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EVALUATING STUDENT OUTCOMES AT FOR-PROFIT COLLEGES

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This topic was brought to our attention by Jesse Felix whose excellent undergraduate research opportunities project was the earliest use, of which we are aware, of the Beginning Post-Secondary Students survey to examine differences in financial and labor market outcomes of students at for-profit and other institutions. We regret that data access restrictions prevented him from working with us on this new project. We are grateful to participants in the Boston University empirical microeconomics workshop for helpful comments and suggestions. The usual caveat applies. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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ABSTRACT

Using the Beginning Postsecondary Student Survey, we examine the effect on earnings of obtaining certificates/degrees from for-profit, not-for-profit, and public institutions. Students who enter certificate programs at any type of institution do not gain from earning a certificate. However, among those entering associates degree programs, there are large, statistically significant benefits from obtaining certificates/degrees from public and not-for-profit but not from for-profit institutions. These results are robust to addressing selection into the labor market from college, and into positive earnings from unemployment, using imputation methods and quantile regression along with a maximum likelihood sample selection model.

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1 Introduction

The dramatic recent growth of the for-profit post-secondary education sector has generated considerable controversy. Critics charge that these institutions recruit unqualified students in order to obtain their federal student aid. Indeed, students at for-profits account for nearly half of all student loan defaults (Zagier, 2011) but only about 12 percent of post-secondary students (Wolfson and Staiti, 2011). Between 2000 and 2010, the private sector share of federal student aid money grew from $4.6 billion to more than $26 billion (Zagier, 2011), about one quarter of all federal student grants and loans (Wolfson and Staiti, 2011). Proponents argue that for-profit institutions educate non-traditional students who would not otherwise attend college and who therefore benefit from their existence. They maintain that the large quantity of loans and defaults reflects the efforts of for-profit institutions to serve this under-served group.[1]

Recent research by Deming, Goldin and Katz (2012) finds that comparable students at for-profits are more indebted, but they are also more likely to obtain some qualifications, most notably certificates and, more modestly, associates degrees, although less likely to obtain a bachelors degree. They also have lower average earnings although to some extent this is

[1]The principal difference between for-profit and not-for-profit organizations is that not-for-profit firms cannot distribute profits to those who exercise control over the firm (Hansmann 1996) but must distribute them in other ways, such as improving buildings and classrooms or by avoiding profits through lower prices. In the United States, not-for-profit educational institutions are generally exempt from corporate and property taxation, donations are tax deductible for the donor, and they have access to tax-exempt bond financing. However, for-profit institutions can use equity financing. Glaeser and Shleifer (2001) develop a model of why firms choose non-profit status that does not focus on the tax benefits, but rather on the firm's desire to constrain their ability to maximize profits. This improves their competitive standing in markets where firms are able to take advantage of their customers. A related literature on for-profit and not-for-profit hospitals finds no clear difference in quality (McClellan and Staiger 2000). In theory, for-profit institutions have greater incentives to provide lower quality care in the absence of perfect information which could lead consumers to pay a premium to stay in a not-for-profit hospital, but this prediction is not confirmed in the data (Philipson 2000).

explained by lower employment rates. Their paper is deliberately exploratory and does not attempt to address in-depth how differences in unemployment and school enrollment rates influence the estimated earnings differentials.[2]

In this paper, we focus on earnings outcomes. Our main contribution is investigating the labor market return to certificates or degrees from for-profit, and not-for-profit/public institutions. Since students who enter for-profit institutions tend to be disadvantaged, their poorer labor market performance after completing their education may reflect our inability to control adequately for pre-entry differences. If, for example, among those entering certificate programs, the difference in earnings between those who do and do not complete their certificate is similar regardless of institution type, we would be more inclined to believe that the lower wages of for-profit graduates reflects unmeasured differences. On the other hand, if the wage gain among for-profit graduates is lower, then the hypothesis that the for-profit education is less valuable becomes more plausible. To draw this conclusion, we require that any ability (or other) bias affecting the measured return to certification be similar across institution type.

There are large, statistically significant, positive effects of obtaining certificates/degrees from a public or not-for-profit institution among those starting in associates degree programs. We find no evidence that students gain from obtaining any certificate or degree from

[2] Using the NLSY97, Cellini and Chaudhary (2011) find no significant differences in income or other outcomes between those attending private and public institutions. However, the NLSY97 does not distinguish between for-profit and not-for-profit private institutions for those enrolled in associates degree programs. In our sample, approximately half of students starting associates degrees in private institutions are enrolled at a not-for-profit institution. Moreover, the NLSY97 cannot be used to study certificate programs. In our sample, about half of students enrolled at for-profit institutions are in certificate programs. Chung (2008) uses NELS88 which has a sample of only 157 respondents enrolled in either certificate or associates degree programs at for-profit colleges. She finds that, after controlling for selection, obtaining a for-profit certificate results in 141 to 158% higher earnings compared to those who have selected into the for-profit sector but do not complete any degree.

a for-profit institution. However, among those both starting and finishing associates degree programs, the benefit of the degree is also not statistically significantly different across institution types. Among those entering certificate programs, we find no benefit of receiving a certificate from any type of institution. These results are robust to addressing selection into the labor market from college and into positive earnings from unemployment, using imputation methods and quantile regression along with a maximum likelihood sample selection model.

The paper proceeds as follows: Section 2 discusses the methods. Section 3 describes the data. Section 4 contains the results. Section 5 presents robustness checks, and Section 6 concludes.

2 Methods

Our data, discussed in detail below, consist of students entering post-secondary education in 2003-04. In everything that follows, we conduct our analysis separately for those entering certificate and associates degree programs. We do not examine students starting in non-degree or BA programs. For ease of presentation, in this section we drop notation distinguishing whether the sample is those entering certificate or associates degree programs. We use the term certification to refer to certificates, associates degrees, or bachelor degrees.

2.1 Basic Regression Analysis

Our starting point is a fairly standard log-earnings equation:

$$y = \alpha + \beta_1 FP + \beta_2 C + \beta_3 A + \beta_4 BA + \beta_5 C_{FP} + \beta_6 A_{FP} + \tag{1}$$
$$+ \beta_7 BA_{FP} + X_1\beta_8 + \eta$$

where y denotes log annual income in 2009. We have dropped the i subscript for simplicity. The variables C are indicators for whether the highest degree obtained was a certificate, and similarly for A(ssociates) and BA(chelors). The variable FP is an indicator for whether the student started at a for-profit, and the subscript FP denotes that the certificate or degree was obtained from a for-profit. Note that an individual who enters, for example, a certificate program in fall 2003, may have no certification, a certificate (but no higher certification), an associates degree or a bachelors degree in 2009.

β_2 through β_4 capture the "return" to earning certification at a not-for-profit or public while β_5 through β_7 capture the difference between this return and the return at a for-profit institution. There is a long history in labor economics of papers addressing ability (and other forms of) bias in estimates of the return to education. We will not revisit this literature. However, ability bias should be smaller in our setting than in the standard case. In our case, ability bias reflects the difference in ability between those finishing and those starting but not finishing at a particular type of institution. This should be smaller than the ability difference between those finishing at all institution types and those either starting but not finishing or never even attempting programs at the same level.

Moreover, the existence of ability bias is relatively unimportant for our analysis because β_5 through β_7 can be interpreted as differences-in-differences estimates. Therefore, we require

only that any ability bias is similar across institutions. We discuss the plausibility of this assumption when we describe the data.

β_1 represents the return to starting a certificate or degree at a for-profit relative to a not-for-profit or public, for those who do not receive any certification. This coefficient can differ from 0 either because human capital acquisition differs even among those who do not complete a certification or because the types of students who do not complete any certification differ among institution types. To address the former explanation, in some specifications we control for time spent on post-secondary education and its interaction with FP.

X_1 denotes the explanatory variables that are determined before or just after enrollment and therefore unlikely to be caused by starting-institution type. These include age, Black, Hispanic, male, whether English is the primary language, marital status in 2003, whether born in the United States, household size (this variable is equal to parents' household size for dependent students, and respondent's household size for independent students), income in 2002 (this variable is equal to parental income for dependent students and respondent income for independent students) and number of dependent children in 2003. It includes family background characteristics: expected family contribution to college finances in 2003, parents' highest level of education is a high school diploma or less, whether the individual was claimed by another as a dependent in 2003, whether both parents were born in the United States, dummy variables for four initial majors (business; liberal arts and sciences; health, human services, and education; manual and technological vocational training; undeclared is the excluded category), whether attained a high school diploma, whether obtained a GED, and the number of years the individual delayed enrollment in college after high school. For those who began their post-secondary schooling before age 24, we also have indicators for

parents' income is less than 30,000 dollars per year, high school GPA above 3.0, took the SAT or ACT, and SAT/ACT score.

There are a number of potentially endogenous variables in the data. These include the total amount of grants and veterans benefits for college the individual received in 2003, whether the individual was always a full-time student during his/her postsecondary schooling, GPA in 2003/2004, number of dependent children in 2006, and 2009, married in 2009, the number of times the student transferred, the number of months since the individual was last enrolled in college, and the total months enrolled. The last two variables are, for example, measures of education quantity and post-education experience but may also be influenced by institution choice. Our preliminary investigations showed only minor differences from our main results when we included the post-2003 characteristics. We therefore present results only with the clearly exogenous explanatory variables except for some robustness checks.

As noted above, there is a potentially important subset of variables available only for those starting post-secondary education before age 24. We therefore chose to obtain four sets of estimates: a "young" sample with all variables, a young sample with only variables available for the entire sample, an "old" sample, and the full sample. For the young sample, a specification test never rejects that our baseline estimates of the effects of credentials are unaffected by including the under-24 variables. Moreover, we cannot reject the hypothesis that the coefficients are the same for the young and old samples. Therefore, we present the main specification estimates for the young sample with all variables and for the full sample. In all remaining specifications, we present only the results for the full sample, which are also generally the most precise. However, in the text we point out any notable differences among samples and specifications.

2.2 Propensity Score Weighting

Since we will see that students beginning at different institution-types have quite different background characteristics, and since the linearity assumptions underlying the regression model may be problematic, as a robustness check we also present propensity-score-weighted regressions. We use the standard logit model to calculate the propensity scores. We include all variables in X_1 *a priori*, including the under-24-only variables in the specifications for which they are included in the wage equation. We calculate propensity scores separately for the four samples/specifications described in the previous sub-section.

To determine which interactions between variables to include, we make use of an algorithm proposed by Imbens (2010). When we use the full sample, the algorithm finds that 50 interaction terms should be included in the propensity score specification. When added to the 21 linear terms, this results in a total of 71 variables in the propensity score specification. Applying the same procedures as above to the associates degree sample, we include 47 interaction terms and the 21 linear terms that we included a priori.

Kernel density estimates show that there is a lack of overlap between those starting at for-profit, not-for-profit, and public certificate programs at the bottom of the propensity score distribution. Following the advice and analysis of Imbens (2010) and Crump, Hotz, Imbens, and Mitnik (2006), we discard observations with propensity scores below .1 or above .9. The lack of overlap is much less dramatic for those starting in associates degree programs. We thus do not need to discard all observations with propensity scores below .1 or above .9. Instead, we trim the sample so there is common overlap; thus we discard all observations starting in not-for-profit or public programs with propensity scores below the

lowest propensity score or above the highest propensity score of an individual starting in a for-profit program.

Once we have calculated the probability that an individual started in a certificate (associates degree) program at a for-profit college, those probabilities are used to generate weights in the usual way.

We also check that the balancing property is satisfied for the propensity score (using the trimmed sample). To do this, we use an algorithm similar to that developed by Becker and Ichino (2002). However, we use the standard Bonferroni correction for determining the threshold p-value for significance of each test since we have multiple tests of whether the average propensity score for each group differs within blocks. When we test whether the average of each variable differs within blocks, we use the Bonferroni correction adjusted for correlation across explanatory variables. Since we have many variables, and some of those variables are presumably correlated, a balancing test for one variable could provide information on the balancing test for another variable. The information from these correlations should be accounted for in the Bonferroni correction, raising the threshold p-value (lowering the critical t) used to determine significance for each test, relative to the traditional Bonferroni p-value which assumes no correlation between variables. In order to implement this strategy, we find the correlation between each of the variables used in the propensity score specification, and then take the mean of those correlations. The mean correlation is then used to adjust the standard Bonferroni correction. In the full sample of those starting in certificate programs, this algorithm results in 6 blocks and no variables that are unbalanced. In the full sample of those starting in associates degree programs, we obtain 8 blocks and one variable that is unbalanced out of the 544 total variables.

3 Data

We use the Beginning Post-Secondary Student Survey, 2004 (hereafter BPS). The BPS sampled approximately 16,680[3] students who began post-secondary education for the first time in the 2003-2004 academic year. Students were surveyed in their 1st, 3rd, and 6th year after entering college. Because a substantial proportion of students who enter four-year programs do not complete within six years, we focus on a sample of close to 2,050 students who entered certificate programs and 5,740 who entered associates degree programs. All of the institutions we observe in the data have signed Title IV agreements with the Department of Education, meaning they are eligible for Title IV aid, and so they are accredited by at least one of the Department of Education's approved accrediting agencies.

We examine outcomes separately for those initially enrolled in certificate and associates degree programs. We do not distinguish among individuals on the basis of the types of programs the institution offers. Thus we pool individuals enrolled in, for example, certificate programs at two-year and four-year institutions. We note that a nontrivial number of respondents report themselves as being enrolled in a bachelors program at a two-year institution or in an associates degree program at a non-degree granting institution. These have been recoded in the released data. However, this cleaning process would not catch respondents who under-reported their certification or the nature of their program. There is thus some risk that some respondents may have reported being in a certificate program and/or having earned a certificate when, in fact, they were in an associates degree program.

Unfortunately, 2009 labor market data were collected only for those whose last date of

[3]For confidentiality reasons, the Department of Education requires that any time a number of observations is provided, it is rounded to the nearest ten.

enrollment was before February 2009. While our baseline results drop students who were still enrolled five and a half years after entering a certificate or associates degree program, we perform a number of robustness checks to address these missing data.

Table 1 summarizes the data. The first two columns are the certificate program sample, separated by those starting at a for-profit (1190 observations) and not-for-profit/public (860 observations). The last two columns describe the associates degree sample which is dominated by those starting at not-for-profit/public programs (5210 observations) relative to those starting at for-profit programs (530 observations). The means are calculated using the sampling weights. Despite some coding differences, the results are broadly similar to those in Deming, Goldin and Katz (2012).

3.1 Certificate programs

It is evident from Table 1 that students starting certificate programs at for-profit institutions have significantly worse outcomes than students starting in not-for-profit/public institutions. Income in 2009 is approximately $5500 lower for students starting at for-profit institutions than for students starting at not-for-profit/public institutions, statistically significant at the .01 level.

It is by no means evident, however, that this difference is causal. Compared to those who start at not-for-profit/public institutions, students starting in certificate programs at for-profit institutions are much more likely to be Black, Hispanic, female, younger, and single at the time they enter college. They are less likely to speak English as their primary language, and their parents are less likely to have been born in the United States. Furthermore,

income in 2002 (parental for dependent students and respondent for independent students) and expected family contribution to college are much lower. They are also less likely to have taken the SAT, less likely to have received their high school diploma, and less likely to have had a high school GPA above a 3.0.

An important issue is whether ability bias is likely to differ across institution type. We can cast some light on this by examining the distribution of certifications. Slightly more of those entering for-profits (46%) than of those entering other institutions (42%) have not gained any certification. This difference falls short of statistical significance at any conventional level. Similarly the former are slightly and insignificantly more likely (53% v 52%) to have attained a certificate but nothing higher. This suggests that any ability bias affecting the estimated return to obtaining a certificate should be similar for students at the two types of institutions.

3.2 Associates Degree programs

Table 1 also shows that students starting associates degree programs at for-profit institutions have significantly worse outcomes than those starting in not-for-profit/public institutions. Their income in 2009 is approximately $3000 less than the income of students starting at not-for-profit/public programs, statistically significant at the 5% level.

Again, it is not clear that this difference is causal. Compared to those who start at not-for-profit/public institutions, students starting in associates degree programs at for-profit institutions are much more likely to be Black and Hispanic; however, they are also more likely to speak English as their primary language. Furthermore, income in 2002 (parental for

dependent students and respondent for independent students) is much lower among for-profit students, their parents' highest level of education is more likely to be a High School diploma or less, and their expected family contribution to college is much lower. They are also less likely to have taken the SAT, and those who do have lower scores. Students starting at for-profits are less likely to have obtained their High School diploma, and more likely to have obtained their GED. Interestingly, those starting at for-profit institutions are more likely to have a High School GPA above a 3.0.

Unfortunately, the pattern of certification differs between those entering associates degree programs in for-profits and not-for-profits/publics. In both cases there are small, not significantly different, numbers of students reporting leaving with a certificate as their highest degree (4% at for-profits and 6% at not-for-profit/publics) and roughly similar numbers who have acquired no certification (65% at both institution-types), but those starting in associates degree programs at public and not-for-profit institutions are much more likely to have gone on to a bachelors degree than are those who started at a for-profit institution. As a consequence, those having only an associates degree are likely to be more favorably selected if they began at a for-profit.

4 Results

As in most of the literature studying labor market returns to education, we use the term "return" somewhat loosely to refer to the percentage difference in income between an individual who obtains a degree and one who started a degree, but did not obtain a degree. Thus, this use of return does not account for costs. As discussed above, there is an extensive literature

on ability bias in OLS estimates of the return to schooling. To the extent that those who attempt post-secondary education but do not complete it are more favorably selected than those who never attempt it, such ability bias should be less of a problem in our sample. We initially estimated our models using four combinations of specifications and samples. However, we could not reject the hypothesis that the key coefficients were unaffected by the presence or absence of variables available only for those entering post-secondary schooling before they turned 24 years old. We also could not reject the hypothesis that the results were the same for the older and younger samples. Therefore we present only the results for the younger sample with the added variables and for the whole sample.

4.1 Return to Certificates/Degrees: Started Certificate Program

Table 2 shows the effect of obtaining certificates/degrees on students who begin in a certificate program. Using the basic specification, the point estimates show no benefit from obtaining a certificate from a public or not-for-profit institution although the standard errors are large. The point estimates for the young, but not the full, sample suggest a small return to a certificate from a for-profit institution, but given the large standard errors, even the positive coefficient is indistinguishable from zero. When we use propensity score weighting the point estimates suggest a positive return to certificates in both for-profits and other institutions for the younger sample, but the coefficients are dwarfed by their standard errors. For the whole sample, we again find no evidence of a benefit from earning a certificate from a for-profit. Taken together, the results strongly suggest that, at least for those completing their education within five and a half years of starting, there is no market benefit from a certificate

among those who begin in such programs.

Although it is not the focus of our paper, we note also that the coefficient on the for-profit dummy is insignificant and, at least in the full sample, small. Thus, those leaving certificate programs without certification have similar incomes regardless of where they enrolled. Unless dropouts differ either in the time spent enrolled or post-enrollment experience (both explored briefly below), we would expect dropouts from different institution types to have similar earnings if we control adequately for other differences. The absence of a significant coefficient on the dummy variable is therefore reassuring.

4.2 Return to Certificates/Degrees: Started Associates Program

Table 3 is similar to Table 2 except that it shows the results for those entering associates degree programs. The results are striking. Students who obtain certificates/degrees from a public or not-for-profit institution receive a large wage premium. The value of an associates degree is large and statistically significant at the .05-level or better in all four specifications, with magnitude as large as 14 log points. Depending on the sample and estimation technique, the value of a certificate ranges from 21 to 35 log points and is statistically significant at the .05-level or better in all four specifications. However, the number of students whose highest degree is a certificate, but started in an associates degree program, is small and probably includes some associates degrees misreported as certificates. For the group obtaining a BA, there is also a large and statistically significant premium in all four sets of estimates. We note, however, that this group is highly selected since it consists of students who entered an associates program and received a BA within five years. Further, the number of these

students is also small.

In contrast, there is little evidence of a return to any certificate or degree from a for-profit institution. The estimated return to an associates degree is negative for one specification and small in the rest of the specifications. Even the largest coefficient (OLS on the full sample) only suggests a modest 9.2 percent return to associates degrees. However, we also note that the difference between the returns from a for-profit associates degree and a not for profit/public associates degree is also not statistically significant and is small in some specifications. Still, since a much higher proportion of students go on to bachelors degrees at not-for-profits and publics, we expected that, if anything, our estimates of the return to an associates degree at a for-profit would be biased upwards relative to other institution types.

The estimated premium from earning a for-profit certificate is negative in three of four estimates and is only 4 percent in the fourth. Similarly, for the young sample, the point estimates of the premium from earning a for-profit BA are negative or very small. For the full sample, they are positive, 6.7 percent using OLS and 8.8 percent using propensity-score weighting. Again, we note that those with a BA are a highly selected sample and the sample sizes of those starting in associates degree programs and obtaining certificates or bachelors degrees is small.

Taken together, these results confirm that students who begin associates degrees at traditional (not-for-profit and public) institutions and obtain a degree earn higher wages than apparently comparable individuals who do not. However, we find no evidence that students benefit from certifications received at for-profit institutions.

We note that, in contrast with the case of students entering certificate programs, the coefficient on the for-profit dummy is negative, large and statistically significant in one

specification. The finding that dropouts from associates degree programs do worse if they started at a for-profit is disturbing and suggests that we have not fully accounted for differences between students at for-profit and other institutions. The robustness checks in the next section are intended, in part, to address this concern.

5 Robustness Checks

In this section, we report the results of a series of robustness checks using only the full sample and only OLS except for a few cases when the results using other samples or propensity-score weighting differ.

5.1 Selection

A major empirical challenge in assessing the effect of for-profit colleges on student outcomes is that there are two groups of people who are missing wages in our sample: those who are unemployed and those who are still enrolled in school as of February 1, 2009.[4] Overall, among the 1870 students starting in certificate programs, 300 were enrolled as of February 2009 and 430 were unemployed. Similarly, among the 5580 who started associates programs, 1650 were still enrolled and 860 were unemployed.

Two types of selection concerned us. The first is selection in levels, for example if the best students leave most quickly. This would lead us to estimate an effect among only a very particular subset of the relevant population. The second is differential selection across

[4]Income is not missing if the student is currently enrolled in an undergraduate college, but not in a degree program, and is employed.

institution types, for example if the sorts of students who are no longer enrolled or who find jobs differ across starting-institution type.[5]

To address these questions, we begin by asking whether there are differences in the proportion of students who were enrolled on February 1, 2009 or later. About one-sixth (16%) of students entering certificate programs report their last date of enrollment as February, 2009 or later. This figure is nearly identical for those starting at a for-profit (16%) and those starting at a not-for-profit or public (17%). The p-value for this test of equality is .51. Thus, at least in terms of proportions still enrolled, the two types of institutions do not differ for those entering certificate programs.

The proportion of those who started in an associates program and were still enrolled on February 1, 2009 or later is much lower for those who start in a for-profit associates program (19%) than for those who start in a not-for-profit or public associates program (31%). We can reject equality of these percentages at conventional levels of significance.

Addressing differences in selection is facilitated if the pattern of selection is similar for all groups. We therefore estimated, separately for those starting in for-profit programs and those starting in not-for-profit/public programs, a linear probability model for whether the individual was still enrolled on our standard set of pre-enrollment variables. For those starting in certificate programs, we cannot reject that the determinants of schooling completion are identical. For those starting in associate degree programs, we cannot reject that the selection equations differ only by an intercept.

We take the results of the selection analysis as support for the view that our results

[5]Selection could also exacerbate ability bias if, for example, the weakest students drop out and the best students finish fastest.

for those entering certificate program are valid albeit with the caveat that they need not apply to the roughly one-sixth of the sample still enrolled. However, for those who initially enrolled in an associates degree program, we are concerned that our results could be affected by selection bias. We note that among those starting in associates degree programs, those still enrolled in school are more likely to be Black, female, younger, have parents not born in the US, smaller household size, lower expected family contribution to college in 2003, but higher income (parental for dependent students and respondent for independent students) in 2002, and less likely to be Hispanic.

5.1.1 Imputations

In this section, we address possible sample selection bias through imputation and quantile regression. Since we do not know the final "highest degree" of those still enrolled, we impute wages only for individuals who are no longer enrolled but are unemployed.

First, among those who are not enrolled in 2009 and are missing wages because they are unemployed, some were not enrolled in 2005, and were employed in that year. For the students in that sample who did not obtain any further degrees from 2005 to 2009, wage in 2005 is used to predict wage in 2009. In order to account for wage growth over that period, we calculate the wage growth for everyone starting in associates degree (certificate) programs who was employed in 2005 and 2009, and did not obtain any degrees between 2005 and 2009. We then calculate the weighted average wage growth separately for those starting in for-profit, and not-for-profit/public institutions and use this average wage growth to predict 2009 wages for those who were employed in 2005, unemployed in 2009, and did not obtain any additional degrees between 2005 and 2009. We obtain the weighted averages

and medians using the sampling weights.[6]

We then follow Olivetti and Petrongolo (2008) and estimate the probability that each individual remaining without a wage, would have had a wage above the median. We have two observations for those who are missing wages: one is assigned a wage above the median, weighted by the probability that the wage is above the median, and the other is assigned a wage below the median, weighted by the probability that the wage is below the median. Since the observations already have sampling weights, we multiply the sampling weight by the probability that the wage is above (or below) the median. The sampling weight of the employed individuals remains unchanged.

Table 4 can be read as follows. The first column repeats coefficients from tables 2 and 3. The top panel gives the coefficients for those entering certificate programs and the bottom one for those entering associates degree programs. The second column shows the estimates when we conduct the imputation exercise described in this sub-section and estimate the income equations by quantile (median) regression.

We continue to find no evidence of a benefit from completing a certificate at either a for-profit or not-for-profit/public. The estimated return to an associates degree among those entering such programs is now slightly lower but remains statistically significant at the .05 level. The differential effect of obtaining the degree from a for-profit is small and insignificant but also sufficiently imprecise that we cannot reject that there is no return to an associates degree from such institutions. We remind the reader that in column 1, we are presenting results for OLS on the full sample, the technique/sample combination in the

[6]Throughout this section, we follow a parallel procedure for the estimates (not shown) using propensity-score weighting.

baseline estimates that showed the largest benefit to for-profit associates degrees. We also continue to find strong evidence of a benefit from earning a certificate or BA among those starting an associates degree at a not-for-profit or public but not at a for-profit institution.

5.1.2 Maximum Likelihood Sample Selection Correction

In this section, we correct for sample selection under the assumption that the error in the income and selection equations are joint normals (Gronau 1974, Heckman 1976, Heckman 1979). We present results using two different exclusion restrictions.

The first exclusion restriction makes use of discontinuities in GPA affecting academic standing and thus enrollment. We assume that GPA is a continuous predictor of ability and that ability affects wages in a continuous manner. Thus once we control for GPA, being in academic difficulty should not affect future income except through its effect on future enrollment. The power of this variable is limited. Leaving school in 2004 because of academic problems has a large positive effect on the probability of having a non-missing wage for the full sample starting in associates degree programs, but it is significant at only the .068 level. It has no predictive power for those entering a certificate program.

Our other exclusion restriction is whether the individual received aid from an employer in his or her first year of post-secondary education. Recall that we control for prior year income and therefore for job quality. This variable is a weak predictor of non-missing wages for the full sample of those starting in both certificate and associates degree programs. The coefficients suggest that those who received aid from their employer in 2003 are much more likely to have a non-missing wage in 2009 than those who do not. However, the coefficient for the associates sample has a p-value of only .078, and falls just short of significance at the

.1 level in the certificate sample.

With the strong caveat that the degree to which we are relying on parametric identification is unclear, the third and fourth (associates programs only) columns of table 4 present the result of the maximum likelihood sample selection. Both sets of results are largely unchanged except that when we use the second exclusion restriction, the results not only indicate a negative return to a BA from a for-profit but the difference in the return between institution-type is now statistically significant.

5.1.3 Health Majors

It is possible that differences in major across institution-types are driving the differences in return to degrees. Students starting in associates degree programs at public/not-for-profits enroll in a very diverse set of majors, including many liberal arts and sciences and general studies majors. However, students starting in associates degree programs at for-profits mainly enroll in vocational majors such as business, computer science, and health. The majors of those starting in certificate programs are generally similar across institution-types, except for a large number of students pursuing personal/culinary studies at for-profit institutions.

The largest major for those starting in certificate and associates degree programs, at both institution-types, is health. Thus, to cast light on whether our results might reflect differences in area of study across institution-type, in the fifth column of table 4, we restrict the sample to students majoring in health. Despite the smaller sample size (330 in certificate programs and 310 in associates programs), there are some striking results.

Among those starting in certificate programs, we observe a large and statistically sig-

nificant return to earning a certificate in health from a public or not-for-profit institution. In contrast, the point estimate for earning such a certificate from a for-profit is close to zero. Although the difference is not statistically significant for the sample and estimation technique shown here, it is significant at the .05 level when we restrict the sample to those at least age 24 (not shown) when they entered post-secondary education.

We also observe a noticeably (albeit not statistically significantly) larger return to an associates degree in health from a not-for-profit/public than we found for the whole sample both for those beginning in certificate (not shown) and associates degree programs. The results shown in the table are too imprecise to allow us either to reject that the return to an associates degree is similarly large for both types of institutions or to reject that it is nonexistent at for-profits. However, for some choices of estimation technique and sample, the estimated return is significantly (in both senses) lower at for-profits.

5.1.4 Time in Program and Time in Labor Market

Certificate programs, especially, may differ in length, and students might persevere longer in one type of institution than another. Therefore in the sixth column of table 4, we control for months enrolled. For both types of program, the coefficients on months enrolled (not shown) are small and insignificant, and the remaining coefficients are unaffected.

In order to determine whether the large, negative, statistically significant, coefficient on starting an associates degree at a for-profit is due to differences in human capital acquisition for drop-outs or differences in drop-outs across institution-types, we include total months enrolled, and total months enrolled interacted with starting at a for-profit. In this specification, the coefficient on starting at a for-profit is the effect of starting at a for-profit and not

spending any time enrolled. However, we continue to find the same coefficient on starting at a for-profit (not shown). To relax the linearity assumption of this test, we restrict the sample to those who have been enrolled for at most 6 months and estimate the main specification (without controlling for months enrolled). Again, we would expect not to find any human capital effects in this specification, and so finding a significant coefficient on starting at a for-profit would be indicative of differences in ability bias across institution-types. Here we find that the coefficient on starting at a for-profit is .014 and is not statistically significant (not shown; sample size here is 200), reassuring evidence that the differences-in-differences estimates are unbiased.

In the last column of the table we control for potential experience since leaving the program. To allow for the possibility that wage growth after leaving school is stronger for those starting at for-profits, we include both a linear term for months since last enrolled and its interaction with starting at a for-profit. The results for our key coefficients are again very similar to those in the original specification. However, we note the coefficient on the for-profit dummy falls to zero among those in associates degree programs. This is somewhat reassuring in that it supports the view that dropouts from for-profits and other institutions are similar once we control for observables. Although the difference is statistically insignificant, our point estimates suggest slower post-labor market entry wage growth for those entering associates degree programs at for-profits (not shown).

5.1.5 Labor Market Conditions

The differential returns might reflect differences in labor market strength in areas where for-profit and not-for-profit/public institutions are located. The distance between the re-

spondent's home and school is somewhat greater among those attending for-profits. The median distance between the respondent's permanent home and starting-institution is 18 miles for individuals starting in for-profit associates degree programs, and 10 miles for individuals starting in not-for-profit or public associates degree programs. The median distance between the respondent's permanent home and starting-institution is 15 miles for individuals starting in for-profit certificate programs, and 12 miles for those starting in not-for-profit or public certificate programs. While these differences are statistically significant, they suggest that for most students, the labor market where the post-secondary institution is located is a good proxy for the student's local labor market.

To investigate the possibility that for-profits are located in worse labor markets, we use the IPEDS public-use data to identify all institutions that primarily grant certificates and associates degrees. We are able to obtain the county name and state for each of these institutions. We then match these data with the average county-level unemployment rate in 2009, published by the Bureau of Labor Statistics. We find that the difference in the unemployment rate of counties where the for-profit institutions are located relative to counties where the not-for-profit or public institutions are located is just significant at the .1 level (p=.1). However, the unemployment rate in counties where for-profits are located is slightly *lower* than that in counties where not-for-profit/public institutions are located (9.1% v 9.3%). Thus, our results are not driven by for-profit institutions being located in particularly weak labor markets.

6 Conclusion

Much of the policy debate surrounding for-profit colleges has involved claims that for-profit colleges leave students with very high debt levels and poor employment outcomes. Basic summary statistics presented in this paper indeed show that post-college income is significantly lower for students starting at for-profit institutions. However, it is also very clear that those who start at for-profit institutions are less well prepared for college, had lower levels of pre-college academic performance, and face other significant obstacles to college and post-college success. This paper controls for these characteristics using both traditional OLS and propensity score methods. In addition, we use imputation methods and a sample selection model to address the various selection issues that are present in the data.

While high standard errors force us to be cautious, our results strongly suggest that, even after controlling for an extensive set of background variables, students at for-profit institutions do not benefit more and often benefit less from their education than apparently similar students at not-for-profit and public institutions. Certificates received by those starting in certificate programs provide little labor market benefit at either type of institution except that certificates in health fields appear to be valuable only if they are from not-for-profits and publics. Among those starting in associates degree programs, the return to a certificate or a BA is lower at for-profits. The return to an associates degree among this group is never statistically significant and is statistically significantly lower than the return to an associates degree from other institutions in health in some specifications.

The mechanism underlying these differential returns is unclear. One possibility is that students at not-for-profit and public institutions have access to better career offices at their

institution. However, the opposite appears to be true. Among people employed in 2009 and not employed in the same or similar job as when they were enrolled, 14% of those who started in for-profit associates degree programs compared with 8% of those starting in other institutions say the school helped them find their job. For those starting in certificate programs, the corresponding figures are 9% and 7%.

There are additional reasons for being cautious about our results. Our income data are from 2009, during an economic recession. If students starting at for-profit universities are more adversely affected in the labor market by the recession than those at not-for-profit or public institutions, we would expect to see lower returns to for-profit certificates and degrees. The lower returns to for-profit degrees could reflect that employers know graduates of for-profit institutions are of lower quality. Alternatively, lower for-profit returns could reflect that employers perceive graduates of for-profit universities to be lower quality when they are in fact equal in competence to those from not-for-profit or public universities. Since our data only allow us to observe wages at most a few years after completing a degree, we can only estimate short-run labor market effects.

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Table 1: Outcomes (2009) by Initial Institution Type

	(1)	(2)	(3)	(4)
	Start Certificate Program		Start Associates Program	
	For-Profit	Not-For-Profit/Public	For-Profit	Not-For-Profit/Public
Highest Degree				
Bachelors	0.001	0.017***	0.033	0.121***
	(0.001)	(0.006)	(0.018)	(0.006)
Associates	0.011	0.045***	0.278	0.166***
	(0.004)	(0.011)	(0.028)	(0.007)
Certificate	0.527	0.515	0.042	0.059
	(0.023)	(0.027)	(0.013)	(0.006)
Majored in				
Business	0.061	0.09	0.124	0.142
	(0.011)	(0.015)	(0.022)	(0.007)
Liberal Arts/Sciences	0.043	0.055	0.069	0.151***
	(0.009)	(0.012)	(0.018)	(0.008)
Health, Human Services, Education	0.519	0.399***	0.355	0.351
	(0.023)	(0.026)	(0.030)	(0.010)
Manual/Technological Vocational Training	0.113	0.226***	0.244	0.088***
	(0.017)	(0.025)	(0.026)	(0.005)
Undeclared	0.265	0.231	0.208	0.267**
	(0.020)	(0.022)	(0.023)	(0.009)
Months Since Last Enrolled, 2009	39.089	36.119*	33.534	23.343***
	(1.013)	(1.195)	(1.269)	(0.464)
Total Months Enrolled	19.236	21.138**	25.508	31.664***
	(0.460)	(0.669)	(1.101)	(0.363)
GPA 2003-2004	316.023	308.551	309.627	284.357***
	(3.674)	(5.100)	(5.634)	(1.822)
Parents' Ed $\leq$ 12	0.652	0.615	0.525	0.405***
	(0.022)	(0.027)	(0.031)	(0.010)
Expected Family Contribution, 2003 ($,000)	1.594	5.935***	3.264	8.074***
	(0.180)	(0.757)	(0.301)	(0.318)
Years Delayed Starting Post-Secondary Education	5.299	8.55***	4.776	4.334
	(0.365)	(0.459)	(0.387)	(0.188)
High School Diploma	0.699	0.76**	0.81	0.87**
	(0.021)	(0.023)	(0.026)	(0.007)
GED	0.195	0.175	0.162	0.084***
	(0.018)	(0.019)	(0.021)	(0.005)
English is Primary Language	0.826	0.882**	0.913	0.879**
	(0.016)	(0.017)	(0.016)	(0.007)
Number of Dependent Children, 2003	0.836	0.883	0.611	0.414***
	(0.052)	(0.081)	(0.059)	(0.021)
Dependent, 2003	0.395	0.353	0.451	0.654***
	(0.022)	(0.026)	(0.030)	(0.010)
Married, 2003	0.15	0.313***	0.144	0.146
	(0.016)	(0.026)	(0.019)	(0.008)

	(1)	(2)	(3)	(4)
	Start Certificate Program		Start Associates Program	
	For-Profit	Not-For-Profit/Public	For-Profit	Not-For-Profit/Public
Age, 2009	29.762	33.15***	28.555	27.919
	(0.369)	(0.483)	(0.395)	(0.192)
Male	0.248	0.427***	0.466	0.428
	(0.021)	(0.027)	(0.031)	(0.010)
Hispanic	0.317	0.166***	0.191	0.15*
	(0.021)	(0.022)	(0.024)	(0.008)
Black	0.316	0.192***	0.25	0.17***
	(0.021)	(0.022)	(0.029)	(0.007)
Income in 2002 ($,000)	21.303	37.24***	28.347	49.356***
	(0.958)	(2.983)	(1.317)	(1.124)
Born in the US	0.859	0.884	0.888	0.877
	(0.014)	(0.017)	(0.028)	(0.007)
Parents Born in the US	0.72	0.787**	0.776	0.764
	(0.019)	(0.024)	(0.030)	(0.009)
Household Size	3.258	3.39	3.114	3.627***
	(0.068)	(0.094)	(0.094)	(0.033)
Under-24-Only Regressors (all Exogenous)				
HS GPA > 3.0	0.732	0.847***	0.748	0.693**
	(0.020)	(0.018)	(0.025)	(0.009)
Took the SAT	0.361	0.55***	0.577	0.709***
	(0.027)	(0.039)	(0.037)	(0.010)
SAT/ACT score/100	8.164	8.738**	8.425	8.931**
	(0.161)	(0.189)	(0.208)	(0.046)
Parents' Income < 30,000	0.446	0.307***	0.365	0.254***
	(0.028)	(0.034)	(0.038)	(0.010)
Outcome Variable				
Income in 2009 ($,000)	26.356	31.889***	28.578	31.788**
	(0.854)	(1.521)	(1.256)	(0.589)

Standard deviations in parentheses under the means.

Stars next to values in Column 2 denote significant differences between Columns 1 and 2. Stars next to values in Column 4 denote significant differences between Columns 3 and 4.

Except for 2009 income, years delayed post-secondary education, parental education, and all of the under-24-only variables, there are 1190 observations that start a certificate program at a for-profit, and 860 observations that start a certificate program at a not for profit/public institution. There are 530 observations that start an Associates program at a for-profit, and 5210 observations that start an Associates program at a not-for-profit/public institution. Unweighted sample sizes do not fall below 10 in any of the cells. Observations are weighted by the sample weights from the survey. We have income data for 680 individuals starting a certificate at a for-profit, 550 at a not-for-profit/public. We have income data for 330 individuals starting an associates at a for-profit, 2830 at a not-for-profit/public.

*** p<0.01, ** p<0.05, * p<0.1

Table 2: Impact of For Profit Colleges on Log Income, 2009: Start in Certificate Program

	(1)	(2)	(3)	(4)
	Under 24	All Ages	Under 24	All Ages
	Basic Specification		Propensity Score Specification	
Start Program at:				
(1) For-Profit	-0.082	-0.037	0.102	-0.028
	[0.108]	[0.080]	[0.113]	[0.082]
Highest Degree:				
(2) Certificate	0.009	0.014	0.129	0.036
	[0.135]	[0.085]	[0.136]	[0.082]
(3) Associates	-0.263	-0.120	0.054	0.072
	[0.291]	[0.232]	[0.181]	[0.166]
(4) Bachelors	0.365*	0.450**	0.492**	0.624***
	[0.186]	[0.188]	[0.219]	[0.127]
Certificate from:				
(5) For-Profit	0.052	-0.007	-0.086	-0.030
	[0.162]	[0.106]	[0.167]	[0.110]
Associates from:				
(6) For-Profit	a	-0.292	a	-0.528**
		[0.271]		[0.211]
Combination				
(7) (2) + (5)	0.062	0.008	0.043	0.006
	[.082]	[.064]	[.093]	[.073]
Under-24-Only Variables	Y	N	Y	N
N	560	1,040	430	990

[a] denotes the coefficient was omitted to preserve the confidentiality of the few individuals obtaining those degrees.

The basic specification weights the observations by the sampling weights of the survey, while the Propensity Score specification weights the observations by propensity scores and sampling weights. Under-24 only variables are taking the SAT/ACT, SAT/ACT score, HS GPA above 3.0, and parental income less than $30,000. Explanatory variables are Expected Family Contribution to college (2003), Number of Dependent Children in 2003, years delayed enrollment into college, age, household size in 2003, income in 2002 (parental income for dependent students and respondent income for independent students) indicators for Black, Male, married, Hispanic, HS diploma, GED, dependent in 2003, highest level of parental education HS or less, English being the primary language, born in the US, major in 2003/2004 (undeclared [omitted category]; business; liberal arts and sciences; health, human services, and education; manual and technological vocational training) and parents born in the US. Degree institution types pertain to the highest degree obtained.

Table 3: Impact of For Profit Colleges on Log Income, 2009: Start in Associates Degree Program

	(1)	(2)	(3)	(4)
	Under 24	All Ages	Under 24	All Ages
	Basic Specification		Propensity Score Specification	
Start Program at:				
(1) For-Profit	-0.021	-0.152**	0.027	-0.088
	[0.052]	[0.074]	[0.062]	[0.068]
Highest Degree:				
(2) Certificate	0.316***	0.208**	0.351***	0.215**
	[0.113]	[0.092]	[0.113]	[0.092]
(3) Associates	0.128**	0.115***	0.142***	0.114**
	[0.051]	[0.043]	[0.051]	[0.046]
(4) Bachelors	0.223***	0.210***	0.234***	0.209***
	[0.050]	[0.048]	[0.053]	[0.051]
Certificate from:				
(5) For-Profit	-0.432***	-0.257*	-0.360**	-0.178
	[0.155]	[0.136]	[0.170]	[0.152]
Associates from:				
(6) For-Profit	-0.131	-0.024	-0.089	-0.051
	[0.092]	[0.097]	[0.098]	[0.102]
Bachelors from:				
(7) For-Profit	-0.318***	-0.143	-0.206	-0.121
	[0.118]	[0.125]	[0.127]	[0.117]
(8) Combination (3) + (6)	-0.003	0.092	0.053	0.063
	[.079]	[.086]	[.087]	[.092]
Under-24-Only Variables	Y	N	Y	N
N	2020	2640	1900	2520

[a] denotes the coefficient was omitted to preserve the confidentiality of the few individuals obtaining those degrees.

The basic specification weights the observations by the sampling weights of the survey, while the Propensity Score specification weights the observations by propensity scores and sampling weights. Explanatory variables are the same as those listed in Table 2. Under-24-Only variables are the same as those listed in Table 2. Degree institution types pertain to the highest degree obtained.

Table 4: Impact of For Profit Colleges on Log Income, 2009: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Principal Specification	Imputation	MLE Sample Selection Correction: Academic Standing	MLE Sample Selection Correction: Employer Aid	Health Majors	Months Enrolled	Months Since Last Enrolled
Panel A: Start in Certificate Program							
Start Program at:							
(1) For-Profit	-0.037	-0.083			0.055	-0.043	-0.100
	[0.080]	[0.092]			[0.116]	[0.079]	[0.175]
Highest Degree:							
(2) Certificate	0.014	-0.019			0.220**	0.008	0.023
Certificate from:	[0.085]	[0.088]			[0.103]	[0.084]	[0.086]
(3) For-Profit	-0.007	0.033			-0.198	0.004	-0.011
	[0.106]	[0.116]			[0.144]	[0.105]	[0.108]
(4) Combination (2) + (3)	0.008	0.014			0.022	0.011	0.012
	[.064]	[.076]			[.097]	[.064]	[.065]
N	1,040	1640			330	1,040	1,040
Panel B: Start in Associates Degree Program							
Start Program at:							
(5) For-Profit	-0.152**	-0.120**	-0.162**	-0.221***	-0.522*	-0.153**	0.004
	[0.074]	[0.060]	[0.078]	[0.077]	[0.298]	[0.074]	[0.126]
Highest Degree:							
(6) Certificate	0.208**	0.214*	0.193**	0.222**	-0.057	0.210**	0.218**
	[0.092]	[0.122]	[0.089]	[0.090]	[0.110]	[0.092]	[0.093]
(7) Associates	0.115***	0.090**	0.100**	0.131***	0.268***	0.125***	0.129***
	[0.043]	[0.044]	[0.044]	[0.042]	[0.092]	[0.045]	[0.043]
(8) Bachelors	0.210***	0.304***	0.187***	0.215***	0.098	0.228***	0.235***
	[0.048]	[0.060]	[0.050]	[0.044]	[0.140]	[0.054]	[0.050]
Certificate from:							
(9) For-Profit	-0.257*	-0.323	-0.230*	-0.329**	0.195	-0.257*	-0.284**
	[0.136]	[0.205]	[0.135]	[0.131]	[0.234]	[0.135]	[0.131]
Associates from:							
(10) For-Profit	-0.024	-0.004	-0.023	-0.074	0.015	-0.027	-0.041
	[0.097]	[0.111]	[0.097]	[0.085]	[0.405]	[0.097]	[0.098]
Bachelors from:							
(11) For-Profit	-0.143	-0.386	-0.153	-0.292***	a	-0.138	-0.219*
	[0.125]	[1.148]	[0.123]	[0.113]		[0.125]	[0.133]
(12) Combination (7) + (10)	0.092	0.085	0.077	0.057	0.283	0.098	0.088
	[.086]	[.102]	[.084]	[.075]	[.362]	[.087]	[.087]
N	2640	3910	5160	5160	440	2640	2640

a denotes the coefficient was omitted to preserve the confidentiality of the few individuals obtaining that degree.

Results are from the full sample; observations weighted by the sampling weights. Explanatory variables are listed in Table 2. Degree institution types pertain to the highest degree obtained. Column 1 presents the results from Tables 2 and 3. Column 2 contains results from median regressions, imputing for the unemployed. Observations in Column 2 also weighted by imputation weights as described in the paper with standard errors obtained by bootstrap. Columns 3 and 4 contain Maximum Likelihood Sample Selection results, in which non-missing wages are a function of the explanatory variables in Table 2, and whether the respondent received employer aid for college in 2003/2004 (Column 3) or 2003/2004 GPA and leaving school because of academic problems in 2004 (Column 4). GPA in 2003/2004 is included as an explanatory variable in Column 4. The first-stage coefficient on receiving employer aid is .213 (.121), and on leaving school because of academic problems it is .366 (.201). Column 5 restricts the sample to Health majors. Column 6 includes as an explanatory variable total months enrolled, while Column 7 includes months since last enrolled and this variable interacted with starting at a for-profit.