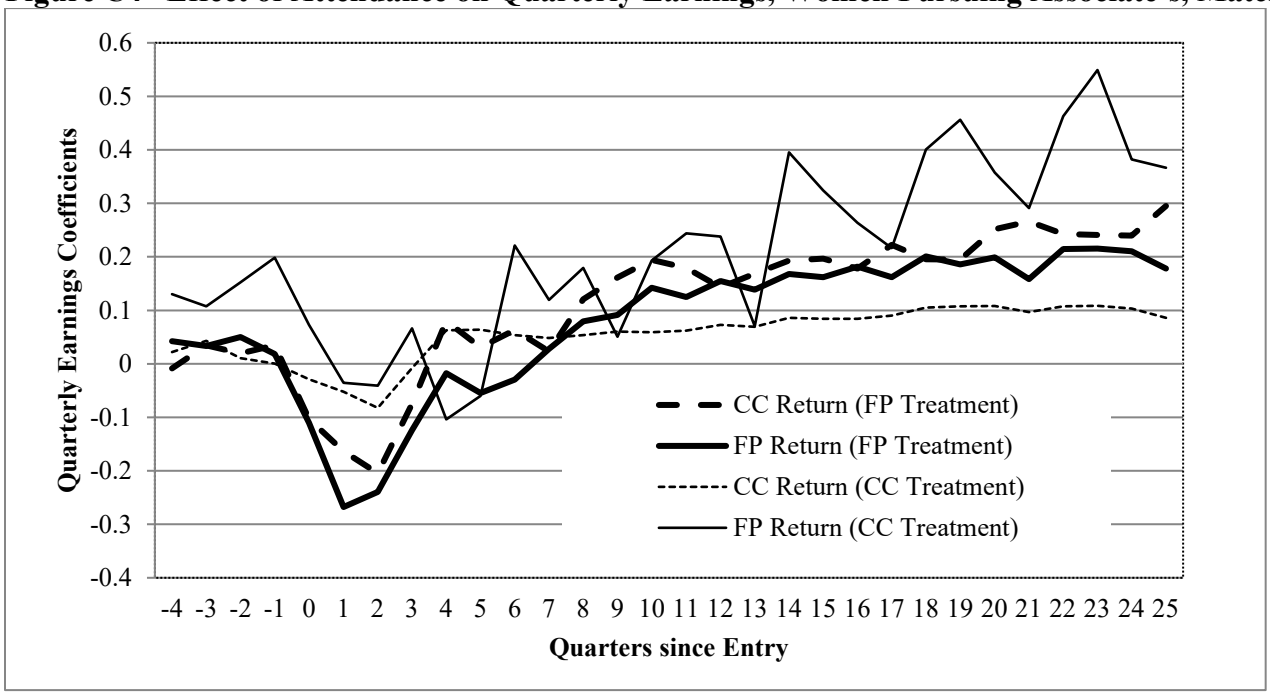

Notes: Each data point is the estimated log quarterly earnings gain based on Equation 3 for the matched sample. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. FP=for-profit students; CC=community college students. See the main text and Appendix C for further details.

**Figure C3 –Effect of Attendance on Quarterly Earnings, Women Pursuing Certificates, Matched Samples**




Notes: Each data point is the estimated log quarterly earnings gain based on Equation 3 for the matched sample. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. FP=for-profit students; CC=community college students. See the main text and Appendix C for further details.

**Figure C4 –Effect of Attendance on Quarterly Earnings, Women Pursuing Associate's, Matched Samples**




Notes: Each data point is the estimated log quarterly earnings gain based on Equation 3 for the matched sample. The return is adjusted for enrollment during the quarters after entry, based on the coefficient for the enrollment variable. FP=for-profit students; CC=community college students. See the main text and Appendix C for further details.

**Table C1 - Match Rate for Treated Cases by Gender, Credential, and School Type**

| Men | | Women | |
|---|---|---|---|
| Seeking Certificates | Seeking Associate's Degrees | Seeking Certificates | Seeking Associate's Degrees |
| Treatment: For-Profit Schools | | | |
| 49% | 88% | 72% | 85% |
| Treatment: Community Colleges | | | |
| 80% | 54% | 35% | 54% |

Note: Reported is percent of treatment cases that match with comparison cases.