The Benefits of Alternatives to Conventional College:
Labor-Market Returns to For-Profit Schooling

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Abstract:
This paper provides novel evidence on the labor-market returns to for-profit postsecondary school attendance. Specifically, we link administrative records on for-profit school attendance with quarterly earnings data for nearly 70,000 students. Because average age at school entry is 30 years of age, and because we have earnings data for five or more years prior to attendance, we estimate a person fixed-effects model to control for time-invariant differences across students. By five years after entry, quarterly earnings returns conditional on employment are around 32-35 percent for men and 25-26 percent for women. Returns are similar for associate's degree programs and certificate programs, but variation by field of study is much greater. Differences in return by gender are completely explained by differences in field of study. Returns net of tuition are generally positive for certificates.

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I. Introduction

The income distribution in the United States has widened over the last few decades. The relative earnings for high school graduates have declined substantially, and job opportunities for less-skilled workers are becoming more limited. U.S. states have drastically reduced funding for education, and public community colleges and universities are particularly hard hit (Phelan, 2014). For-profit college (also known as proprietary school) enrollment tripled between 2000 and 2012, compared to a 27 percent increase in public schools; in 2012, nearly 9 percent of all postsecondary students were attending for-profit schools (U.S. Department of Education, 2013). Enrollments have fallen somewhat in recent years (Gilpin and Stoddard, 2017).

During the Obama administration, the U.S. government, along with several states, increased oversight of the 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 U.S. Department of Education "gainful employment" rules, for-profit institutions had to satisfy requirements concerning their graduates' employment in order to be eligible for federal aid. Controversial when implemented, the rules have been labeled "overly burdensome" by the current administration and their full implementation delayed (U.S. Department of Education, 2017). In the midst of this controversy, evidence on whether and to what extent these schools improve labor-market outcomes is critical.

We provide novel evidence of the returns to for-profit schooling using administrative data for nearly 70,000 students who enrolled in one state between 2005 and 2009. We complement previous studies using national data by including a broader set of schools, specifically schools that do not receive U.S. federal aid. Our preferred model includes person fixed effects to control for time-invariant differences across individuals. Our model allows the earnings increment resulting from program participation to vary with time since enrollment. We demonstrate that the specification used in previous work on for-profit schools, where the return to schooling is taken to be the same in each post-schooling time period, can produce misleading results.

We find sizable earnings returns to for-profit school attendance as predicted from a base period 5 to 20 quarters before enrollment. By the fifth year after entry, quarterly earnings conditional on employment exceed earnings in the absence of schooling by 32-35 percent for men and 25-26 percent for women. We cannot reject the hypothesis that the returns for associate's degree programs and certificate programs are equal. For field of study, the highest returns are in computers, trades, and the "other" category, whereas returns for health, a field where women are much more likely than men to study, are considerably lower. We also provide new evidence that differences in overall educational returns by gender are completely explained by differences in field of study. Finally, although tuition costs are substantial, we show that the cumulative benefits of attendance net of tuition are generally positive for students seeking certificates. For the average student seeking an associate's degree, returns are less clear: The cumulative present value is negative within the seven-year window of our study but would be expected to turn positive about 10 years after initial enrollment.

II. Relation to Previous Literature

Research on the labor-market returns to for-profit schools fall in to 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);[1] (2) studies using administrative data (Liu and Belfield, 2013;

[1] 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.

Cellini and Turner, 2018) and (3) audit studies (Darolia et al., 2015; and Deming et al., 2016).[2] Cellini and Koedel (2017) 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.

Perhaps the most widely cited recent paper in this area, published after the above reviews, is by Cellini and Turner (2018). They use administrative data from the U.S. Department of Education to study labor-market returns to 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 in for-profit certificate programs with students in public community colleges and finds that earnings are lower for the former group. However, they are not able to control for race, which could induce bias given that blacks are overrepresented in for-profit schools and generally obtain lower earnings.

We provide three contributions to this literature. First, we provide a more flexible model specification that allows the returns to for-profit schools to vary over time. Although several recent papers estimate models that allow the return to community college to vary by time since enrollment, existing studies of for-profit schools usually fit models that assume the return is constant in each time period. We show that these latter models not only miss important variation over time, but that they may produce results that are seriously misleading in fixed effects models.

Second, we complement the work using national data on a subset of schools with a much broader set of for-profit schools in one state. Survey datasets have the advantage of covering the entire country, but they have relatively small numbers of respondents attending for-profit

[2] Armona, Chakrabarti, and Lovenheim (2018) 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.

schools.[3] The national data used by Cellini and Turner (2018) include only schools eligible for Title IV federal assistance, and they have data only on students who actually receive federal aid. Many for-profit schools offer only certificates and do not participate in federal government programs. Any comprehensive look at for-profit institutions should include schools and students that do not receive federal assistance as well as those that do.

Our final contribution is to explain why men and women have differences in returns to school attendance; previous work has merely reported returns by gender rather than attempting to explain differences.

III. Data

Our analysis focuses on students who entered for-profit schools in Missouri from January 2005 to December 2009. Each for-profit school with a physical presence in the state must provide student-level data as part of Missouri's Proprietary School Certification Program.[4] 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.[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. The data are not limited to schools that receive Title IV funding from the U.S. government. Although we know of no comprehensive listing that would allow us to identify

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[3] 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.

[4] See http://dhe.mo.gov/psc/. Although the program formally requires certification of both nonprofit and for-profit postsecondary education programs, a variety of provisions exempt most non-profit postsecondary institutions. We were able to identify the for-profit or not-for-profit status of all schools that accepted students in 2010. This set of schools accounted for 81 percent of individuals analyzed in our sample. In this subgroup, 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.

[5] We found no evidence of any on-line only for-profit colleges in the Integrated Postsecondary Education Data System (IPEDS). Because non-Title IV schools are not required to fill out the IPEDS, few do so.

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.

Our analysis focuses on entries of students who had not participated in a for-profit school program in the state in the 12 months prior to the observed program entry.[6] The sample is limited to students who enroll in certificate or associate's degree programs because the vast majority of students in for-profit schools enroll in these two program types. We exclude the small number of students who are coded as seeking a bachelor's or graduate degree, along with a very small set of students who declare some other degree or indicate that they are not seeking a certificate or degree. We also exclude students who indicate at the time of enrollment that they are not permanent residents of the state or the neighboring state for which we have administrative earnings data.

For each student attending for-profit schools during this time period, the data contain the specific school attended, the Classification of Instructional Programs (CIP) code with the field of study, the entry and exit date for each enrollment spell, and—for award recipients—the type of certificate or associate's degree received.

These data are matched with administrative data on earnings from the Missouri and Kansas Unemployment Insurance (UI) programs, which provide information on quarterly

[6] We have this sample restriction as an attempt to identify returns to current enrollment rather than previous enrollment spells. However, because our data include only those entering programs in January 2004 or subsequent months, our analysis – particularly for those entering in the early years of our analysis window (which begins in January 2005) – will include some individuals who were attending for-profit schools in the year prior to entry but who entered before January 2004. This inclusion is unlikely to affect our results, as the results are nearly identical (available from authors upon request) if we include entries of students who had participated in a for-profit school program in the state in the 12 months prior to the observed program entry.

earnings for the overwhelming majority of workers who live in these states.[7] The data do not include earnings from employment in other states, or jobs not covered by UI reporting requirements, such as informal or federal employment. Notwithstanding these omissions, it has been suggested that 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).

We have quarterly earnings information from the first quarter of 1999 through the second quarter of 2015. Thus, we have data for at least five years prior to for-profit school attendance and a minimum of five-and-a-half years (22 quarters) after initial enrollment in for-profit schooling. The resulting data set is a panel of student entries and time periods. We exclude observations 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 30 quarters after program entry. We also exclude all observations for individuals where age, Social Security Number, or program exit date are missing. The number of individuals omitted for these latter reasons is very small.

Although our data pertain only to those attending for-profit schools 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 are no more than 10 percent below the U.S. average. The proportion of the population that is African-American is slightly below the national average. The proportion Hispanic is substantially below the U.S. average but similar to that of most states. Because the

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[7] One of the two largest population centers in Missouri, the Kansas City metropolitan area, is on the border of Kansas. Although the other center, 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, approximately 16 percent of private sector jobs were in Illinois in 2012 (www.bls.gov/news.release/cewqtr.toc.htm), and we expect most of these jobs to be taken by Illinois residents.

state we study is representative of the nation in many respects, the results provide estimates that are plausible for many parts of the country.

IV. Methods

To estimate labor-market returns, we compare the post-schooling earnings of an individual with the pre-schooling earnings of the same individual. 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.

This fixed-effects model is a valid tool for estimating returns to schooling for individuals with pre-schooling earnings information. Economists regularly use fixed-effects models to estimate labor-market returns for nontraditional students.[8] Cellini and Turner (2018) and Cellini and Chaudhary (2014) use a student fixed-effects model for measuring the labor-market returns to for-profit schools. Several papers also use student fixed-effects models to estimate labor-market returns to community colleges (Belfield and Bailey, 2017). Using pre-schooling time periods as controls is appropriate in our data because over 80 percent of students are age 20 or above when they initially enroll, and average age is around 30.

Following Cellini and Chaudhary (2014), we focus on quarters with positive earnings. The fixed-effects model fits the following multivariate regression:

$$(1) \quad LNEARN_{it} = \alpha \cdot ENROLL_{it} + \theta \cdot ENROLL_{it} \cdot ASSOCIATE_i + \beta \cdot FORPROFIT_{it}$$
$$+\gamma \cdot FORPROFIT_{it} \cdot ASSOCIATE_i + \delta \cdot AGE_{it} + \eta_i + \tau_t + \varepsilon_{it}.$$

In this equation, i denotes a person and t denotes a quarter. *ENROLL* is a categorical variable equal to one for quarters in which the individual is enrolled in school for the entire quarter and a

[8] The main limitation of student fixed effect models pointed out by Dynarski, Jacob, and Kreisman (2016) is the underlying assumption that the pre-enrollment earnings trends between completers and dropouts are similar. This concern does not apply here because we do not compare completers and dropouts.

value of one-half for the first quarter and last quarter of school enrollment. Because the school entry and exit dates are unlikely to coincide perfectly with the calendar quarter, we assume that individuals spend only a fraction of those quarters enrolled in school.

The input of interest is for-profit school attendance. The vector *FORPROFIT* contains a set of dichotomous variables measuring time relative to enrollment in for-profit schooling. Hence, we include variables for each quarter starting from the fourth quarter before enrollment, i.e., we include a variable for the fourth quarter before enrolling, a variable for the third quarter before enrolling, extending through the thirtieth quarter after enrolling. 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 entering for-profit schooling, taking account of age and calendar quarter effects.

We allow for different returns for certificate and associate's degree programs by including a set of interaction terms between the dichotomous variables in $FORPROFIT$ and an individual-level dichotomous variable for students enrolled in associate's degree programs, $ASSOCIATE$, as well as an interaction term between the categorical enrollment variable *ENROLL* and $ASSOCIATE$. However, our ability to distinguish differences in returns between associate's degree and certificate programs is limited because over two thirds of our students pursue certificates.

LNEARN is the log of total reported UI earnings across all jobs for the quarter. Quarters with no reported UI earnings in a quarter are excluded. *AGE* is the individual's age in years, represented by a cubic. The parameter η is a set of person fixed effects, capturing all person-specific components that are constant over time, such as race/ethnicity or innate ability. The model also contains a set of 54 dichotomous variables to control for each calendar quarter (τ). The last component (ε) is the error term. As mentioned previously, we have earnings data from the first quarter of 1999 through the second quarter of 2015. Because we exclude observations more than 24 quarters before program entry and more than 30 quarters after program entry, we have up to 55 quarters of earnings observations per person. We estimate separate regressions by gender.

The quarterly variables for the period after initial enrollment provide a flexible way to capture the returns to attendance, similar to recent work on the returns to community colleges (Jaggars and Xu, 2016; Bahr, 2016; Minaya and Scott-Clayton, 2017).[9] Unlike the preferred estimator in previous work on for-profits, we do not constrain the earnings to have any specific parametric relationship with the time since enrollment. Estimates of the impact of attendance are identified relative to the implicit counterfactual defined by the dummies for calendar quarter and the cubic function for age.

Because the sample includes only individuals who attend for-profit schools, 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

[9] Furthermore, the seminal papers on returns to community college, Jacobson, LaLonde, and Sullivan (2005a, 2005b), allowed the overall effect of community attendance to vary with the number of quarters since enrollment through the inclusion of what they call short-run deviations.

at different ages and in different periods. Having data on non-students is not necessarily essential for identification, as Stevens, Kurlaender, and Grosz (2018) and Jepsen, Troske, and Coomes (2012) find similar returns for community college awards in models that exclude dropouts to their preferred model that includes them.

We look at attendance rather than completion in order to avoid endogeneity concerns associated with non-random completion, as noted in Cellini and Chaudhary (2014). Another, more practical, reason for the focus on attendance rather than completion is that, in those cases where the degree completed is not specified, we cannot always determine whether the individual left the program without a degree or the information is missing for some other reason.

Another strength of the data is that they permit us to classify for-profit school enrollment spells by their area of study—for example computers or traditional trades such as construction—to examine differences in labor-market outcomes. In addition to estimating the model for all men and women, we estimate separate models of attendance by field of study. As men and women have very different distributions across fields, we examine the extent to which the differential overall returns by gender are due to differences in the distribution across fields and differences in the return within field by gender.

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

$$\begin{aligned} (2) \quad EMP_{it} = {} & \alpha \cdot ENROLL_{it} + \theta \cdot ENROLL_{it} \cdot ASSOCIATE_i + \beta \cdot FORPROFIT_{it} \\ & + \gamma \cdot FORPROFIT_{it} \cdot ASSOCIATE_i + \delta \cdot AGE_{it} + \eta_i + \tau_t + \varepsilon_{it} \end{aligned}$$

Employment (*EMP*) is a dichotomous variable equal to one for individuals with observed earnings. We estimate the model as a linear probability model.

There are several reasons that individuals may not be employed in a quarter. Given that our earnings measures capture employment only in the state where enrollment occurred and one adjacent state, those who leave these states after completing schooling may have earnings elsewhere that we do not capture. Insofar as for-profit school attendance is associated with departure from the state, our estimates that predict employment will misrepresent returns from enrollment. In contrast, individuals may have no observed earnings because they are unemployed or have left the labor market. Insofar as these latter employment differences reflect the impact of for-profit school attendance, we wish to take them into account.

As a way to incorporate the possibility that individuals may have left these states, 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 the quarter 30 (or the last quarter for which earnings data are available), 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. This approach will fail to account for employment of those who left the state after completing enrollment and were employed elsewhere before resuming employment in the state, because the intervening years would be coded as including no employment. Conversely, this approach omits some individuals who are unemployed for more than 10 quarters or withdraw from the labor market because of poor opportunities. In each case, bias would result if these activities were associated with for-profit school attendance. Given this limitation in the employment analysis, we prefer to focus on earnings conditional on employment.

V. Results

Descriptive Statistics

Table 1 contains the descriptive statistics for the analysis sample of nearly 70,000 entries for students into the state's for-profit schools between January 2005 and December 2009.[10] We provide statistics separately by gender and by degree program. The percentage white varies from 56.5 percent for women in certificate programs to 66.6 percent for men in certificate programs. The average age at entry is 27 years for associate's degree programs and 29 to 33 years for certificate programs. Approximately 75 percent have a high school degree; 15-20 percent have a GED; around 2 percent in associate's programs and 7 percent in certificate programs have less than a high school degree; and at most 2 percent have missing high school / GED status. Approximately 70 percent of students live in the state's major metropolitan areas, although only around 60 percent of the state's population lives in those areas (authors' calculations from 2010 Census). For men, over 76 percent pursue a certificate rather than an associate's degree, compared to 69 percent for women. Completion rates are between 60 and 70 percent for certificates, compared to 50 percent or less for associate's degree programs. Focusing on those seeking certificates, over two-thirds of women are in health as compared to less than 20 percent for men. For men, transport and trades are the most popular fields of study at roughly one-third each for certificates; the "other" category (including services and academic fields) is the most popular field for associate's degree programs at 40 percent.

Table 2 provides mean earnings and levels of employment for selected quarters prior to and subsequent to enrollment. Figures 1a and 1b (for men and women, respectively) present the

[10] Recall, the analysis sample is limited to entries into certificate or associate's degree programs, where the individual indicated permanent residency in the state or the neighboring state for which we have administrative earnings data, and where the individual had not been enrolled in a for-profit school in the state in the prior 12 months.

trends in average earnings by quarter relative to quarter of entry, where quarter 0 denotes the quarter of initial enrollment. Individuals with no reported earnings are coded as having zero earnings for the quarter, so the reported means are not conditional on employment. However, in order to exclude earnings for those who left the state, as noted above, we omit 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. As Figure 1a shows, men in certificate programs have noticeably higher earnings than men in associate's degree programs, although the gap narrows toward the end of the sample period. The difference can be traced to measured characteristics, most notably age.[11] Both groups experience an "Ashenfelter dip" in earnings around the time of school entry, as well as reduced earnings following the entry quarter, often called a "lock-in" effect, reflecting participation in school. Because earnings growth is higher in the post-entry period than the pre-schooling period, average earnings exceed their pre-schooling levels a few quarters after school entry. The highest average earnings, observed 30 quarters after enrollment, are approximately $7,100 per quarter for the certificate program and $6,300 for the associate's degree program.

For women, average earnings are much more similar for the two programs. Average earnings for those in certificate programs are slightly higher in the pre-schooling period. After a substantial earnings reduction around entry, participants in both programs experience large increases in average earnings during the first few post-entry quarters, but then the rate of growth is more modest in later periods. From 9 quarters after entry until 30 quarters after entry, average

[11] Certificate degree seekers are nearly six years older, are more likely to enter the program in more recent years, and are more likely to be high school graduates than those seeking associate's degrees. Such differences are fully controlled in our regression estimates, which include individual fixed effects.

earnings are slightly higher for associate's degree programs than for certificate programs. At the end of the sample period, average quarterly earnings are slightly under $5,000 for both programs.

For both men and women, these trends in average earnings strongly suggest positive impacts of participation. We now turn to regression results, which control for calendar quarter, age, and student fixed effects, for the estimates of the return to for-profit school attendance.

Effects on Earnings

Figures 2a and 2b contain the regression results for the model depicted in equation (1), estimated separately for men (Figure 2a) and women (Figure 2b). The dependent variable is the logarithm of quarterly earnings. The figures show the returns to attendance for individuals pursuing certificates (dashed line) and associate's degrees (solid line) for each quarter beginning four quarters before entry to 30 quarters after entry.[12] 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, based on the combined effect of the coefficients for the dummy variables in $FORPROFIT$ and the coefficient for $ENROLL$. However, we set the $ENROLL$ dummy variable equal to one for the average length of enrollment to determine the number of quarters of enrollment in each figure. 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 in the graph for quarters 0 to 2 in the graph, i.e., the first three quarters in which individuals are enrolled.

In equation (1), the coefficient for a given quarter of the $FORPROFIT$ variables is the return for attendance in certificate programs, whereas the return for attendance in associate's degree programs in a given quarter is the sum of that coefficient and the interaction term for the

[12] Standard errors (and coefficient estimates) are available in Appendix Table 1. For simplicity, figures exclude confidence intervals. We discuss important differences in significance levels in the text.

associate's degree program (the coefficient for one of the $ASSOCIATE \cdot FORPROFIT$ variables in equation (1)). For example, in quarter 10, the return for certificate programs is 0.121 log points for men, and the interaction term between quarter 10 and associate's degree program is 0.014. Thus, the return is 0.135 (= 0.121 + 0.014) for associate's degree programs, implying an increment of 14.5 percent.[13] Finally, note that the calendar quarter dummies included in equation (1) control for calendar quarter effects such as those due to statewide changes in wages due to inflation or variation in the health of the economy.

The figures show a broadly similar pattern for men and women: slightly lower earnings in the last four quarters before entry, a large decline around entry (somewhat less for men in associate's degree programs), followed by consistent gains in earnings for both program types. Earnings gains are higher and have a steeper earnings profile for men compared to women. By the fifth year after entry,[14] the average earnings gain (relative to earnings more than one year prior to school entry) is 32 percent for those in certificate programs and 35 percent for those in associate's degree programs. However, as we show later, this sizeable increase in percentage terms translates into a relatively modest dollar return relative to forgone earnings and tuition over the seven years of our study.

For women, certificate programs are associated with higher earnings than associate's programs for most of the first eight quarters after entry, but in quarters 10 through 30, returns are essentially the same. In the fifth year after entry, average quarterly returns, relative to earnings more than one year before entry, are between 25 and 26 percent for both certificate and associate's degree programs.

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[13] Because the average enrollment period is less than 10 quarters, the coefficients for the enrollment variables are not included in the estimated return in quarter 10.

[14] We choose five years or quarters 17-20 after entry because that time period corresponds roughly with the average post-schooling time period in Cellini and Chaudhary (2014), thus facilitating comparisons of our results with theirs.

Because our preferred specification makes no distinction between individuals who complete an award and individuals who do not, we also estimate equation (1) where we limit the sample to individuals who have completed a certificate or an associate's degree. The decision to complete may be endogenous to labor-market outcomes, so we do not interpret the results as causal. For men, the results for completers in Appendix Figure A1 are similar to the results in Figure 2a for the full sample, although returns in the last two years of our window are 2-4 percentage points higher for completers. For women, completers (Appendix Figure A2) have higher returns after two years than the full sample (Figure 2b).

Effects on Employment

Figure 3a and 3b provide estimates of the effects of for-profit school enrollment on employment for males and females. 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. Employment during the period of enrollment for those seeking certificates declines by about 10 percentage points for both males and females during the second quarter of enrollment. Following enrollment, the estimate is positive, peaking around the fifth year after participants for both men and women in the range of 5-8 percentage points. By the end of our period, the increment is under 5 percentage points for women and close to zero for men.

For women seeking an associate's degree, the employment pattern is similar to that for those seeking certificates. For men seeking an associate's degree, effects on employment are somewhat anomalous during enrollment, indicating that enrollment is not associated with a substantial decline in employment. After exiting the program, effects on employment for men in associate's programs are generally higher than for those seeking certificates, peaking at over 8 percentage points.[15]

Sensitivity Analysis

In this section, we check the sensitivity of our preferred specification to alternative model and data specifications. The full set of results, which we summarize below, are available from the authors upon request. Both the earnings and employment results are quite similar if we estimate age as a series of dummy variables rather than as cubic. The results are also robust to excluding the relatively small share – under 20 percent – of students who attend part-time. Excluding the top one percent and the bottom one percent of earnings conditional on employment, a robustness test included in Cellini and Chaudhary (2014), does not affect the pattern of results. Because schools have substantial variation in whether they report exit dates, we estimate our models excluding participants attending schools with more than 20 percent missing exit dates during the year they entered, with no change in substantive results. When we include entries of students who had participated in a for-profit school program in the state in the 12 months prior to the observed program entry, we find little difference in returns, with the exception that the larger sample has smaller employment returns for men pursuing associate's degrees. Analyses of employment results are broadly similar when we change our exclusion standard for the length of the continuous string of quarters with no earnings subsequent to enrollment from 10 quarters to 15 quarters.[16] However, the employment results are sensitive to

[15] We also estimated effects on employment using a fixed-effects logit model. Patterns of estimates were essentially identical to those obtained with the linear probability model, although, when calculated at the sample mean, the largest effects in absolute value were as much as 3 percentage points smaller.

[16] Estimates of the effects of participation on employment up through three years following initial entry are not sensitive to this choice. Estimates for 4-7 years following initial entry are reduced by up to 3 percentage points when only strings of quarters of length 15 are omitted.

whether or not we have an exclusion standard, as the returns to employment are much lower and are in many cases negative if we do not exclude any strings of zero earnings.

For earnings outcomes, the results are also robust to the following two changes: (1) restricting the sample to individuals who have positive earnings in at least one pre-enrollment quarter in our data set; and (2) excluding individuals who attended for less than one month. The results for employment vary slightly among these specifications compared with our preferred specification. Appendix Figure A3 shows the employment effects for men pursuing certificates, and Appendix Figure A4 shows employment effects for women pursuing certificates, both restricted to those who had positive earnings prior to enrollment (dotted line). For both men and women, the estimated employment returns are lower if we limit the sample to individuals with some pre-schooling employment. This sample restriction excludes individuals with no labor-force experience prior to for-profit school entry, as well as individuals who have labor-force experience in states other than the two for which we have earnings data. Thus, it is not clear how to interpret this finding, other than to note the possibility that the employment effects for our preferred sample may be overstated if individuals with no pre-entry employment data in our two states are employed in other states prior to entry.

Appendix Figures A3 and A4 indicate that limiting the sample to students who attend for at least a month has different implications for employment effects for men and women (dashed line). When we exclude men who attend for less than a month (roughly a third of the sample), the employment returns are higher, suggesting that students with very short-term attendance derive fewer benefits than those who attend longer. For women, the results are very similar for the whole sample and for the subsample of people who attend for at least a month, in part because only 13 percent of the sample attend for less than a month.

Our preferred model estimates post-employment earnings relative to earnings at least one year prior to school enrollment, consistent with the dip observed in Figures 1a and 1b. If, instead, we estimate earnings gains relative to quarters at least two years prior to enrollment, we obtain return estimates that are somewhat smaller. For men, estimates are approximately 0.07 log points smaller, with a coefficient maximum at approximately 0.34. Estimates for women are only slightly smaller than our preferred ones in this specification, with an estimated maximum return of about 0.27 log points, about 0.02 less than those estimated above.

Our primary model, which takes the logarithm of earnings as the dependent variable, excludes any month with zero earnings. In contrast, in the analyses predicting employment, quarters with zero earnings are included, except that we omit strings of zero-earnings of length 10 or more extending to 30 quarters, or to the end of our data, if shorter. If, instead, we omit all individuals with such strings of zero earnings, this approach eliminates approximately 10 percent of observations in the earnings analysis. Results for our primary earnings model on this subsample of individuals are nearly identical to the results in Figures 2a and 2b and are available from the authors upon request. In the analysis predicting employment, this exclusion omits 16 percent of observations for men and 13 percent for women. Results based on this sample predicting employment are consistent with the view that those individuals who did not leave the state were less successful. For men, the coefficients fitted on this limited sample imply no change in employment five years after enrollment for those seeking certificates, and 2 to 3 percentage points increase for those seeking associate's degrees, in contrast to over 5 percentage points in our full sample. For women, we find positive impacts on employment that are generally less than 5 percentage points, less than two-thirds of those in our main model.

We have also fitted the model that takes earnings (rather than log of earnings) as the dependent variable. These analyses include quarters with no earnings coded as zero, except that, as in our main model of effects on employment, we omit strings of quarters of length 10 or more with zero earnings through quarter 30 after date of entry. For men, these estimates imply an increment of about $1,800 per quarter in the fifth year after enrollment for both certificates and associate's degrees. Based on the mean earnings for these quarters, this amounts to an increment of about 0.35 log points and 0.45 log points for certificates and associate's degrees, respectively. Estimates for women imply dollar increments of $1,300-$1,400, or 0.38-0.39 log points. Since these estimates incorporate the effects on both employment and earnings (contingent on employment), we expect them to approximately equal the sum of employment and earnings effects as reported in our main results. In fact, these estimates exceed the sum by 0.02 to 0.09 log points.

We also performed an analysis that excludes schools that are not eligible for Title IV funding.[17] Although this selection of schools is similar to that of Cellini and Turner (2018), we are not able to limit the sample to students receiving federal aid, as they do. Only 54 percent of men and 74 percent of women attend aid-eligible schools. Appendix Figures A5 and A6 present results for men and women. For men during the fifth year after entry, the estimated earnings increment is 44 percent for certificates and 54 percent for associate's degrees, 12 and 19 percentage points above the estimates reported for the full sample. For women, the salary increment estimates are 30 percent and 32 percent, 5-6 percentage points, above our main

[17] Specifically, we assume that schools with Office of Postsecondary Education identification (i.e., OPEIDs) from the U.S. Department of Education are eligible for Title IV funding, and we exclude schools that do not have OPEID for this analysis, based on discussions with researchers who produce the College Scorecard (https://collegescorecard.ed.gov) for the U.S. Department of Education.

estimates reported above. In short, our estimates suggest that aid-eligible schools have larger returns than those that are not eligible.[18]

Our final sensitivity test compares returns for individuals first enrolling in the first half of the sample period, January 2005 to June 2007 (earlier cohort), and returns for enrollees in the second half, July 2007 to December 2009 (later cohort). Here we use a single equation, but fit interaction terms that allow us to identify separate effects for each period. For earnings, although the returns are often slightly higher for individuals in the later cohort, differences are not statistically significant. For employment, the returns one to three years after enrollment are higher for the later cohort, but for subsequent returns, differences are not statistically different between the two time periods. The basic pattern is the same in the two periods.

In summary, the earnings returns are not sensitive to several different samples, with the exception that estimated returns increase when the sample is limited to Title IV-eligible schools. The employment results depend on our sample exclusion, where we assume that individuals have left the state if they have no earnings data for 10+ quarters at the end of our sample period. Estimates based on earnings rather than log earnings imply larger effects, in part reflecting the combined effects on employment and earnings.

Models with a Single Post-Enrollment Effect

The sensitivity analyses reported above are based a model that allows for the effects of school enrollment to vary with time since completion. Although recent papers have argued in favor of such an approach (Jaggars and Xu, 2016; Bahr, 2016; Minaya and Scott-Clayton, 2017), other studies have used a single variable to identify all periods following completion of a degree,

[18] In our discussion below, we note that tuition at for-profit schools is quite high. Relative to schools that are not eligible for aid under Title IV, average tuition is approximately $8,400 higher at aid-eligible schools for men, and nearly $5,500 higher for women.

a specification that assumes enrollment affects earnings in all periods by the same increment (e.g., Cellini and Chaudhary, 2014; Cellini and Turner, 2018).

In this section, we test the implicit assumption that the post-enrollment dummy variable represents an average post-schooling return (Jaggars and Xu, 2016; Bahr, 2016); this assumption has not been tested.[19] We create a synthetic data set based on our sample of men who pursue a certificate. We show that an estimate of the effect of enrollment using a single post-enrollment dummy can be biased when the returns differ by year.

The bias is most clearly illustrated when benefits of schooling increase with time since enrollment. Specifically, we replace the actual data with synthetic or artificial data where the earnings increase by 0.01 log points for each quarter after enrollment. As an example, the earnings increment due to enrollment five quarters after enrollment is 0.05 log points. When we fit a model like that specified by Cellini and Chaudhary (2014), which attempts to capture the post-enrollment period with a single dummy, the estimated coefficient on the dummy is close to zero. Specifically, the model we fit corresponds to a simplified version of equation (1) above, but it replaces the dummies identifying time since enrollment with a single post-enrollment dummy. Figure 4 presents the true relationship with a solid line; the dashed line presents the effect implied by the estimates of this model using a single post-enrollment dummy.[20]

The reason for this discrepancy stems from the use of flexible specifications for calendar time and age in combination with an inflexible specification for the enrollment effect. The least squares procedure assures that estimated time and age effects attempt to fit earnings in periods both before and after enrollment. Because the relationship between earnings and enrollment

[19] Jaggars and Xu (2016) state, “a lack of controls for time-since-exit results in estimates of average returns across post-award time” (p. 449).
[20] Details on the synthetic data set and model estimation are in the Appendix.

varies over time, these controls are more successful than the single dummy in capturing earnings growth following enrollment; the post-enrollment dummy merely adjusts to aid the estimation. Stated in a different way, the single effect estimate is biased as a measure of post-enrollment return because the set of controls allows for much greater flexibility in fitting returns than does the measure capturing the effect of interest. Thus, a large portion of the post-enrollment return is captured by the age and time effects.

We experimented with a variety of synthetic effects, including effects corresponding to those estimated in our analysis, and whenever true effects vary with time since enrollment, estimates based on a single post-enrollment dummy suffer bias. Our own model, which fits an effect for each quarter following enrollment, correctly reproduces the synthetic effect structures we consider. We also correctly reproduce the synthetic effect structures when we fit a model that allows the effect of enrollment to change linearly in each quarter. This result is not surprising, since, as a practical matter, the change in the increment from one quarter to the next is not too far from being linear over the 30 quarters of data we have.

Finally, Appendix Table A2 contains results for two models using our data, not the synthetic data. In the first column, we estimate a model like that in Cellini and Chaudhary (2014) with a single post-enrollment dummy variable. Here, the coefficient is negative for males and positive but small (less than 0.025) for females, implying little if any return to enrollment. In the second column, we estimate a model like Minaya and Scott-Clayton (2017) where, in addition, we include a post-enrollment dummy variable interacted with quarters since enrollment. Here, the post-enrollment dummy variable is also small, but the linear time trend for post-enrollment is positive in all samples, and the overall effect of post-enrollment (i.e., the sum of the two effects) becomes positive and substantial after a few quarters. Given our findings with the synthetic data, we conclude that the results based on a model assuming time-invariant effects of enrollment are seriously misleading when fitted using these data.

Placebo Test

Our model identifies the impact of the program based on comparing earnings following enrollment with the expected pattern based on calendar quarter and age effects. Person fixed-effects estimates are all based on within-person patterns of earnings, and it is the patterns before and after enrollment that identify the effects. Because all individuals in the sample ultimately enroll, one may be concerned that, in practice, enrollment is merely capturing earnings increases that are properly attributable to natural earnings growth due to aging and economic growth.

In order to test this possibility, we developed a placebo test that uses pre-enrollment data for the individuals in our sample, where we randomly assign enrollment.[21] Details of this model and data are in the Appendix. Figure 5 presents estimates based on the placebo enrollment dates and on data for the same period based on actual enrollment. Estimates based on the placebo entry dates are small and none are statistically significant at the 10 percent level. In contrast, actual data provide estimates that are positive and statistically significant beginning in quarter 6. For quarters 10 through 14, estimates range from 0.11 to 0.17, with an average of 0.13. The results are slightly below the results in Table 2 of 0.12 to 0.21 – with an average of 0.17 – over the same period.[22]

This test shows that the estimated effect of enrollment is not a result of natural earnings growth that is observed prior to enrollment. Growth in earnings clearly is different following enrollment, and the return estimates are based on this differential. We cannot, of course, be sure

[21] We thank our colleague Cory Koedel for suggesting this placebo test.
[22] The time period here is shorter than that in our main analysis above, reflecting the need to limit consideration to pre-enrollment data. See the Appendix for details.

that this return is a causal effect of participation if the average participant in a for-profit school experienced correlated factors causing changes in patterns of earnings. However, for such factors to produce estimates of the magnitude we observed in our analysis, these would have to be strongly associated with both school enrollment and subsequent labor market success.

VII. Returns by Area of Study

Because our data set provides information on area of study for all students, we obtain reasonably precise estimates of returns by estimating separate regressions for different fields of study. Figures 6a (for men) and 6b (for women) contain the earnings estimates based on the model presented in equation (1) for those pursuing certificates. Estimates for each field are obtained by fitting the model with the sample limited to those in the specified field. The black line in each panel identifies the return to the specified area of study (e.g., under "business," the black line denotes the returns for business), whereas the grey lines show effects for each of the other subject areas, to allow comparison.

For men, if we look at the return for each area of study separately, we find that, with one exception, estimated effects on earnings for those in certificate programs are generally positive and statistically significant after two to three years following entry into the program. The exception is business, but this appears to reflect small sample size rather than lower estimated effects, as estimates are similar to those in transport and health, which are statistically significant. (Table 1 provides the distribution of men and women by area of study.)

Figure 6a makes clear that the six study areas can be divided neatly into two groups by average level of return. We see that men in business, transport, and health obtain earnings 15 to 20 percent above their predicted earnings in the absence of participation three years after entering the program, and they are earning about 25 percent more at five years. Although these are good returns, those with areas of study in trades, computers, and the other category (which includes areas such as technology and services) have earnings increments that are several percentage points higher at three years (over 20 percent), with an increment that grows to over 40 percent at five years. Although there are differences between the fields of study within these two groups, they are not statistically significant and are small compared the differences between the two groups.

For women (Figure 6b), returns are similar to those of men within area of study, with the exception of trades and transport. Both of these are areas with very few women. In contrast to the case for men, where estimates for those in trades were above others, for women estimates for trades are somewhat lower than estimates for most other fields. But the statistical power for women studying trades is very limited, so that both zero and the highest estimates for any field are within two standard errors of effect estimates for women studying trades. We observe returns for transport that appear higher than the returns in most other fields, in contrast to the case for men, but the difference between estimates in transport for men and women are not statistically significant.

For the areas of study with substantial numbers of women, the relative returns are very similar to those for men, i.e., returns for business and health are below those in computers or the miscellaneous category. In fact, if we compare men and women within these categories, there are no statistically significant differences between men and women. This similarity suggests that observed differences between men and women may be largely due to area of study. We now investigate this possibility in more detail.

Figure 7 provides returns for men and women in certificate programs averaged across all the fields of study. As expected, when returns for men are weighted by the distribution for men, the average is very similar to the effect estimate obtained in the basic model reported in Figure 2a. Similarly, women's weighted returns are very close to those reported in Figure 2b. The gap between the return for men and women grows after about quarter 10, and the difference is about 11 percentage points at 20 quarters and 20 percentage points at 30 quarters. However, when we weight the male returns by the female distribution (dashed line), we see that returns correspond closely to that for the female distribution. This indicates that observed differences between men and women in estimated returns are due to differences in the distributions of field of study. Almost all of the difference is due to the fact that 75 percent of women seeking certificates are in the health field, whereas the number is only 15 percent for men. Men are dramatically overrepresented in trades, a high return field, with 32 percent of certificate seekers in that area, compared to a mere 1.5 percent for women.[23]

We undertook comparable analyses by field for those in associate's degree programs. Because the sample size is appreciably smaller for both men and women, we do not present figures by field of study. Very few individuals in an associate's program list either trades or transport as a field, so our primary fields are business, computers, health and the other category. For both genders, computers and the other category have greater returns, corresponding with the results for certificates. For three of the four fields, business, computers, and the other category, the returns for men and women are very similar, and differences by gender are not statistically significant. The exception is health, where the returns for women are appreciably higher than those for men. For women, the earnings increment for those in health approaches 35 percent by

[23] An alternative is to weight the estimated returns for women by the distribution for men, but we do not believe these results are meaningful, since they rely on the very imprecise estimates of returns for women in trades and transport.

the end of our period, whereas for men, the increment is almost always less than 25 percent. In several quarters in the fifth and sixth years, the difference is over 15 percentage points.

Figure 8 presents estimates of earnings effects for men and women in associate's degree programs based on weighting field of study returns by the number studying in each field, producing estimates that correspond closely to estimates reported in Figure 2. About 63 percent of women in associate's programs are in the health field, as compared with about 20 percent for men. When we estimate what returns for men would be if they had the same distribution across fields as women, this implies moving men into fields with lower returns than the fields in which they are observed. Average returns are also smaller than the returns for women in these fields due to the smaller returns from studying health for men than for women. Figure 8 shows that such weighting reduces the average returns for men to appreciably below those for women. Hence, our prior conclusion that the men's higher returns are entirely due to the distribution of their fields of study still holds. As in the case of those in certificate programs, higher returns for men can be traced to their lower representation in health. Men are overrepresented in the computer category and the other category, relatively high-paying areas of study for both men and women.

In conclusion, the difference in returns between men and women can be traced to the difference in their fields of study. We have no evidence—with one exception—that returns for men and women differ within field. The one exception is that, among those in an associate's degree program, men studying in the health field have lower returns than women in the same field. The conclusion that there is no difference for other fields should be tempered by the recognition that, because women are very unlikely to pursue trades or transport, we have imprecise estimates of women's returns in those fields. Consistent with the stereotype, both for those seeking certificates and associate's degrees, women are much more likely to study in health fields than men, and our analysis shows that, because returns to health are lower than in traditional male fields, returns for women are lower.

VIII. Conclusion

This paper investigates the relationship between for-profit school attendance and quarterly earnings. We use an individual fixed-effects method to control for time-invariant differences between students. We find positive effects of attendance on earnings for students enrolled in certificate and associate's degree programs. Although men have higher returns than women, that difference can be entirely explained by field of study: the fields of study most men choose have higher returns than the fields most women choose.

How do our results compare to others in this literature? Cellini and Chaudhary (2014) find a weekly earnings increment conditional on employment of around 10 percent in the four years after leaving school for young students in the NLSY attending associate's degree programs at for-profit institutions. If we look at a similar post-schooling time period, the average returns in quarters 5 to 20 – the first 4 years after an average attendance of 5 quarters in our data (i.e., quarters 0 to 4) – are approximately 18 percent for men and 16 percent for women. Thus, our estimates of the earnings increment resulting from attending an associate's degree program are larger than theirs when looking at the returns over the same time period. However, for longer post-schooling periods, our analysis suggests that the returns to attendance grow, so that the effective benefits of attendance appear somewhat larger.

Jepsen, Troske, and Coomes (2014) find that students who complete associate's degree programs in Kentucky community colleges have higher quarterly earnings of 56 percent for women and 24 percent for men. Their time period of study is 4.5 to 6 years after entry, so we use our results for quarters 18-23 for comparison. We find smaller gains for women attending associate's degree programs, 27 percent, but we find larger gains for men attending associate's degree programs, 38 percent.[24] For comparison, Stevens, Kurlaender, and Grosz (2018) find average returns in excess of 40 percent for attending vocational associate's degree programs. With respect to employment outcomes, we find an increase of roughly five percent, smaller than the effect for completing an associate's degree of 12-19 percent in Jepsen, Troske, and Coomes (2014).

Deming et al. (2012) and Lang and Weinstein (2013) have preferred estimates that compare for-profit schooling to public schooling rather than reporting overall returns. Deming et al. (2012) report lower earnings for degree seeking students in for-profit schools, whereas Lang and Weinstein (2013) find no statistically significant differences, although power in the latter study is very limited.

For individuals pursuing certificates, Cellini and Turner (2018) find that for-profit students have lower returns than matched community college students. In contrast, Lang and Weinstein (2013) find no difference. Studies of public or nonprofit schools offering certificates provide a wide variety of estimates. In a study of Kentucky community colleges, Jepsen, Troske, and Coomes (2014) find modest unconditional earnings gains of 5-7 percent. For California, Stevens, Kurlaender, and Grosz (2018) find larger effects when they condition on employment, of 12 to 23 percent. If we use the time period of 5 to 20 quarters after enrolling (comparable to the period considered in those studies), we find an average conditional earnings increase of 16-18 percent.

With respect to field of study, we find that computers, the "other" category, and trades

[24] This pattern of results also holds if we compare our results to unreported results from Kentucky that condition on employment by estimating a log earnings model.

have higher returns than business, health, and transport.[25] Although Lang and Weinstein (2013) find relatively smaller returns to certificates in vocational (i.e., trades and transport) and health areas, it is difficult to directly compare their results with ours because their returns by field of study include public as well as for-profit colleges. The pattern of results in Lang and Weinstein (2013) is similar to the pattern of results by field of study found in the Jepsen, Troske, and Coomes (2014) analysis of community colleges. Stevens, Kurlaender, and Grosz (2018) find much higher returns for health-related fields than non-health-related fields, whereas – like us – Cellini and Turner (2018) find lower returns to attending a for-profit for health.

With the exception of Cellini and Turner (2018), our results suggest that the benefits of for-profit school attendance are broadly similar to previous studies such as Cellini and Chaudhary (2014) for for-profit colleges and Stevens, Kurlaender, and Grosz (2018) for community colleges.

Measured in terms of percentages, our estimates imply that returns for for-profit schools are substantial. However, in determining whether attendance is worthwhile, it is necessary to take account of tuition. For-profit schools in Missouri provided information on the total tuition required to complete their programs, and we matched this information with the programs listed by individuals in our analysis. We found acceptable matches for 95 percent of our individuals.[26] We omitted cases where zero tuition was reported, and where we suspect that tuition was erroneously reported.

[25] As noted above, we ignore estimates for women in trades and transport, given the imprecision of the estimates.
[26] Tuition is listed by institution, year, degree title and CIP code. For 80 percent of cases, tuition was available matching on all these measures. For an additional 15 percent of cases, we used tuition for the same institution and degree title, but we used tuition for another year or another program with a similar but not identical CIP code.

Mean tuition for our sample members in programs offering certificates is $11,136 for men and $11,934 for women.[27] At the extremes, between 5 and 10 percent of individuals attended programs with reported tuition under $1,000, whereas 10-20 percent were in programs with reported tuition between $20,000 and $25,000. Very few reported total tuition over $25,000. Although returns to for-profit schooling are large in proportional terms, tuition has an important impact on net returns because individual earnings are relatively low. At a 10 percent interest rate (a modest rate for student loans), assuming average tuition and our estimated return by quarter, the cumulative net benefits of attending school become positive in the seventh year for both men and women.[28]

For those seeking associate's degrees, the mean tuition is $32,740 for men and $24,117 for women, in each case more than twice the average for certificates. For men, approximately 7 percent had reports of tuition under $20,000, whereas nearly 30 percent reported tuition between $40,000 and $50,000. Essentially no programs reported tuition over $50,000. For women, nearly 20 percent had tuition levels under $20,000, whereas only about 10 percent had reported tuition over $30,000. When we look at returns for both men and women, at a 10 percent interest rate, the cumulative discounted returns are not positive in the seven years following program entry, our analysis window. If the relative earnings for recipients were to remain constant in later years, the breakeven point would occur around the tenth year.

Overall, the universe of students attending for-profit schools in this state seem to gain valuable labor market skills. But, perhaps not surprisingly, the benefits are not equally distributed, and the tuition costs are substantial relative to the returns. Our results are broadly

[27] Recall that all tuition figures are for completion of the program (certificate or degree), not per year.
[28] For simplicity, this comparison between tuition and earnings ignores the fact that the actual tuition paid by students may be lower that the "sticker price" tuition reported by schools.

supportive of for-profit schools, suggesting that net returns are positive, at least for the average participant.

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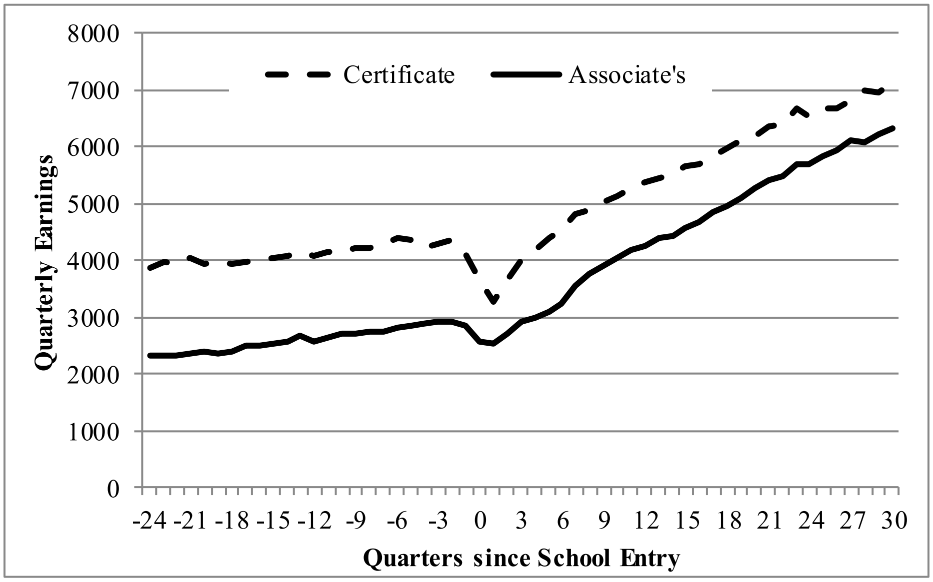
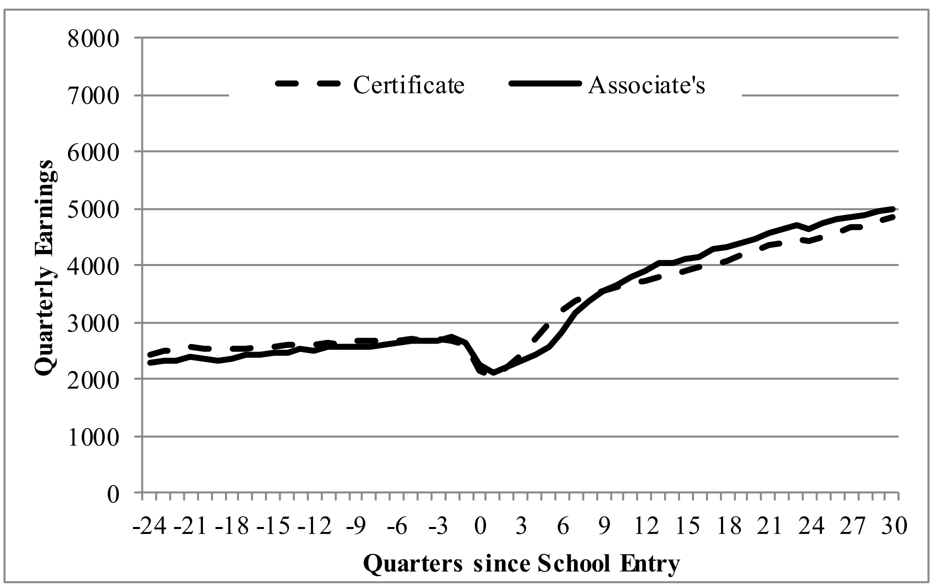
Figure 1a – Quarterly Earnings by Program Type and Quarters since School Entry, Men



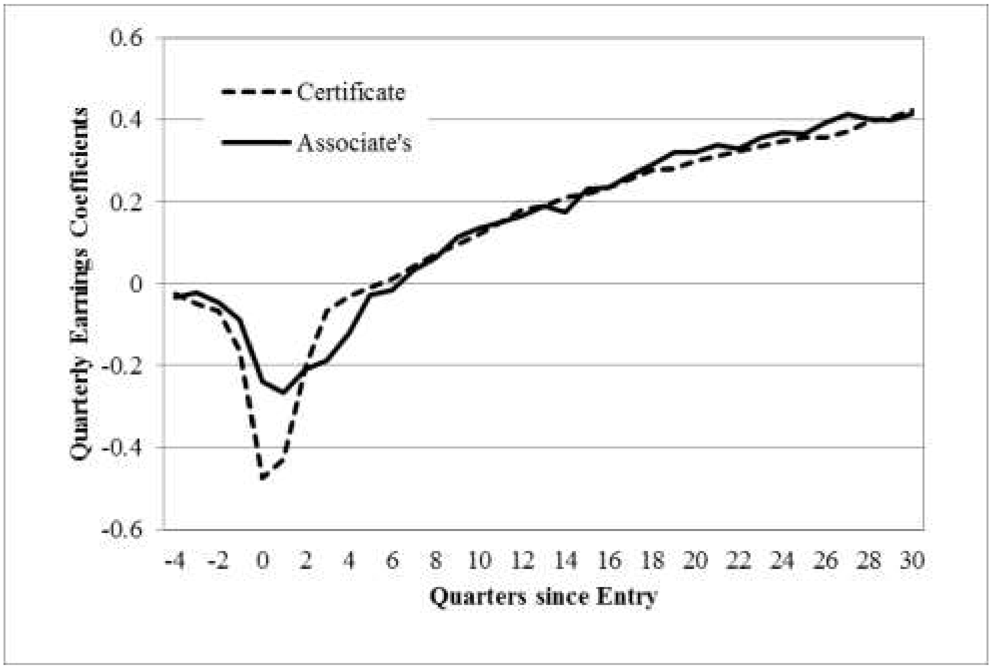
Note: The figure shows the average quarterly earnings separately for men pursuing associate's degrees and men pursuing certificates. Earnings are not conditional on employment, except for the exclusion of strings of quarters of zero earnings of length 10 or more through quarter 30 following initial enrollment.

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



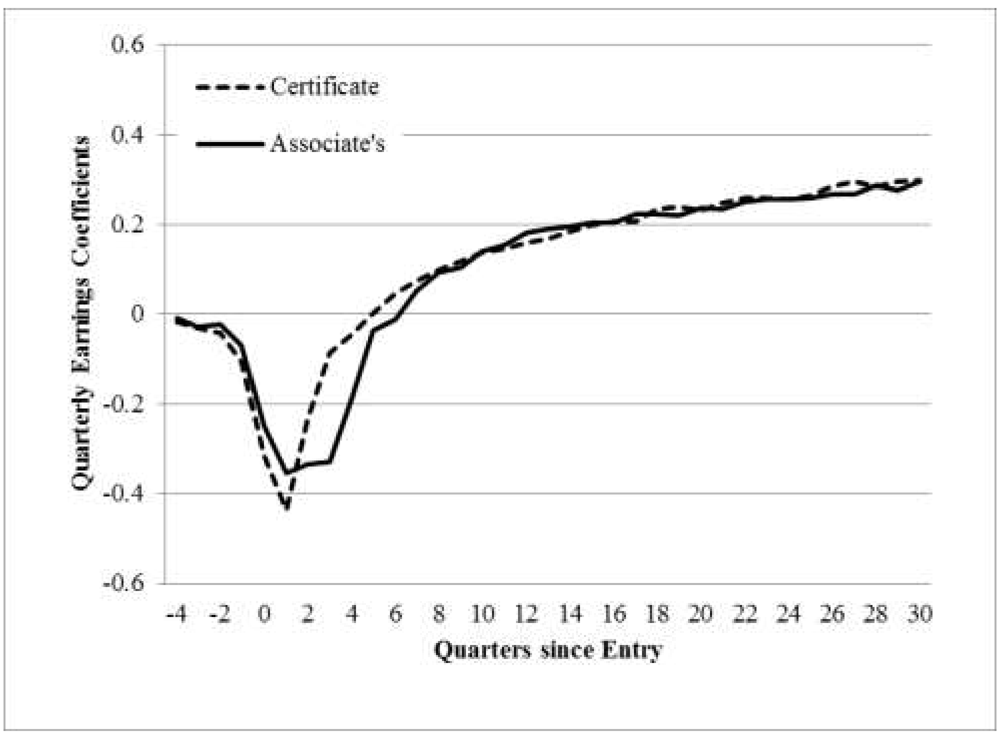
Note: The figure shows the average quarterly earnings separately for women pursuing associate's degrees and women pursuing certificates. Earnings are not conditional on employment, except for the exclusion of strings of quarters of zero earnings of length 10 or more through quarter 30 following initial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

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



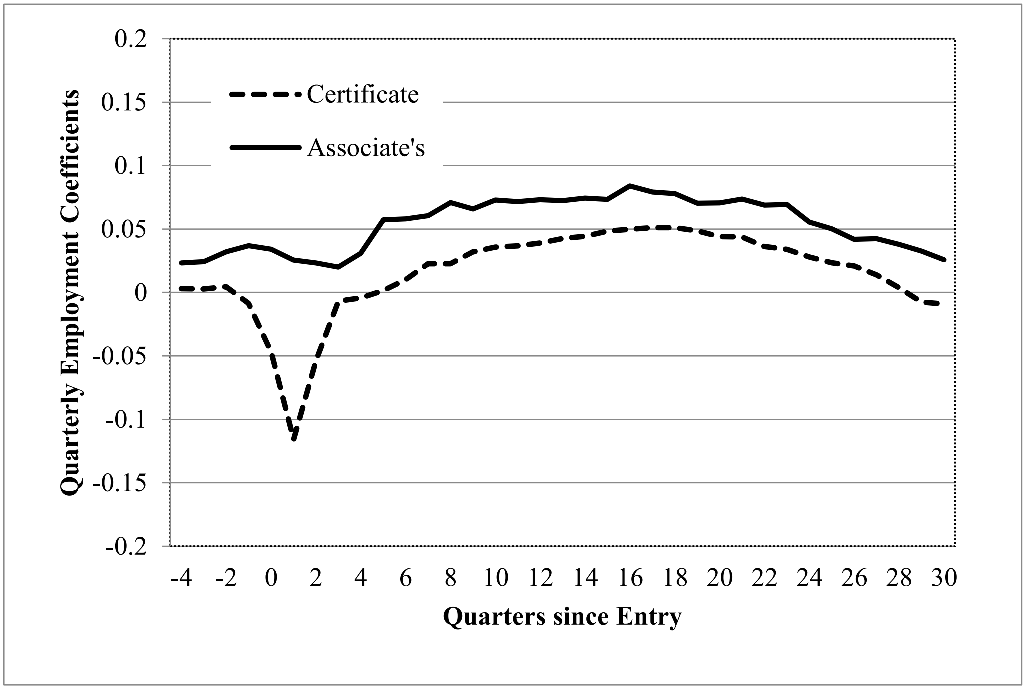
Notes: Each data point is the effect estimate for quarterly earnings from the earnings regression shown in Appendix Table A1.

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



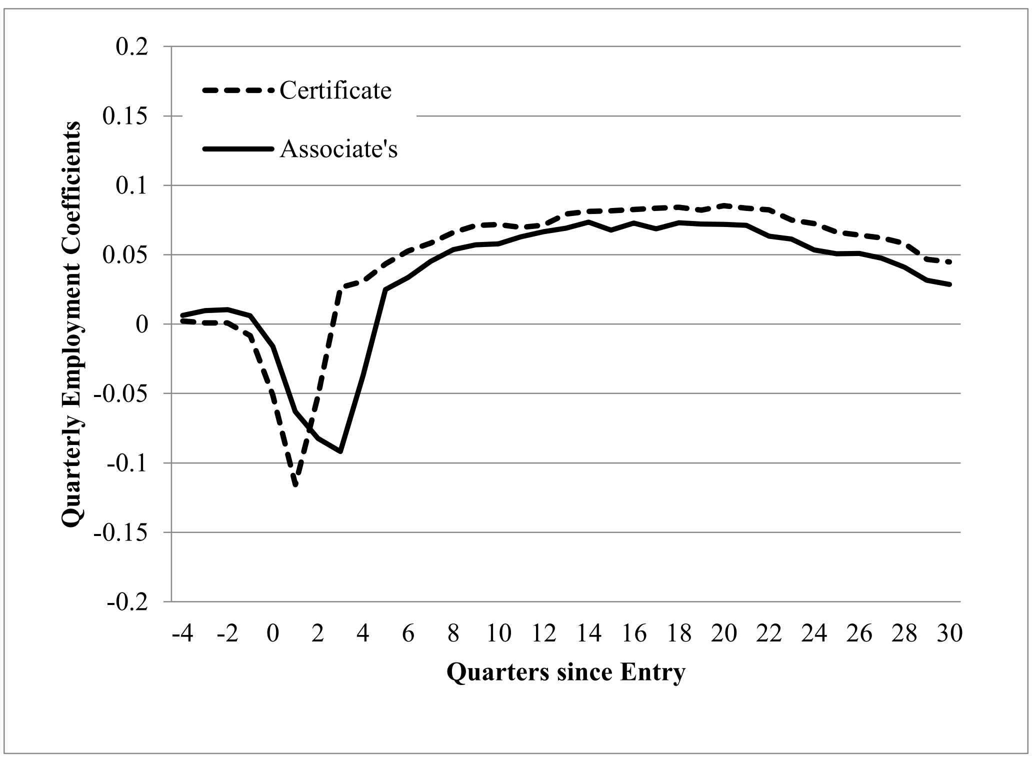
Notes: Each data point is the effect estimate for quarterly earnings from the earnings regression shown in Appendix Table A1.

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



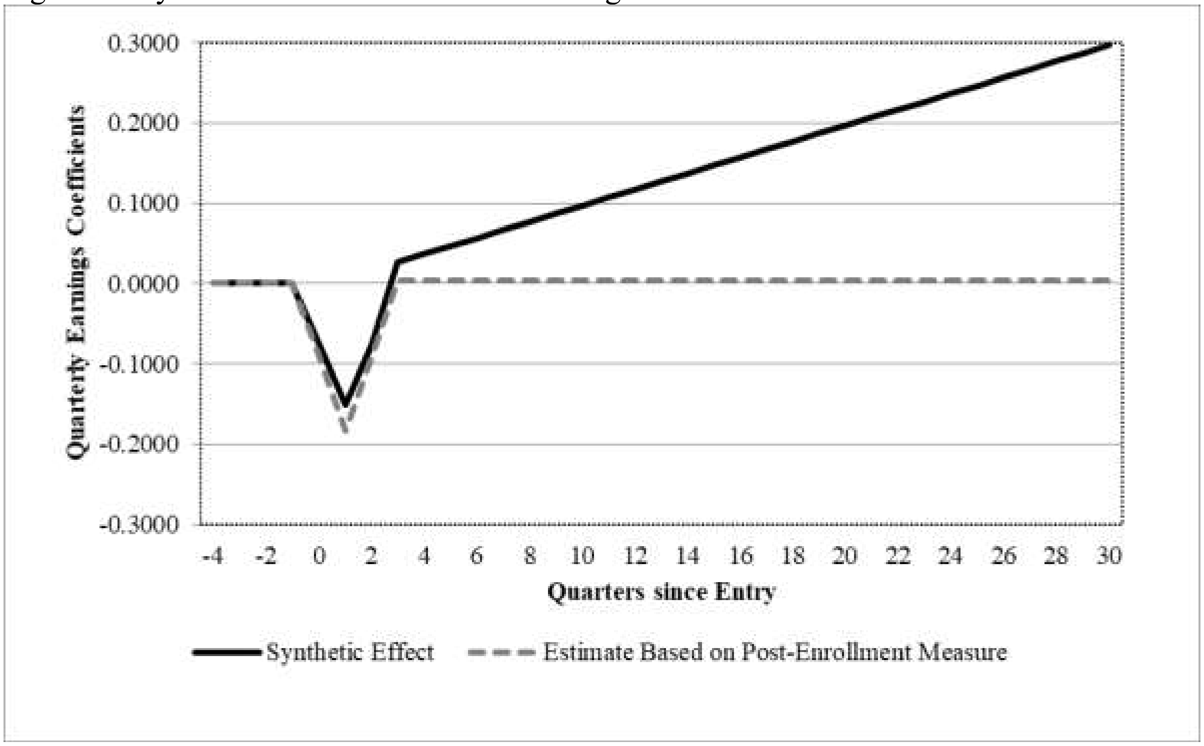
Note: Each data point is the effect estimate for quarterly employment based on equation (2).

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



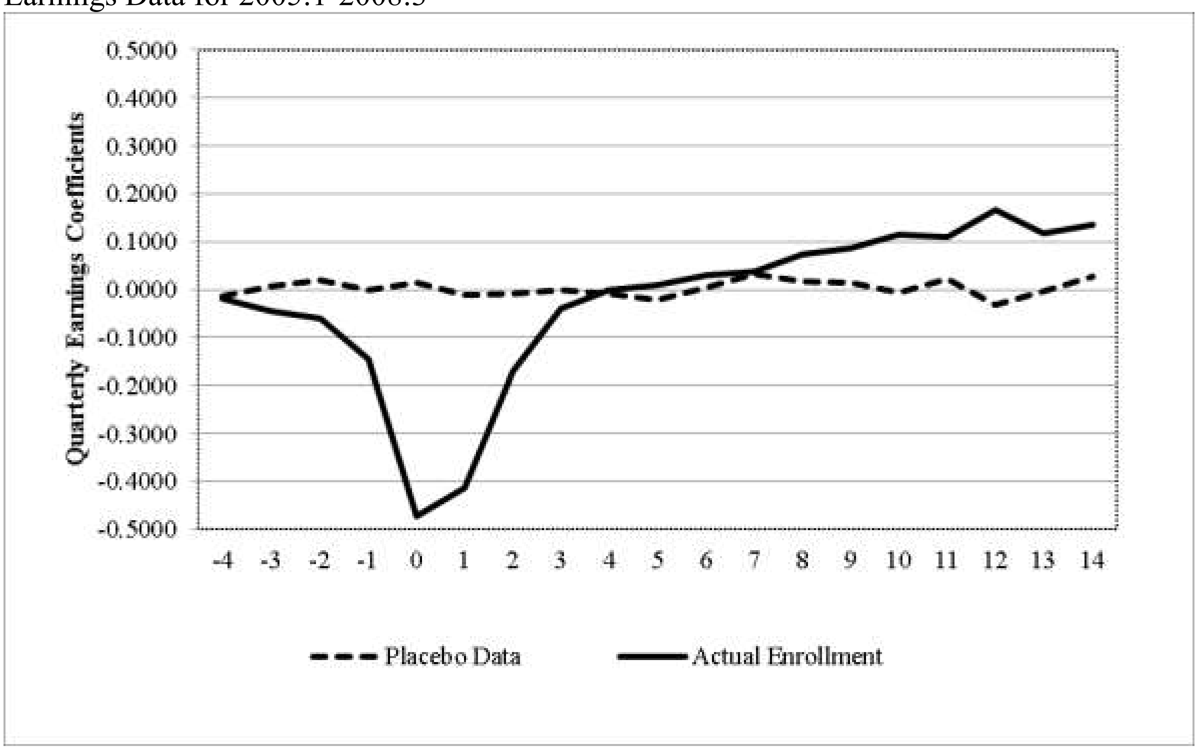
Note: Each data point is the effect estimate for quarterly employment based on equation (2).

Figure 4 - Synthetic Effects Estimates with Single Enrollment Measure



Note: See Appendix.

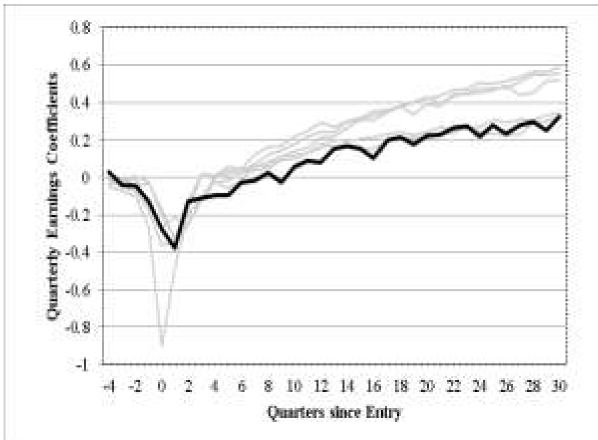
Figure 5: Estimates of Enrollment Effect Based on Placebo and Actual Enrollment Data: Earnings Data for 2005:1-2008:3



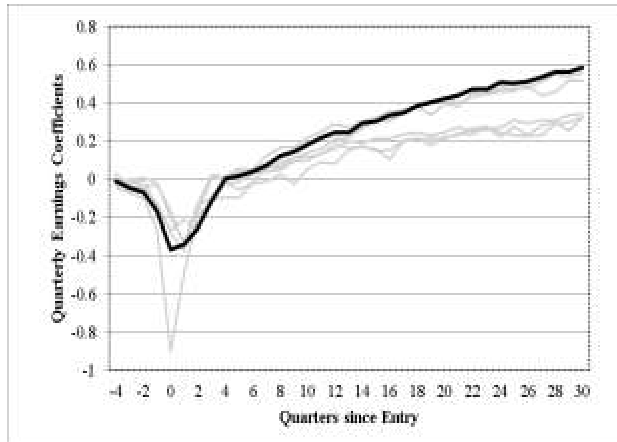
Note: See Appendix.

Figure 6a – Effect of Attendance on Earnings by Quarter and Field of Study, Men
Certificate Programs Only

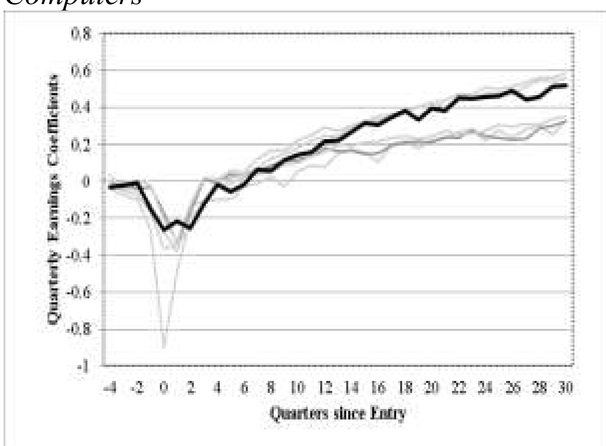
Business



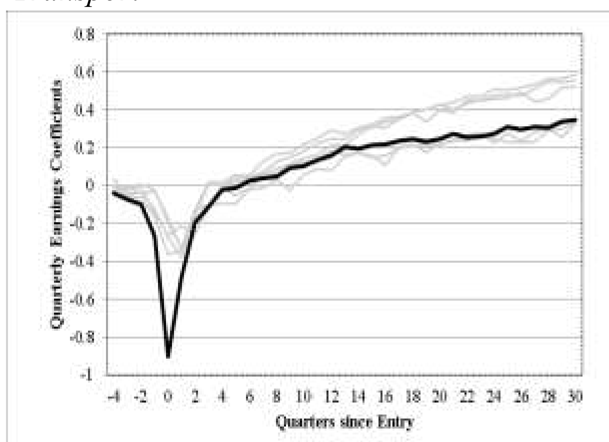
Trades



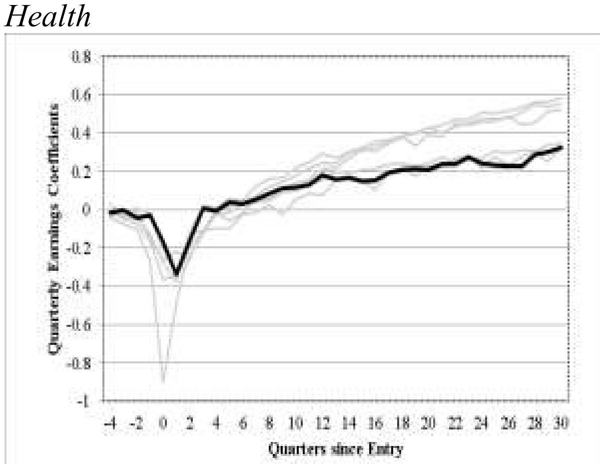
Computers



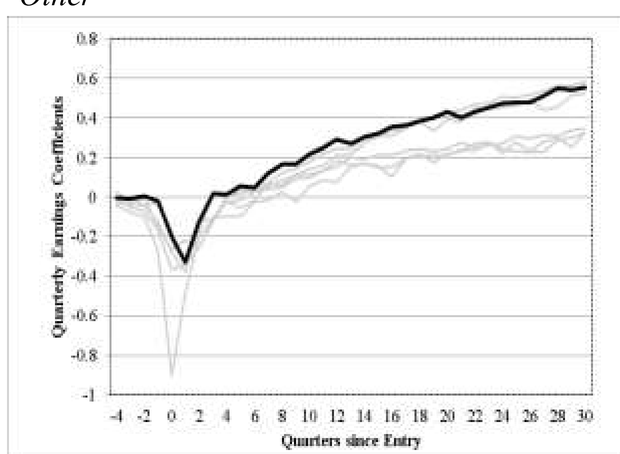
Transport



Health



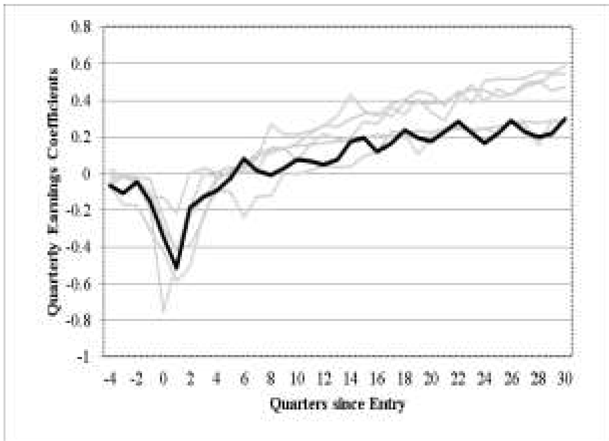
Other



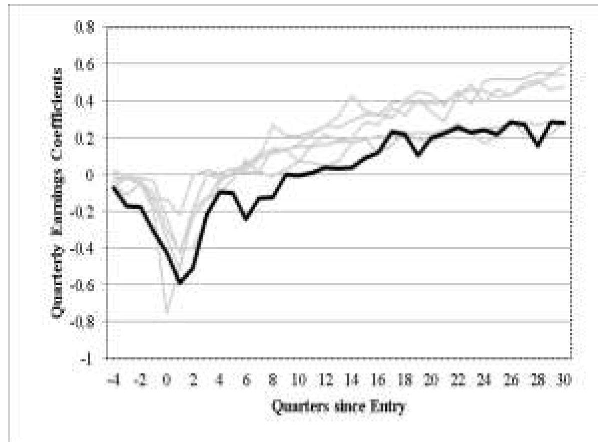
Note: Each panel displays the results from the same regression, but the line in black denotes the returns for the subject indicated in each panel (such as the return for health in the panel labeled "health").

Figure 6b – Effect of Attendance on Earnings by Quarter and Field of Study, Women Certificate Programs Only

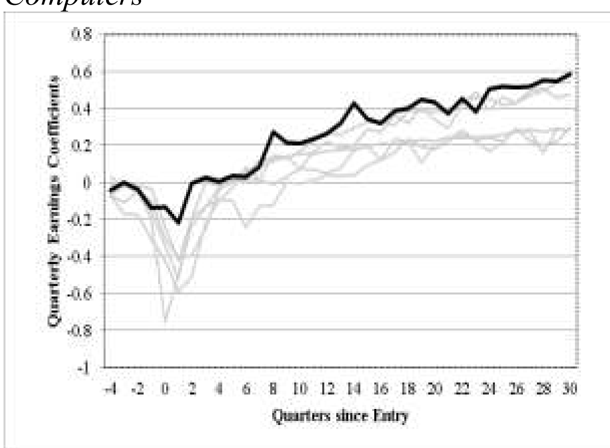
Business



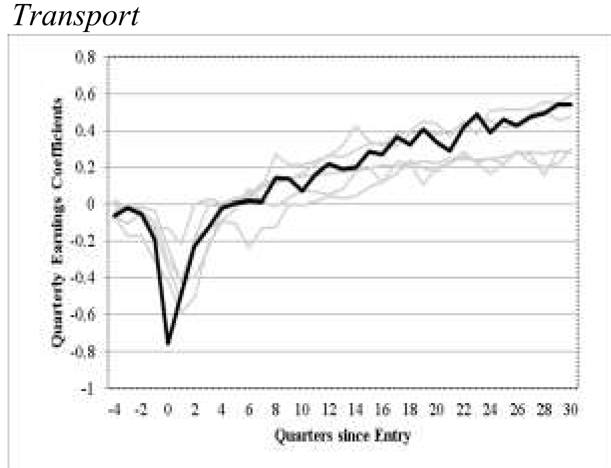
Trades



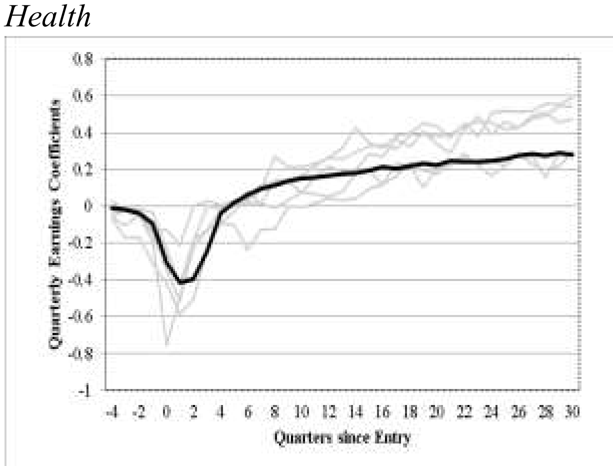
Computers



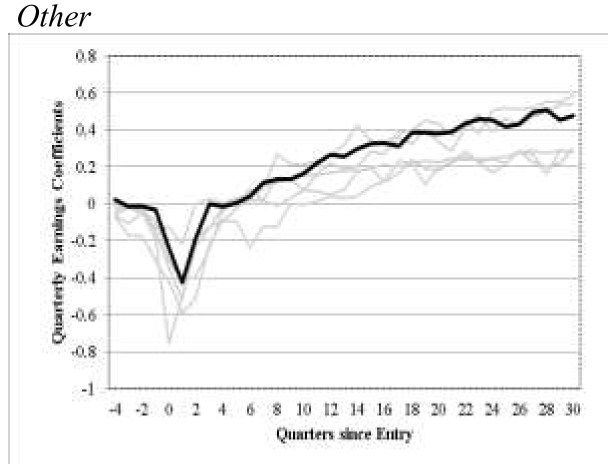
Transport



Health

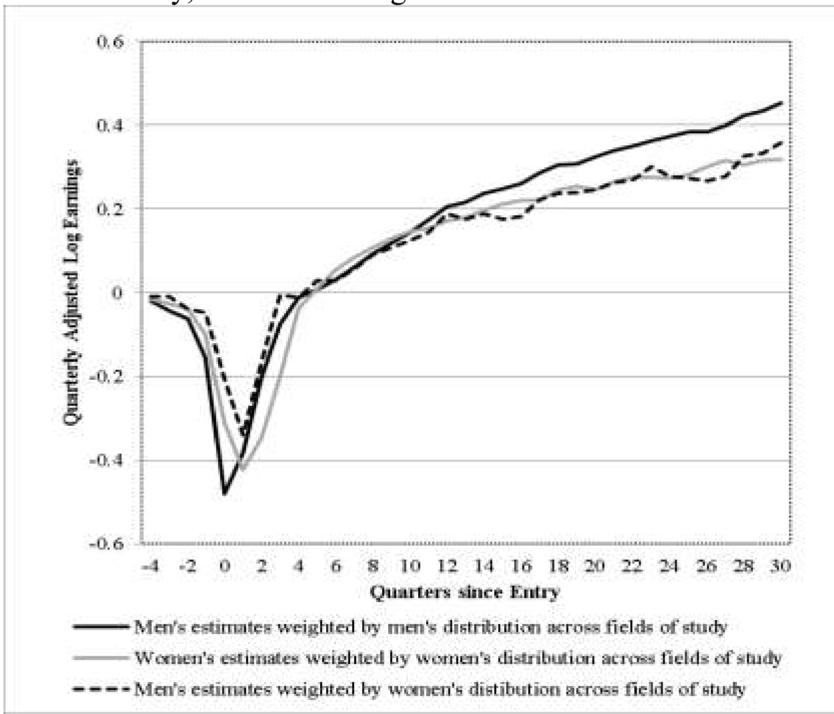


Other



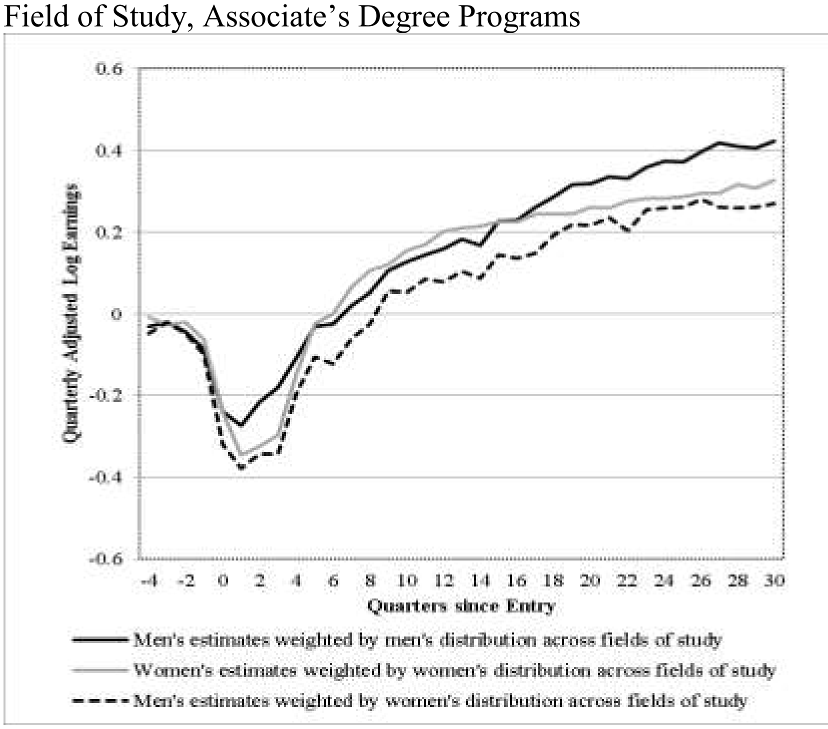
Note: Each panel displays the results from the same regression, but the line in black denotes the returns for the subject indicated in each panel.

Figure 7 – Effect of Attendance on Earnings by Quarter, Men and Women, Weighted by Field of Study, Certificate Programs



Note: Estimates are averages across fields of study weighted by the number of individuals in each field.

Figure 8 – Effect of Attendance on Earnings by Quarter, Men and Women, Weighted by Field of Study, Associate's Degree Programs



Note: Estimates are averages across fields of study weighted by the number of individuals in each field.

Table 1 – Descriptive Statistics by Gender and Program Type

	Men		Women	
	Certificate	Associate's	Certificate	Associate's
Variable	Mean	Mean	Mean	Mean
Demographics and Schooling Information				
White	0.666	0.594	0.565	0.636
Black	0.272	0.270	0.367	0.284
Other / Missing Race	0.063	0.136	0.068	0.080
Age at time of entry	32.7	27.0	29.3	27.3
	(10.7)	(8.0)	(9.9)	(8.4)
Less than high school	0.066	0.018	0.070	0.026
High school	0.721	0.767	0.769	0.797
GED	0.193	0.209	0.153	0.167
Missing education	0.020	0.006	0.008	0.009
Entry year 2005	0.210	0.214	0.214	0.215
Entry year 2006	0.211	0.180	0.199	0.204
Entry year 2007	0.190	0.161	0.190	0.173
Entry year 2008	0.178	0.222	0.192	0.203
Entry year 2009	0.210	0.222	0.205	0.205
Major Urban	0.684	0.742	0.709	0.716
Not Major Urban	0.316	0.258	0.291	0.284
Schooling Information				
Studying business	0.038	0.101	0.069	0.132
Studying computers	0.038	0.276	0.016	0.045
Studying health	0.139	0.179	0.747	0.617
Studying trades	0.329	0.037	0.016	0.003
Studying transport	0.323	0.000	0.028	0.000
Studying other	0.133	0.407	0.124	0.204
Completed certificate	0.700	0.015	0.616	0.032
Completed associate's	0.008	0.434	0.016	0.501
Missing completion info	0.292	0.545	0.368	0.455
Number of Students	22,648	7,079	26,738	12,264

Note: Standard deviation for age is in parentheses.

Table 2 – Descriptive Statistics for Outcomes by Gender and Program Type

	Men				Women			
	Certificate		Associate's		Certificate		Associate's	
Variable	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Earnings								
3-24 quarters before entry	3,806	5,096	2,357	2,757	2,340	2,900	2,254	2,372
2 quarters before entry	4,338	9,087	2,918	4,544	2,671	4,576	2,726	4,947
1 quarter before entry	4,093	10,569	2,862	4,893	2,616	5,292	2,643	4,495
Quarter of entry	3,629	13,069	2,555	4,676	2,152	4,670	2,242	3,827
1 quarter after entry	3,268	8,178	2,537	3,869	2,040	4,016	2,108	3,139
2 quarters after entry	3,657	7,206	2,707	3,851	2,206	3,864	2,208	3,202
3 quarters after entry	4,009	7,474	2,919	4,183	2,414	4,097	2,317	2,989
4 quarters after entry	4,182	7,937	2,979	4,221	2,713	4,021	2,423	3,415
5-8 quarters after entry	4,574	5,814	3,358	3,499	3,217	3,531	2,940	2,849
9-20 quarters after entry	5,457	6,189	4,407	3,793	3,809	3,650	3,951	3,177
21-30 quarters after entry	6,647	7,883	5,775	4,755	4,488	4,111	4,755	3,770

	Men Certificate	Men Associate's	Women Certificate	Women Associate's
	Mean	Mean	Mean	Mean
Employment				
3-24 quarters before entry	0.898	0.871	0.896	0.906
2 quarters before entry	0.678	0.663	0.665	0.702
1 quarter before entry	0.663	0.669	0.657	0.699
Quarter of entry	0.621	0.664	0.612	0.675
1 quarter after entry	0.604	0.667	0.598	0.659
2 quarters after entry	0.643	0.679	0.611	0.660
3 quarters after entry	0.661	0.691	0.643	0.668
4 quarters after entry	0.672	0.689	0.679	0.673
5-8 quarters after entry	0.843	0.851	0.870	0.865
9-20 quarters after entry	0.961	0.962	0.966	0.968
21-30 quarters after entry	0.965	0.967	0.966	0.971

Notes: Earnings are measured in current dollars. Observations with zero earnings are included in the means except for the exclusion of quarters when individuals have 10 or more consecutive quarters after enrollment of missing earnings at the end of the observed time period.

Appendix – Supplemental Analysis, Tables, and Figures

Synthetic Data Set and Model

To illustrate concerns about estimating returns as a single post-enrollment dummy variable, we created a synthetic dataset that incorporates a known (linear) enrollment return. As a first step, we selected from our data men seeking certificates and retained only quarters of data that were five quarters or more prior to school enrollment. We then fitted earnings in those quarters using an individual fixed-effects model, including dummies for each calendar quarter in the data, and a cubic for age effects. For each individual in the dataset, we then substituted synthetic earnings for quarters beginning four quarters prior to their actual enrollment, with earnings constructed to correspond with alternative synthetic return structures. In each case, earnings were constructed using calendar quarter and individual age effects. Because we do not observe any earnings five or more quarters prior to entry after the third quarter of 2008 (recall, all individuals in our dataset begin enrollment in 2009 or before), we do not have estimates for calendar quarter effects after that point, so we have arbitrarily specified the year effects beginning in the fourth quarter of 2008.[29] On these data, we fit a model corresponding to the difference structure in Cellini and Chaudhary (2014), where the post-enrollment dummy takes on the value of one for all quarters after the individual exits the program.

Consider the case where the specified effect on log earnings is 0.05 in the fifth quarter after enrollment and then increases by 0.01 in each subsequent quarter. The solid line in Figure 4 illustrates the synthetic return. However, the estimate based on the constant effect model is very close to zero, as illustrated by the dashed line. We have fitted data where we have specified

[29] We replicated the analysis using only quarters through the third quarter of 2008, for which we can estimate calendar quarter effects for earnings at least five quarters prior to enrollment. Although estimates of effects were limited to only 15 quarters following initial enrollment, the results were substantively identical to those we report below.

a variety of synthetic effects that imply increases over time in the enrollment effects, including effects that correspond to those we have estimated with our data, and estimates of the post-enrollment coefficient are always small compared to actual effects.

Appendix Table A2 provides a comparison based on alternative models fitted with our actual data. In each case, the model corresponds to that in (1) above, including calendar quarter and age controls, but with the enrollment effects replaced by either a single post-enrollment dummy, or a post-enrollment dummy and a linear time trend interaction. In addition, calendar quarters of enrollment are identified with an indicator variable coded as indicated above.

Placebo Test Details

Each individual in our data attends for-profit schooling. A potential concern is that our estimated returns to for-profit schooling merely reflect growth in earnings that would have occurred regardless of attendance. To investigate this concern, we conduct the following placebo test. First, we construct a sample of earnings quarters for males seeking certificates that is limited to earnings at least five quarters prior to enrollment, and so eliminates any quarter when an individual was actually enrolled. For each individual represented in this sample, we randomly assign an enrollment quarter corresponding to a quarter that precedes actual enrollment by at least seven quarters, with the distribution of initial enrollment dates corresponding approximately to the actual enrollment for the full sample.[30]

Figure 5 presents estimates based on the placebo enrollment dates and on data for the same period based on actual enrollment. The actual data contain information on all males

[30] In order to obtain a distribution of enrollment entries, we used our full dataset to create a file with calendar quarter of initial enrollment and last quarter of enrollment for each individual. Omitting those with initial entry dates after the first quarter of 2008, we ordered this file randomly and merged it with each individual in our placebo dataset. Where the merge produced synthetic dates of enrollment that extended beyond the pre-enrollment data in our placebo file, we randomly selected another set of dates from our date file, repeating until we found a set of dates that fell within the range of earnings available for a particular case. This procedure produced a distribution of enrollment dates and length of enrollment that corresponded reasonably closely to the actual enrollment dates in the source file.

seeking certificates with initial enrollment dates from 2005 through the first quarter of 2008, but with all earnings information after the third quarter of 2008 omitted. The model is exactly the one fitted in Table 2 (omitting measures related to associate's degrees), and the results are presented in the same format as for Figure 2a. Since the earliest entrants in the first quarter of 2005 have only 14 successive quarters of earnings data, we can only estimate enrollment effects for 14 quarters. Estimates based on the placebo entry dates are small and none are statistically significant at the 10 percent level. In contrast, actual data provide estimates that are positive and statistically significant beginning in quarter 6. For quarters 10 through 14, estimates range from 0.11 to 0.17.

Appendix Tables and Figures

Appendix Table A1 – Effect of For-profit School Attendance on Log Quarterly Earnings

	Men		Women	
	Coefficient	Std. Error	Coefficient	Std. Error
4 quarters prior to entry	-0.025	(0.007)	-0.018	(0.008)
3 quarters prior to entry	-0.049	(0.008)	-0.031	(0.008)
2 quarters prior to entry	-0.069	(0.009)	-0.042	(0.009)
1 quarter prior to entry	-0.164	(0.010)	-0.105	(0.009)
Quarter of entry	-0.400	(0.013)	-0.159	(0.011)
1 quarter after entry	-0.277	(0.013)	-0.127	(0.013)
2 quarters after entry	-0.129	(0.012)	-0.079	(0.012)
3 quarters after entry	-0.068	(0.012)	-0.087	(0.011)
4 quarters after entry	-0.031	(0.012)	-0.045	(0.011)
5 quarters after entry	-0.009	(0.012)	0.002	(0.011)
6 quarters after entry	0.012	(0.013)	0.047	(0.012)
7 quarters after entry	0.042	(0.013)	0.075	(0.012)
8 quarters after entry	0.072	(0.014)	0.099	(0.012)
9 quarters after entry	0.096	(0.014)	0.118	(0.013)
10 quarters after entry	0.121	(0.015)	0.136	(0.013)
11 quarters after entry	0.150	(0.016)	0.145	(0.014)
12 quarters after entry	0.180	(0.016)	0.161	(0.015)
13 quarters after entry	0.189	(0.017)	0.167	(0.015)
14 quarters after entry	0.211	(0.018)	0.185	(0.016)
15 quarters after entry	0.220	(0.018)	0.199	(0.016)
16 quarters after entry	0.234	(0.019)	0.208	(0.017)
17 quarters after entry	0.257	(0.020)	0.207	(0.017)
18 quarters after entry	0.278	(0.020)	0.233	(0.018)
19 quarters after entry	0.279	(0.021)	0.239	(0.019)
20 quarters after entry	0.298	(0.022)	0.231	(0.019)
21 quarters after entry	0.310	(0.022)	0.249	(0.020)
22 quarters after entry	0.324	(0.023)	0.259	(0.020)
23 quarters after entry	0.335	(0.024)	0.259	(0.021)
24 quarters after entry	0.347	(0.025)	0.257	(0.022)
25 quarters after entry	0.355	(0.025)	0.264	(0.022)
26 quarters after entry	0.355	(0.026)	0.283	(0.023)
27 quarters after entry	0.372	(0.027)	0.296	(0.024)
28 quarters after entry	0.394	(0.028)	0.285	(0.025)
29 quarters after entry	0.404	(0.029)	0.295	(0.025)
30 quarters after entry	0.422	(0.030)	0.299	(0.026)
Associate's*4 quarters prior	-0.008	(0.017)	0.008	(0.013)
Associate's*3 quarters prior	0.026	(0.017)	0.002	(0.013)

Appendix Table A1 (Cont'd) – Effect of For-profit School Attendance on Log Quarterly Earnings

	Men		Women	
	Coefficient	Std. Error	Coefficient	Std. Error
Associate's*2 quarters prior	0.023	(0.017)	0.018	(0.013)
Associate's*1 quarter prior	0.077	(0.018)	0.035	(0.014)
Associate's*quarter of entry	0.235	(0.022)	0.062	(0.018)
Associate's*1 quarter after	0.160	(0.025)	0.077	(0.021)
Associate's*2 quarters after	0.069	(0.022)	0.048	(0.018)
Associate's*3 quarters after	0.030	(0.021)	0.062	(0.017)
Associate's*4 quarters after	-0.015	(0.020)	0.012	(0.016)
Associate's*5 quarters after	-0.020	(0.020)	-0.039	(0.015)
Associate's*6 quarters after	-0.028	(0.019)	-0.058	(0.015)
Associate's*7 quarters after	-0.011	(0.019)	-0.023	(0.014)
Associate's*8 quarter after	-0.010	(0.019)	-0.005	(0.014)
Associate's*9 quarters after	0.018	(0.018)	-0.013	(0.014)
Associate's*10 quarters after	0.014	(0.018)	0.004	(0.014)
Associate's*11 quarters after	-0.001	(0.018)	0.008	(0.014)
Associate's*12 quarters after	-0.014	(0.018)	0.021	(0.014)
Associate's*13 quarters after	0.000	(0.018)	0.023	(0.014)
Associate's*14 quarters after	-0.037	(0.018)	0.011	(0.014)
Associate's*15 quarters after	0.014	(0.018)	0.006	(0.014)
Associate's*16 quarters after	0.000	(0.018)	-0.004	(0.014)
Associate's*17 quarters after	0.009	(0.018)	0.017	(0.014)
Associate's*18 quarters after	0.013	(0.018)	-0.011	(0.014)
Associate's*19 quarters after	0.040	(0.018)	-0.018	(0.014)
Associate's*20 quarters after	0.020	(0.018)	0.006	(0.014)
Associate's*21 quarters after	0.026	(0.018)	-0.015	(0.014)
Associate's*22 quarters after	0.004	(0.018)	-0.009	(0.014)
Associate's*23 quarters after	0.021	(0.018)	-0.002	(0.014)
Associate's*24 quarters after	0.022	(0.018)	-0.001	(0.015)
Associate's*25 quarters after	0.009	(0.019)	-0.004	(0.015)
Associate's*26 quarters after	0.036	(0.019)	-0.015	(0.015)
Associate's*27 quarters after	0.042	(0.019)	-0.028	(0.016)
Associate's*28 quarters after	0.007	(0.021)	0.004	(0.016)
Associate's*29 quarters after	-0.006	(0.021)	-0.019	(0.017)
Associate's*30 quarters after	-0.009	(0.022)	-0.003	(0.017)
Enrolled	-0.152	(0.013)	-0.311	(0.012)
Associate's*Enrolled	0.001	(0.022)	0.008	(0.019)
N	888,054		1,178,783	
Adj R-squared	0.100		0.092	

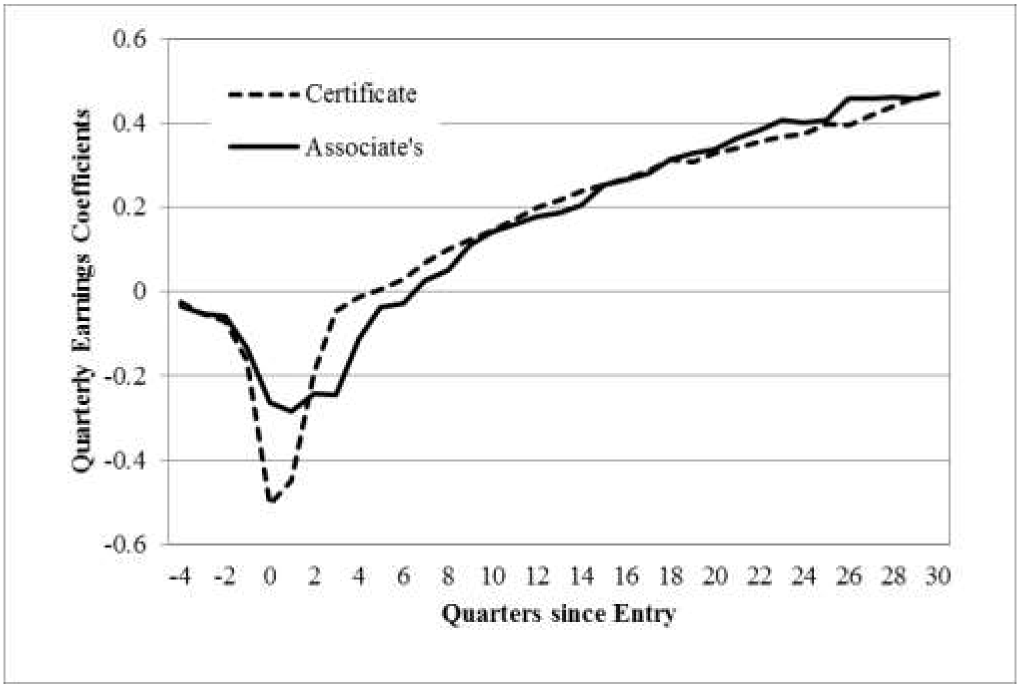
Notes: Coefficient estimates are from equation (1). Standard errors are clustered at the individual level. In addition to the variables shown, all models contain controls for age as a cubic, and person and calendar quarter fixed effects.

Appendix Table A2 – Time-Invariant and Linear Time Trend Estimates of the Log Earnings Returns to For-profit Schooling

	Post-Enrollment Measure		Post-Enrollment Measure and Time Trend	
Men, Certificates	Coefficient	S.E.	Coefficient	S.E.
Post-schooling period	-0.013	0.008	-0.025	0.008
Time trend since entry			0.026	0.001
Men, Associate's				
Post-schooling period	-0.038	0.015	-0.048	0.015
Time trend since entry			0.022	0.002
Women, Certificates				
Post-schooling period	0.025	0.008	0.018	0.008
Time trend since entry			0.017	0.001
Women, Associate's				
Post-schooling period	0.024	0.011	0.015	0.011
Time trend since entry			0.014	0.001

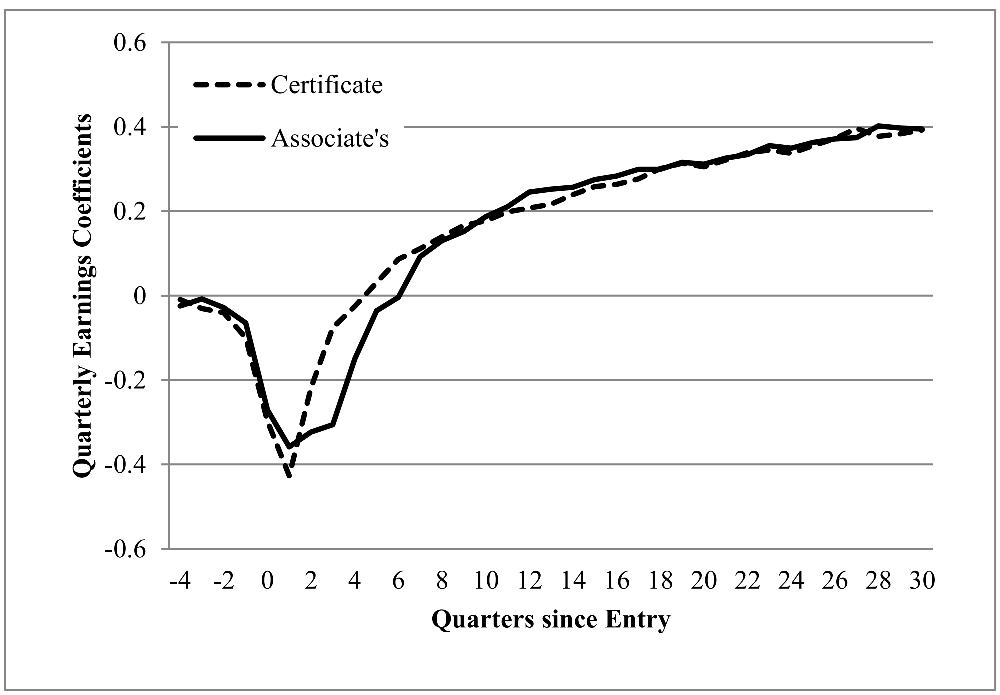
Notes: In addition to the variables shown, all models contain controls for age as a cubic, person and calendar quarter fixed effects, and a variable identifying quarters when the individual was enrolled.

Appendix Figure A1 – Effects of Attendance on Earnings by Quarter and Program Type, Men Completing Degrees



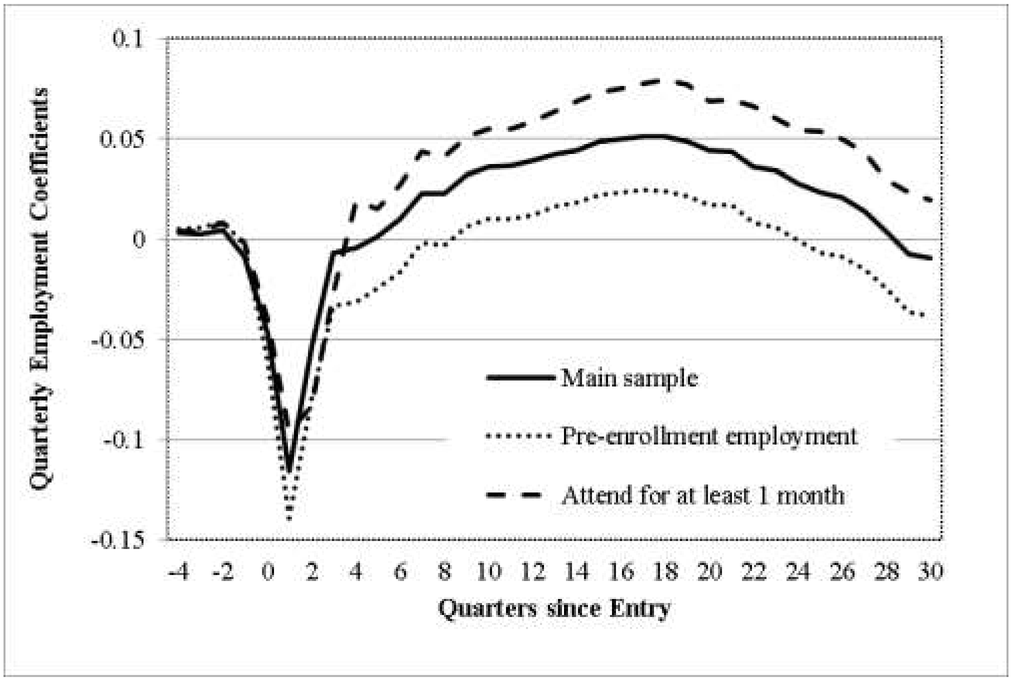
Note: Each data point is the effect estimate for quarterly earnings based on equation (1). The sample is limited to men who completed an award.

Appendix Figure A2 – Effects of Attendance on Earnings by Quarter and Program Type, Women Completing Degrees



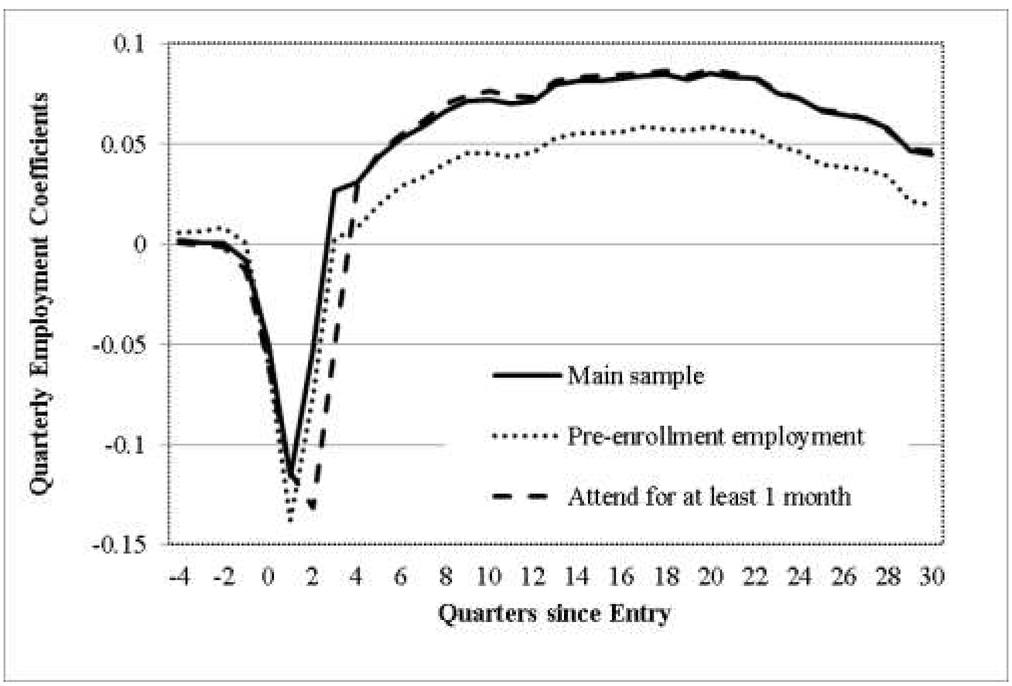
Notes: Each data point is the effect estimate for quarterly earnings based on equation (1). The sample is limited to women who completed an award.

Appendix Figure A3 – Effect of Attendance on Employment by Quarter, Men in Certificate Programs, Alternative Samples



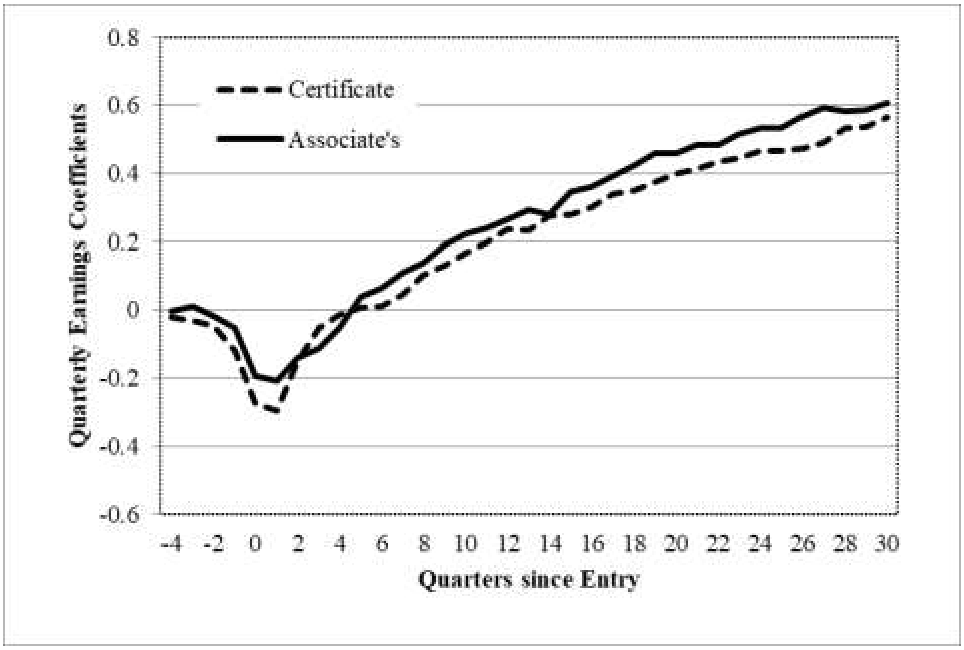
Note: Each data point is the effect estimate for quarterly earnings based on equation (2), fitted for men. The samples differ as indicated.

Appendix Figure A4 – Effect of Attendance on Employment by Quarter, Women in Certificate Programs, Alternative Samples



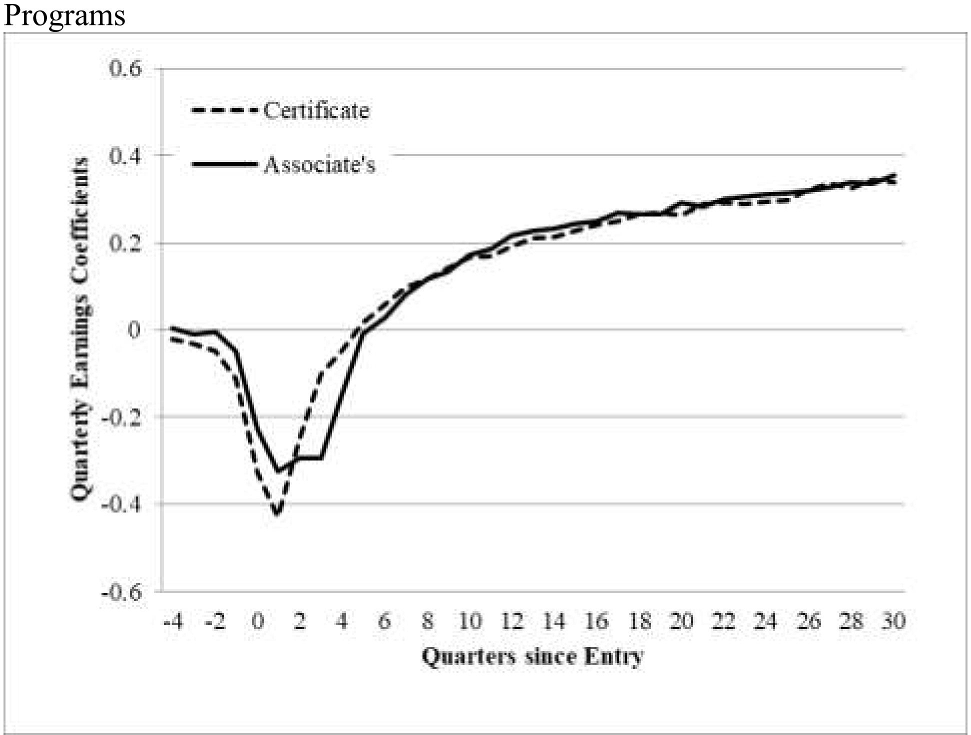
Note: Each data point is the effect estimate for quarterly earnings based on equation (2), fitted for women. The samples differ as indicated.

Appendix Figure A5 – Effect of Attendance on Earnings by Quarter, Men in Title IV-Eligible Programs



Note: Each data point is the effect estimate for quarterly earnings based on equation (1), fitted for men.

Appendix Figure A6 – Effect of Attendance on Earnings by Quarter, Women in Title IV-Eligible Programs



Note: Each data point is the effect estimate for quarterly earnings based on equation (1), fitted for women.