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Do Students Profit from For-Profit Education?
Estimating the Returns to Postsecondary Education with Tax Data

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Abstract: I use administrative panel data on earnings and educational attendance from the Internal Revenue Service to estimate the differential earnings effect of attending a not-for-profit postsecondary institution, relative to a for-profit institution. The results suggest that there is a substantial differential increase in earnings after enrollment, relative to years prior to enrollment, for individuals who attend not-for-profit institutions, compared to individuals who attend for-profit schools. The results also suggest that earnings decrease during enrollment, compared to years before enrollment, but that this decline is relatively smaller for individuals who attend not-for-profit institutions. Given that for-profit institutions cost more on average than not-for-profit institutions, these results imply that students may realize better long-run financial outcomes from attending not-for-profits institutions.

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*This paper represents my own research efforts and does not reflect the views or opinions of the U.S. Treasury.

I. Introduction

The Higher Education Act requires that for-profit institutions and career-focused education at not-for-profit institutions prepare students for gainful employment in a recognized career to be eligible for Title IV federal student aid.[1] However, the United States Department of Education has not enforced this provision, known as the "gainful employment" clause. Recently, the Department of Education issued regulations to implement the gainful employment provision. The new regulations may restrict federal student aid by students at for-profits schools and career-focused not-for-profit institutions beginning in 2015, based in part on the labor market outcomes of former students. The stakes of losing federal student aid are large for the for-profit education sector. In fiscal year 2009, Title IV federal aid[2] as a share of total institutional revenue, which by law cannot exceed 90 percent, was 77 percent nationally among for-profit schools in the U.S. (U.S. Department of Education 2011c). In addition, the Department of Defense is considering rule changes for G.I. education benefits based on graduation rates (Lipton 2010), which may further restrict federal support for for-profit institutions. The availability of Title IV student aid and military education benefits enable for-profit institutions to realize billions of dollars in profits (U.S. Senate Health, Education, Labor and Pensions Committee 2010).

This paper explores the underlying basis of the gainful employment rule by quantifying the differential earnings effect of attending a for-profit institution relative to a not-for-profit institution. I estimate the individual return to additional years of postsecondary attendance for 1,996 individuals who enroll as undergraduates at two-year or four-year schools sometime between 2002 and 2006. Making use of several Internal Revenue Service (IRS) tax forms,

[1] Public Law No: 111-152 SEC. 481. [20 U.S.C. 1088] (b) (A) 1.

[2] Title IV Federal Student Aid includes Pell Grants, Supplemental Educational Opportunity Grants, Academic Competitiveness Grants, National Science and Mathematics Access to Retain Talent ("SMART") Grants, Teacher Education Assistance for College and Higher Education ("TEACH") Grants, Leveraging Educational Assistance Partnership ("LEAP") Grants, Direct Student Loans, Perkins Loans and Work-Study Programs.

including the 1040, the W2, Schedule C, and the 1098-T, I construct a panel dataset of earnings and postsecondary attendance for primary and secondary taxpayers. I merge these data with information from the Integrated Postsecondary Education Data System from the U.S. Department of Education to identify the institutional control of the school attended. To estimate the differential effect of postsecondary attendance on earnings, I compare the earnings of individuals after attendance, relative to before, across for-profit and not-for-profit schools. I include individual fixed effects to control for unobserved differences across individuals that may affect both education and earnings.

Despite a rich literature examining the individual return to education (Kling 2000; Jacobson, LaLonde and Sullivan 2005a; Kane and Rouse 1995, 1999; Grubb 1993; Marcotte, Bailey, Borkoski and Kienzl 2005; Leigh and Gill 1997; Card 1999), there is a dearth of evidence on the return to *for-profit education*. In the only other work to consider the labor market outcomes of for-profit students, Cellini and Chaudhary (2011) find that there is no significant differential earnings effect for students who attended for-profit two-year schools relative to students who attended public two-year schools. I build on this study in several ways. First, I analyze the earnings effect for a broader range of undergraduate schools, including two-year schools and four-year schools. Second, I estimate the earnings effect for a relatively older group of students compared to Cellini and Chaudhary (2011), which may better characterize the population of individuals who attend for-profit institutions.

The results suggest that there is a positive earnings differential for individuals who attend a not-for-profit school, compared to individuals who attend a for-profit school, in the years after attendance, relative to earnings prior to enrollment. The differential effect of attending not-for-profit school, relative to a for-profit school, is both meaningful (8 percentage points) and

significant. The results suggest that the return to for-profit education is small and negative (2-3 percent), although this effect is not significantly different than zero. The results also suggest that earnings substantively decrease (19 percent) during periods of enrollment, compared to earnings prior to attendance. However, the differential earnings decline during enrollment is roughly 12 percentage points smaller for individuals who attend not-for-profit schools.

A key weakness of the empirical approach is that students may sort into for-profit and not-for-profit schools based on unobserved factors that impact earnings, but that are not captured by individual level fixed effects. For example, it is possible that students of higher ability, who may be better able to capitalize on postsecondary education in the labor market, are more likely to enroll in not-for-profit institutions that are generally more selective compared to for-profit schools. While I find evidence that the results hold after accounting for admissions selectivity, it is still possible that there is selection across not-for-profit and for-profit schools within similar admissions groups. As a result, the differential earnings effect of attending a not-for-profit institution may represent both the causal effect of not-for-profit attendance on earnings as well as selection effects that arise from the decision to attend a not-for-profit school.

The rest of this paper proceeds as follows. In Section II, I provide background information on the postsecondary education market in the U.S., with attention to the recent growth in for-profit institutions. In Section III, I describe the econometric technique that I use to estimate the individual return to postsecondary education. I discuss the empirical results in Section IV, and in Section V, I conclude.

II. Federal Student Aid, For-Profit Higher Education and the Gainful Employment Rule

Federal and state student aid programs totaled over $117 billion for the 2008-09 school year (Baum, Payea and Cardenas-Eliott 2010). There are two primary justifications for this level of public support for higher education. One, education is a public good. Beyond contributing to greater economic growth (Topel 2005; Glaser, Scheinkman and Shleifer 1995), there is evidence that higher education offers important social benefits such as increased civic participation (Dee 2004; Milligan and Moretti 2004), improvements in infant health (Currie and Moretti 2003) and positive spillover effects for less skilled employees (Moretti 2003). Two, individuals may face credit constraints that prevent enrollment. Lochner and Monge-Naranjo (2008), Ellwood and Kane (2000) and Kane (1995, 1994) argue that credit constraints may impede higher education enrollment for some students.[3] Absent federal student aid, individuals are likely to select a lower level of education, which may limit both social and private benefits.

Traditionally, public support for higher education in the United States flowed almost exclusively to public and private postsecondary institutions. As a result of rapid growth by for-profit institutions, students at for-profit schools now receive a large share of federal student aid. Between the 1997-98 and the 2009-10 school years, the number of for-profit institutions nearly doubled, increasing from 672 to 1,199.[4] During this same period, enrollment at for-profit institutions grew more than four-fold, increasing from just over two percent of total national enrollment to over nine percent.[5] Yet, in the 2008-09 school year, 24 percent of Pell Grants were awarded to students at for-profit schools (Lynch, Engle and Cruz 2010) and in the first year of

[3] The role of credit constraints on postsecondary education is not resolved in the literature. Nielsen, Sorensen and Taber (2008), Cameron and Taber (2004), Carneiro and Heckman (2002), Cameron and Heckman (2001, 1999), and Keane and Wolpin (2001) provide evidence that short-term credit constraints are unimportant. However, these studies analyze credit constraints in the presence of existing federal student aid programs, and this implication may not hold in the absence of federal aid programs.

[4] Data from the 2010 Digest of Education Statistics Table 275.

[5] Data from the 2010 Digest of Education Statistics Table 197.

the Post 9/11 G.I. Bill, more than 36 percent of tuition payments went to for-profit schools (Lipton 2010). In the 2008-09 school year, students at for-profit institutions received a total of $24 billion (23 percent) of Title IV federal student aid (U.S. GAO October 2010). Federal aid receipt allows for-profit schools to realize large profits. The eight largest publicly traded and the eight largest privately held for-profit education companies earned a total of 2.7 billion dollars in 2009 (U.S. Senate Health, Education, Labor and Pensions Committee 2010). The U.S. GAO (October 2010) reports that the market capitalization of the fourteen publicly traded for-profit education corporations was $26 billion in July 2010.

Rapid growth in for-profit enrollment and the relatively large share of federal aid received by students at for-profit schools has caused alarm among some policymakers. These policymakers cite allegations of corruption in recruiting practices (U.S. GAO August 2010) fraudulent use of federal aid (U.S. GAO August 2009), low graduation rates at for-profit institutions (Lynch, Engle and Cruz 2010), and devotion of excessive resources to recruitment rather than to education or to job-placement services (Lewin 2011) as prime examples of why for-profit education should not receive public support. Further, critics of for-profit education suggest that for-profit colleges are not providing an education that is even *privately beneficial*, citing high student loan default rates (Cunningham and Kienzl 2011) and an inability to gain employment after graduation (Lipton 2010). In contrast, supporters of for-profit education suggest that the profit motive results in schools that are more responsive to student needs, for example by insuring that students are never closed out of courses (Wilson 2010), and by effectively helping students navigate a difficult student aid process (Scott-Clayton 2011).

Amid this debate over the efficacy of for-profit institutions, the Department of Education issued new regulations to interpret the gainful employment clause. The new regulations may

restrict the use of federal student aid at roughly 4,500 postsecondary institutions, over 40 percent of which are for-profit.[6] At non-profit schools, only non-degree programs are subject to the gainful employment rule. In contrast, at for-profit schools, all programs are subject to the gainful employment rule except for liberal arts baccalaureate degree programs. The gainful employment rule will deny Title IV federal student aid when institutions fail to meet the following three conditions three times in a four year period: one, at least 35 percent of former students are in active student loan repayment; two, student loan payments are less than 30 percent of discretionary income; three, student loan payments are less than 12 percent of total income.[7] Analyzing institutions with regulated programs, the Department of Education (2011a) estimates 84 percent of all programs restricted from awarding federal aid under the gainful employment rule will be for-profit institutions and that 5 percent of all for-profit programs will lose eligibility.[8]

One reason that the gainful employment rule may have a large impact on for-profit schools is because the cost of postsecondary attendance is higher at for-profit institutions compared to public schools. Panel A of Table 1 shows tuition and fees and room and board charges for the 2007-08 school year. Room and board charges are highest at for-profit institutions among both four-year and two-year schools. Assuming that students have perfect

[6] Among regulated programs, the U.S. Department of Education identifies 42.3 percent as for-profit, 37.3 percent as public and 20.4 percent as not-for-profit private (U.S. Department of Education 2011a).

[7] After failing to meet all three conditions one time, schools must disclose this failure to students. After failing to meet all three conditions a second time, schools must warn students about excessive debt and possible school closure. Discretionary income is defined as the difference between the mean (or median) annual earnings and 150 percent of the most current poverty guideline for a single person in the continental U.S. (U.S. Department of Education 2011a).

[8] By comparison, the Department of Education (2011a) estimates that 14 percent of restricted programs will be public schools and 2 percent will be non-profit private schools and that only 1 percent of each of these school types will lose eligibility for Title IV aid (The U.S. Department of Education (2011a, Table 9a).

information on these costs, differences in the cost of postsecondary attendance should be reflected in future earnings.[9]

Table 1: College Costs and Student Loan Details

	Four-year Schools			Two-year Schools	
	For-Profit	Public	Not-for-Profit Private	For-Profit	Public
Panel A: Costs of Postsecondary Education, 2007-2008					
Tuition & Fees	15,226	5,943	23,328	13,363	2,061
Room & Board	10,439	7,485	8,722	10,159	4,915
Panel B: Share of Students using Student Loans, 2007-2008					
Federal Stafford	94	42	54	95	11
Private	46	14	25	42	5
Panel C: Distribution of Student Debt Among Borrowers, 2007-2008					
Less than $10,000	4	25	14	22	61
$10,000 to $19,999	13	31	27	35	25
$20,000 to $29,999	24	23	24	24	8
$30,000 or more	60	20	36	19	5
Median	32,653	17,700	22,375	18,783	7,125
Panel D: Distribution of Loan Repayment Status, 2005					
Not Delinquent	47	66	72	37	40
Delinquent	29	24	20	27	36
Default	24	10	8	36	24

Note: Less than 5 percent all not-for-profit two-year schools are private, so I do not separately consider this category.
Sources: Tuition & Fees, Room & Board from Digest of Education Statistics 2010 Table 345. Student loan use from Lynch, Engle and Cruz (2010), Table 5. Distribution of student debt from Baum and Steele (2009). Student loan repayment status from Cunningham and Kienzl (2011).

Students at for-profit colleges are more likely to take out loans to finance their education, incur larger amounts of student debt, and are more likely to default on their loans, compared to students at comparable public and private institutions. As shown in Panel B of Table 1, in the 2007-2008 school year, nearly all students at for-profit colleges received federal student loans. Students at for-profit colleges are also more likely to use private student loans, which generally have higher interest rates relative to federal loans. Panel C shows the distribution of student debt among borrowers. The majority of borrowers at four-year for-profit colleges incur student debt of at least $30,000. By comparison, only 20 percent of borrowers at four-year public schools and

[9] To the extent that student value education as consumption, differences in costs could also be reflected in attributes of the institution that students value.

36 percent of borrowers at four-year not-for-profit private schools have this level of student debt. Among two-year schools, roughly one in five borrowers who attend for-profit schools has debt of at least $30,000, compared to roughly one in twenty at not-for-profit schools.

Panel D shows student loan repayment status in 2005. Among four-year schools, students at for-profit colleges are between two and three times more likely to be in default, compared to students from public or private institutions. Among two-year schools, for-profit students are roughly 50 percent more likely to be in default, compared to students at public schools. In FY2009, individuals who attended for-profit institutions accounted for 47 percent of all student loan defaults (U.S. Department of Education 2011d). When individuals default on their federal student loans, the government covers the cost. In 2009, the federal government incurred $9.2 billion in student loan defaults (U.S. Department of Education 2011b). Defaulting on student loans also involves substantial costs for the former student, including damage to credit scores and the denial of certain occupational licenses (U.S. Department of Education 2011b).

Another criticism leveled against for-profit schools is that they fail to graduate their students at a rate comparable to public or private institutions (U.S. Senate Health, Education, Labor and Pensions Committee 2010). Students who enroll at four-year for-profit colleges are less likely to graduate within six years, compared to students at public or private schools. Panel A of Table 2 shows graduation rates by the first year of enrollment. In contrast, students enrolled at two-year for-profit schools are more likely to graduate within three years, compared to students at two-year public colleges. This pattern holds across race/ethnicity groups and student gender, as shown in Panel B for the students enrolling in 2002.

Table 2: Rates of First-time Degree-Seeking Undergraduate Students Graduating within 150 percent of Normal Time

	Four-year Schools			Two-year Schools	
	For-Profit	Public	Private	For-Profit	Public
Panel A: Graduation Rates by Student Cohort					
1999	29.1	54.1	64.0	61.0	22.9
2000	32.6	54.8	64.5	59.1	23.6
2001	24.5	55.0	64.4	58.7	22.9
2002	22.0	54.9	64.6	57.1	21.9
Panel B: Graduation by Student Demographics, 2002 Cohort					
White	25.5	57.4	67.2	61.0	24.5
Black	16.3	39.4	44.9	49.3	13.2
Hispanic	27.5	46.3	59.5	59.7	16.7
Male	23.6	51.7	61.9	56.6	20.9
Female	20.5	57.5	66.7	57.4	22.8

Source: Digest of Education Statistics 2010 Table 341.

III. Econometric Method

A. Analysis Sample

To estimate the effect of postsecondary attendance on earnings, I use administrative data on educational attainment and income from the Internal Revenue Service (IRS). Administrative data offer important advantages over survey data that is traditionally used to estimate the returns to education (Cellini and Chaudhary 2011, Kane and Rouse 1995a, Card 1995, Monk-Turner 1994, Angrist and Krueger 1991, Grubb 1993, Leigh and Gill 1997). Previous research finds measurement error in educational attainment (Kane, Rouse and Staiger 1999) and earnings (Bound, Brown and Mathiowetz 2000, Pischke 1995) in survey data, which may bias the estimated return to education. Such measurement error is especially problematic in the presence of fixed effects (Griliches and Hausman 1986).

I analyze data from a sample of individual income tax returns included in the Continuous Work History Sample (CWHS) from 1999-2008. Selection into the CWHS panel is based on the last four digits of the primary taxpayer's Social Security number. Taxpayers with the selected Social Security number ending are included in each year of the sample that they file a tax return

as a primary taxpayer. During the sample period, five social security endings are included so that the CWHS is a 1 in 2,000 random sample of tax returns. In order to separately identify the earnings of individuals who are part of joint returns, I merge data from the 1040 tax form with wage data from the W2 form and self-employment income from Schedule C. The resulting data are unique at the individual-year level.

I use IRS form 1098-T to determine which taxpayers enroll in a postsecondary institution during the sample period. Institutions of higher learning are required to send a 1098-T form to nearly all students, and to the IRS.[10] The 1098-T includes information on the type of enrollment (undergraduate or graduate) and enrollment intensity (part-time or full-time). The 1098-T form also includes the employer identification number of the institution of higher learning. To identify institutional characteristics, including institutional control, I merge the tax data with data from the Department of Education's Integrated Postsecondary Education Data System (IPEDS) by the employer identification number.[11] This identification number is unique at the system level. For example, all University of California schools (UC Berkeley, UCSD, UCLA, etc.) have the same employer identification number. While the merged data provide detail on institutional characteristics, such as admissions selectivity and institutional control, the data are not able to identify the course of study or the normal time to degree for the program in which the student is enrolled.

[10] All schools eligible for Title IV federal student aid programs are required to file a 1098-T for most students who are enrolled. Exceptions to this rule include: courses for which no credit is earned; nonresident alien students; and students whose qualified tuition is covered by a formal billing arrangement between the institution and the student's employer.

[11] I include any school that has an employer identification number in the "directory information" dataset between 2000 and 2006. The match rate between the CWHS and the IPEDS using the employer identification number (EIN) is about 90 percent. Of the non-matching records, roughly 20 percent have invalid EINs (negative, missing or insufficient digits). Completion of IPEDS surveys is mandatory for schools that participate in Title IV federal student financial aid programs, so that the matched records should reflect the set of schools eligible for federal aid programs. More than 6,700 institutions complete IPEDS surveys each year. (http://nces.ed.gov/ipeds/about/)

After merging the CWHS and the IPEDS data, I limit the sample in several important ways. First, due to sample size considerations, I include only individuals who attend a two-year or four-year postsecondary institution as an undergraduate. This removes graduate students and students who attend a less than two-year school. Second, I limit the sample to individuals aged 18-55 in their first year in the data. A small number of students are outside this age range or have invalid age information.[12] Third, I limit the sample to individuals who have at least a three-year history of earnings prior to the initial year of enrollment in the data and at least a two-year history of post-enrollment earnings. The inclusion of individual fixed effects requires that an individual has multiple years of earnings. I require several years of earnings history in order to mitigate the effect of idiosyncratic earnings shocks in the pre- or post-enrollment periods.

I also use a relatively longer history of pre-enrollment earnings to reduce the likelihood that the initial year of enrollment observed in the data is part of an interrupted spell of postsecondary enrollment. Due to the requirements on pre- and post-enrollment spells that I impose, individuals cannot be enrolled in the years 1999-2001, or during the years 2007-2008. Panels A and B of Table 3 show the counts of individuals in the sample based on the first and last year in the data and the first and last year of enrollment. Panel C shows the average number of years that individuals are observed prior to enrollment, during enrollment, and after enrollment. On average, individuals have 3.9 years of earnings prior to enrollment, are in school for 1.6 years, and have 3.5 years of post-enrollment earnings. These durations are not significantly or substantively different across for-profit and not-for-profit schools among two-

[12] Roughly 3 percent of observations have invalid age information. After removing these observations, roughly 4 percent are outside of the 18-55 age range.

year or four-year institutions. Nearly all individuals (95.3 percent) have uninterrupted spells of enrollment during the sample period.[13]

Table 3: Number of Individuals by Years in Sample and Years of Enrollment and Duration in Sample

Panel A: First Year in Sample and First Year of Enrollment

	First Year Enrolled					
First Year In Sample	2002	2003	2004	2005	2006	Total
1999	501	438	286	207	141	1,573
2000	72	68	33	21	16	210
2001	0	48	35	29	9	121
2002	0	0	26	16	15	57
2003	0	0	0	20	15	35
Total	573	554	380	293	196	1,996

Panel B: Last Year in Sample and Last Year of Enrollment

	Last Year Enrolled					
Last Year In Sample	2002	2003	2004	2005	2006	Total
2004	9	0	0	0	0	9
2005	14	13	0	0	0	27
2006	5	11	19	0	0	35
2007	15	25	31	41	0	112
2008	260	357	368	408	420	1,813
Total	303	406	418	449	420	1,996

Panel C: Average Number of Years in Sample

		Four-year Schools		Two-year Schools	
School	All	For-Profit	Not-for-Profit	For-Profit	Not-for-Profit
Prior to Enrollment	3.9	4.0	3.9	3.9	3.8
Enrollment	1.6	1.5	1.5	1.7	1.7
After Enrollment	3.5	3.3	3.6	3.1	3.5
Total Years	9.1	8.8	9.1	8.7	9.1

Source: Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System

The resulting sample includes 18,094 observations at the individual-year level, comprised of 1,996 individuals who attend 934 schools. Table 4 shows descriptive statistics of the analysis sample, broken out by institutional control. The top panel shows average annual earnings, defined as wage income plus self-reported income, during different periods in the sample. Earnings of individuals who attend a not-for-profit institution are larger compared to individuals who attend a for-profit school in the years following enrollment. There are significant and

[13] Among individuals with interrupted spells during the analysis period, roughly 3.3 percent are not enrolled for one year, 1.2 percent are not enrolled for two consecutive years and 0.2 percent are not enrolled for three consecutive years in between years of enrollment.

meaningful differences in earnings in the years prior to enrollment across for-profit and not-for-profit schools. A key identifying assumption of the identification is that individuals who attend a for-profit school have a similar *earnings trend* in the pre-enrollment years, compared to individuals who attend a not-for-profit school. I explore this assumption in the robustness checks section.

Table 4: Characteristics of Analysis Sample, by School Type

	(1) All	(2) For-Profit	(3) Not-for-Profit	(4) |t-stat| (2)=(3)
Earnings				
All Periods	35,564	30,019	37,321	9.1
Pre-Enrollment	34,577	29,368	34,942	3.0
Post-Enrollment	38,466	31,151	39,427	6.7
During Enrollment	32,050	26,591	33,830	3.8
Student Characteristics				
Part-time	0.43	0.75	0.41	11.6
Full-time	0.57	0.25	0.59	6.6
Age	37.0	35.9	37.3	3.4
Age First Enrolled	32.7	31.6	32.1	3.8
Enrolled Part-time	0.30	0.30	0.30	0.2
Other Income	7,215	4,249	8,367	2.6
Joint	0.50	0.34	0.52	15.1
Head of Household	0.17	0.27	0.16	5.8
Single	0.33	0.39	0.32	6.1
School Characteristics				
Two-year	0.69	0.50	0.71	18.4
Four-year	0.31	0.50	0.29	18.3
Open Admissions	0.71	0.47	0.75	25.9
Standardized Test Score (if non-open admissions)	0.52	0.13	0.62	29.0
Observations	18,094	1,789	16,305	
Individuals	1,996	204	1,792	
Schools	934	111	823	

Note: Standardized test score equal to one for schools that require or recommend standardized test scores as part of the application and the school is not open admissions.
Source: Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System All dollar values in 2008 dollars.

The average age during the first year of enrollment for students in the sample (33) is older relative to traditional college age students (18-24). This older sample of students may better reflect the population on the margin of attending for-profit institutions. Nationally, in the

2003-04 school year, 69 percent of students enrolled at four-year for-profit institutions were age 25 or older, and 46 percent of students enrolled at two-year for-profits schools were in this age range.[14] Focusing on an older group of students is preferred given the fixed effects specification. Unlike the younger sample that Cellini and Chaudhary (2011) analyze (average age of 19-20 during the first year of enrollment), the sample that I consider is less likely to include earnings of high-school students. Earnings of high-school students, which may be attributable to part-time work, may not accurately capture the counterfactual level of earnings that an individual would have had absent postsecondary attendance.

The bottom panel of Table 4 shows the distribution of enrollment by school type and characteristics of schools' admissions policies. Across all students, nearly 70 percent enroll in a two-year school, while 30 percent enroll in a four-year school. However, sample splits based on two-year and four-year schools are likely to be a rough proxy the normal time required for degree completion. Data from the National Postsecondary Student Aid Study suggests that many students at four-year for-profit colleges pursue two-year degrees, while this occurs relatively infrequently at not-for-profit schools (see Appendix table A2). Unfortunately, there is no information on program type in the tax data so that the relative shares of two-year or four-year enrollments pursuing two-year or four-year degrees are not known. The majority of individuals enroll in open admissions schools, though the relative share is lower at for-profit schools. Conditional on attending a non-open admissions school, roughly half of all individuals attend an institution that requires standardized tests scores in the admissions process. However, this share varies across for-profit and not-for-profit schools types. Only 13 percent of non-open admissions for-profit schools require standardized test scores, compared to 62 percent of non-open

[14] In that same school year, the share of students 25 and older was 32 percent at four-year public schools, 40 percent at four-year private schools, and 44 percent at public two-year schools Age distributions from the Digest of Education Statistics 2005, Table 174 and 2009 Table 193. Shares exclude students with unknown ages.

admissions not-for-profit schools. I present results for sample splits based on these broad measures of admissions characteristics, to insure that the results are driven by comparisons of institutions of similar quality.

B. Specification

A key concern of empirical estimates of the return to education is properly controlling for factors that impact the years of educational attainment and/or the quality of education as well as earnings. The correlation between education and earnings will differ from the true causal effect if unobserved characteristics of the individual are correlated with both education and earnings. Many authors use instrumental variables to address this issue, relying on instruments that impact schooling choices but that are uncorrelated with unobserved factors that affect earnings. Common instruments are tuition at state colleges (Kane and Rouse 1995; Rouse 1995), the distance to a nearby college (Kane and Rouse 1995; Card 1995; Rouse 1995), interactions of college proximity and parental education (Card 1995) and compulsory schooling laws (Angrist and Krueger 1991).[15] Another approach found in the literature is to include individual level fixed effects to control for unobserved characteristics that may affect both education and earnings.[16]

I follow this second approach, estimating the differential effect of attending a public or private institution, compared to a for-profit institution, in a model with individual fixed effects. Equation (1) shows the specification

[15] Angrist and Krueger (1991) use quarter of birth interacted with year (and state) of birth. In later work, Bound, Jaeger and Baker (1995) and Staiger and Sotck (1997) show that this approach is likely to be asymptotically biased because it relies on a large number of relatively weak instruments.

[16] Card (1999) discusses two additional approaches, including models for siblings and twins, and the use of parental education to directly control for student ability.

(1)

$$y_{it} = \beta_1 Post_{it} + \beta_2 Post_{it} * Not\text{-}for\text{-}Profit_i + \beta_3 Enrolled_{it} + \beta_4 Enrolled_{it} * Not\text{-}for\text{-}Profit_i + \beta_5 X_{it} + \alpha_i + \varepsilon_{it}$$

where *i* and *t* index individuals and time. The dependent variable in Equation (1) is the natural log of annual earnings (in $2008), defined as wages plus self-employment income. (As a robustness check, I also consider the natural log of wages.) The key independent variables are *Post* and *Post* interacted with an indicator variable that is equal to one for students who attend a not-for-profit school (*Not-for-Profit*). The *Post* variable is equal to one in the year following the last year of attendance and for each subsequent year. This variable captures the return to attending a for-profit institution. The interactions of *Post* with *Not-for-Profit* give the differential effect of attending a not-for-profit institution, relative to a for-profit institution, after enrollment, relative to before. The *Not-for-Profit* variable is determined by the school attended during the last year of enrollment observed in the data. In the analysis sample nearly all students enroll in the same type of institution during both their first and last years of enrollment.[17] To account for the likelihood that earnings during enrollment are likely to be lower compared with pre-enrollment or post-enrollment earnings, I include an indicator variable (*Enrolled*) that is equal to one during the years of enrollment. To allow for differential earnings effects during enrollment, I include interactions of the *Enrolled* variable with the *Not-for-Profit* variable. To further address earnings during enrollment, I also estimate a specification that removes the years of enrollment, the *Enrolled* variable and interactions with the *Enrolled* variable.[18]

[17] The results are similar if I use the institution attended in the first year of enrollment. See Appendix Table A1 for student counts based on the last year and first year of attendance across school types.

[18] For students with interrupted spells of enrollment, the years between enrollments are not included in the post-enrollment period. The results are similar if I remove all students with interrupted spells of enrollment during the analysis period.

In *X*, I control for a number of time-varying characteristics that may affect earnings. I control (linearly) for unearned income, which includes capital gains, interest income, dividend income, alimony received, IRA and pension distributions and rental income. I include the age and age squared of the individual to capture life cycle earnings effects, although the results are robust to including age fixed effects. I include year indicator variables to account for secular changes in earnings. I cluster the standard errors at the tax return level.[19]

By including individual fixed effects in Equation (1), given by α, I insure that identification is driven solely by the comparison of each individual's earnings after exiting a postsecondary institution, relative to before. This approach will produce biased estimates if there is selection into school types in a way that is correlated with earnings. As discussed in the robustness checks section, I find evidence that there is not a substantive decrease in earnings in the year preceding enrollment, compared to earlier years. This suggests that there is no differential sorting into school type based on experiencing a bad earnings shock in the year before enrollment. However, there may still be selection into school types based on student characteristics. Unfortunately, the tax data that I analyze have limited information on student characteristics including race and ethnicity. If taxpayers sort into school types based on these characteristics and if these characteristics affect earnings, then the estimated effect of earnings across school types will also include this sorting effect. If this sorting effect is observed in both pre- and post-enrollment earnings, it will be captured by individual level fixed effects. However, it is possible that higher ability students, who are more likely to realize a larger benefit from education, are more likely to enroll in more selective schools that are largely not-for-profit. To the extent that this sorting exists, the results may overstate the differential return to not-for-profit

[19] Relatively few tax returns have multiple individuals enrolled in the data. The results are similar if I instead cluster on individual (N=1,996) rather than on the tax return (N=1,990).

attendance. As a check of this source of bias, I limit the sample of schools based on school admissions characteristics in an attempt to compare students to schools of similar selectivity.

IV. Empirical Results

A. Baseline Results

Table 5 presents the baseline results.[20] Panel A shows the results for the specification that includes the years of enrollment, and Panel B shows the results for the specification that removes the years of enrollment from the sample. The results for the entire sample are in Column (1). In this case, the differential effect of attending a not-for-profit school, relative to a for-profit school, after enrollment, relative to before, is roughly 8 percentage points in both Panels A and B.[21] The estimated effect of attending a for-profit school, given by the coefficient on the *Post* variable, is negative in both Panels A and B for the entire sample in Column (1), although this effect is imprecisely estimated. As a result, the earnings effect of attending a for-profit school is unclear. Calculating the total effect of attending a not-for-profit institution as the sum of the estimated effect of attending a for-profit institution plus the estimated differential effect suggests that individuals who attend a not-for-profit school increase their earnings by 5-6 percent. On average, students in the sample enroll for roughly two years,[22] so that these effects imply an earnings increase of 2.5-3 percent for each year of enrollment. These effects are lower than results from Kane and Rouse (1995a), Monk-Turner (1994) Card (1995), Cellini and Chaudhary (2011), Kane and Rouse (1995a), and Monk-Turner (1994) who find that postsecondary

[20] I show the results from an OLS specification that removes the individual level fixed effects in the Appendix.
[21] In Table 5, the *Not-for-Profit* group consists of both public and not-for-profit private schools. When I include separate interactions for each of these school types, I find positive and significant differential effects. The estimates suggest a slightly larger differential effect for not-for-profit private enrollment compared to public school enrollment, although I cannot rule out equal differential effects for these two school types. I show these results for the entire sample and for four-year schools in the Appendix.
[22] The distribution of years of enrollment is: 1 year, 62 percent; 2 years, 20 percent; 3 years, 10 percent; 4 or more years 8 percent.

enrollment increases earning by 4-10 percent per year of attendance. However the total effect for individuals who attend not-for-profit schools includes the impact of the *Post* variable that is imprecisely estimated. Assuming that the estimated effect of *Post* is zero, I cannot rule out a total earnings effect for individuals who enroll in not-for-profit schools that is comparable to previous estimates.

Table 5: Baseline Results

School Type	(1) All	(2) Two-year	(3) Four-year	(4) Two-year Not-for-Profit and All For-Profit
Not-for-Profit*Post	0.077**	0.031	0.152**	0.065
	[0.036]	[0.063]	[0.070]	[0.046]
Post	-0.020	0.024	-0.082	0.008
	[0.050]	[0.068]	[0.078]	[0.050]
Enrolled	-0.188***	-0.240***	-0.156**	-0.177***
	[0.049]	[0.078]	[0.061]	[0.049]
Enrolled*Not-for-Profit	0.119**	0.188**	0.052	0.118
	[0.049]	[0.077]	[0.066]	[0.049]
Observations	18,094	12,458	5,636	13,354
Individuals	1,996	1,370	626	1,473
Not-for-Profit*Post	0.077*	0.032	0.150**	0.068
	[0.045]	[0.066]	[0.072]	[0.047]
Post	-0.032	0.019	-0.106	-0.001
	[0.049]	[0.066]	[0.080]	[0.050]
Observations	14,981	10,433	4,548	11,152
Individuals	1,996	1,370	626	1,473

Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

The estimated effect of the *Enrolled* variable suggests that earnings decrease during enrollment, compared to earnings in the years prior to attendance. The results in Column (1) in Panel A imply that this decline is roughly 19 percent for individuals who enroll in for-profit schools. However, the results in Column (1) suggest that the relative decrease in earnings is 12 percentage points smaller for individuals who attend not-for-profit schools. Using the average

earnings prior to enrollment from Table 4, this suggests that individuals in the sample who enroll in for-profit schools forgo about $5,300 more in earnings during years of attendance, compared to the earnings forgone by individuals who enroll at not-for-profit institutions.[23] The decrease in earnings during years of enrollment is not the result of differences in the likelihood of working, as virtually all individuals have positive earnings during enrollment. Instead, differences in earnings while enrolled may result from a decrease in hours worked. (Unfortunately, I do not observe hours of work in the tax data.) To the extent that individuals shift work hours into study hours, the relatively larger decrease in earnings for individuals who attend for-profit institutions may be the driven by relatively more hours devoted to schoolwork.

Results based on sample splits for two-year and four-year schools appear in Columns (2)-(4) of Table 5. These sample splits may not accurately capture the differential returns to two-year and four-year courses of study, because the likelihood of pursuing a two-year degree at a four-year institution varies across for-profit and not-for-profit schools. In particular, a relatively larger share of four-year for-profit enrollment pursues two-year degrees, compared to enrollment at four-year not-for-profit schools. (See Appendix Table A2 for the likelihood of pursuing a two-year degree across for-profit and not-for-profit schools.) As a result, the findings in Column (3) for four-year schools result from the comparison of the returns to not-for-profit education, where most students pursue four-year degrees, to the returns to for-profit attendance, where roughly 43 percent of all students pursue two-year degrees. The differential returns to not-for-profit education are larger in Column (3), relative to Column (1), consistent with this type of compositional effect, assuming that the returns are larger for four-year degree programs. Similarly, the results in Column (2) for two-year schools do not capture the returns to two-year

[23] The results suggest that during each year of enrollment individuals who enroll at for-profit institutions forgo $5,580 in earnings, while individuals at not-for-profit intuitions forgo $2,446. The figure in the text assumes 1.7 years of enrollment, equal to the sample average.

degrees of study generally. Instead, the results in Column (2) compare the earnings of individuals pursuing two-year degrees at not-for-profit schools with earnings of individuals who pursue two-year degrees at two-year for-profit schools, omitting individuals who pursue two-year degrees at four-year for-profit schools. To further assess the impact of composition effects, I include the results for a sample that includes all individuals who enroll in for-profit schools and individuals who enroll in two-year not-for-profit schools in Column (4). In this case, the differential effect is positive. This finding is consistent with the idea that the return to education for individuals who pursue two-year degrees at not-for-profit colleges is larger than the return for individuals who pursue either a two-year or a four-year for-profit degree. However, because this differential effect is not significant, this implication is unclear. Given the differences in composition of degree programs across two-year and four-year sample splits, I focus on results for the entire sample.

B. Additional Results

The substantive differential effects in the years after attendance in Table 5 may be the result of differences in enrollment intensity across for-profit and not-for-profits schools. As shown in Table 4, individuals enrolled in for-profit schools are more likely to enroll part-time, compared to individuals who enroll in not-for-profit schools. To explore if differences in enrollment intensity drive the earnings differential in Table 5, I split the sample based on enrollment intensity during the last year of enrollment. The results are similar if I split the sample based on enrollment intensity in the first year of enrollment.[24] Table 6 shows the results after dropping the years of enrollment, with part-time enrollment in Column (2) and full-time enrollment in Column (3). The results suggest that there is a large and significant differential effect for attending a not-

[24] 91 percent of students who are ever enrolled part-time are enrolled part-time during the last year of enrollment.

for-profit school among individuals who enroll part-time. In contrast, the estimated differential effect for individuals who enroll full-time is smaller compared to the effect for individuals enrolled part-time. However, the differential effect among individuals who enroll full-time is not precisely estimated, and I cannot rule out an effect equal to that for part-time enrollment. When the years of enrollment are included, as in Panel A of Table 5, I find that earnings decrease substantively during both part-time and full-time attendance, and I cannot rule out equal decreases in earnings during enrollment across full-time and part-time enrollments.[25]

Table 6: Results by Intensity of Enrollment

	(1) All	(2) Part-time	(3) Full-time
Not-for-Profit*Post	0.077*	0.131**	0.023
	[0.045]	[0.056]	[0.083]
Post	-0.032	-0.031	0.035
	[0.049]	[0.063]	[0.090]
Observations	14,981	6,307	8,674
Individuals	1,996	878	1,118

Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

Another reason for the differential earnings effects reported in Table 5 may be due to differences in institutional quality across for-profit and not-for-profit schools. Individuals who attend not-for-profit schools may have higher ability, and therefore be more likely to realize a larger return from postsecondary attendance. Using coarse measures of admissions characteristics, I find suggestive evidence that the baseline results hold among institutions with similar admissions requirements. In Column (1) of Table 7, I report the results for schools with open admissions policies. In Columns (2) and (3), I report the results for schools that do not have open admissions requirements but that differ on the requirement that individuals include

[25] The estimated effects of *Enrolled* are: part-time -0.191 [0.059]; full-time -0.075 [0.092]. The estimated effects of *Enrolled *Not-for-Profit* are: part-time 0.082 [0.060]; full-time 0.060 [0.093].

standardized test scores in their application. These results exclude the years of enrollment, similar to the results in Panel B of Table 5. (The results are similar if I include the years of enrollment as in Panel A of Table 5.) The finding of a positive differential effect within a given admissions group of schools may be evidence that the baseline results are not driven by sorting of students into for-profit and not-for-profit schools based on ability. However, the differential effect is not significantly different than zero for any of the admissions groups, so that this implication is unclear.

Table 7: Results by School Admissions Characteristics

	(1) Open Admissions	(2) Non-Open Admissions, No Standardized Test Scores	(3) Non-Open Admissions, Standardized Test Scores
Not-for-Profit*Post	0.080	0.098	0.174
	[0.059]	[0.092]	[0.118]
Post	0.037	-0.158*	-0.162
	[0.062]	[0.095]	[0.216]
Observations	10,829	1,942	2,210
Individuals	1,442	266	288

Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

The results in Table 5 represent the return to *additional postsecondary attendance*, rather than the *effect of degree attainment*. A limitation of the tax data is that there is no information on lifetime education or on degree receipt. To the extent that there are differences in graduation rates across for-profit and not-for-profits students, then differences in earnings across for-profit and not-for-profit schools may also reflect differences in degree attainment. Jaeger and Page (1996) find significant diploma effects for some groups of students and for some degree types. In contrast, Marcotte, Bailey, Borkoski and Kienzl (2005) find that degree-attainment effects or "sheepskin effects" are small after controlling for years of attendance, for both two-year and

four-year schools. Kane and Rouse (1995b) find that students who attend community college but do not earn a degree realize a significant earnings increase compared to similar individuals without any college education. In the Appendix, I discuss one approach for investigating sheepskin effects, which suggests that there may be a positive degree-attainment effect. However, this approach relies on the strong assumption that enrollment spells in the data are good approximations for degree completion, and the approach also relies on sample splits based on two-year and four-year schools that are rough proxies for length of time needed for degree.

C. Robustness Checks

A key assumption of the identification is that individuals who enroll in for-profit schools have a similar trend in earnings, compared to the earnings trend for individuals who attend not-for-profit schools, in the years prior to attendance. Figure 1, which plots the natural log of earnings over time, suggests that this assumption holds. (In Figure 1, all years of enrollment are plotted at t=0.) However, Figure 1 does not include individual fixed effects or other controls from the specification. I further test the assumption that individuals who attend not-for-profit and for-profit schools have similar trends in earnings during the pre-enrollment period in two ways.

First, I test whether there are differential changes in earnings in the year before enrollment, compared to earlier years by limiting the sample to the pre-enrollment period and including indicator variables for the number of years before enrollment. These variables measure the differences in earnings two, three, four, or five or more years prior to enrollment, relative to the year before enrollment. I interact these variables with measures of institutional control to test for differential changes in earnings during the year preceding enrollment. As shown in Column (1) of Panel A in Table 8, there are no significant differential effects relative to

the year before enrollment for the entire sample (I show the results for two-year and four-year schools in the Appendix). This may be evidence that students do not differentially sort into schools based on experiencing a bad earnings shock in the year prior to enrollment.

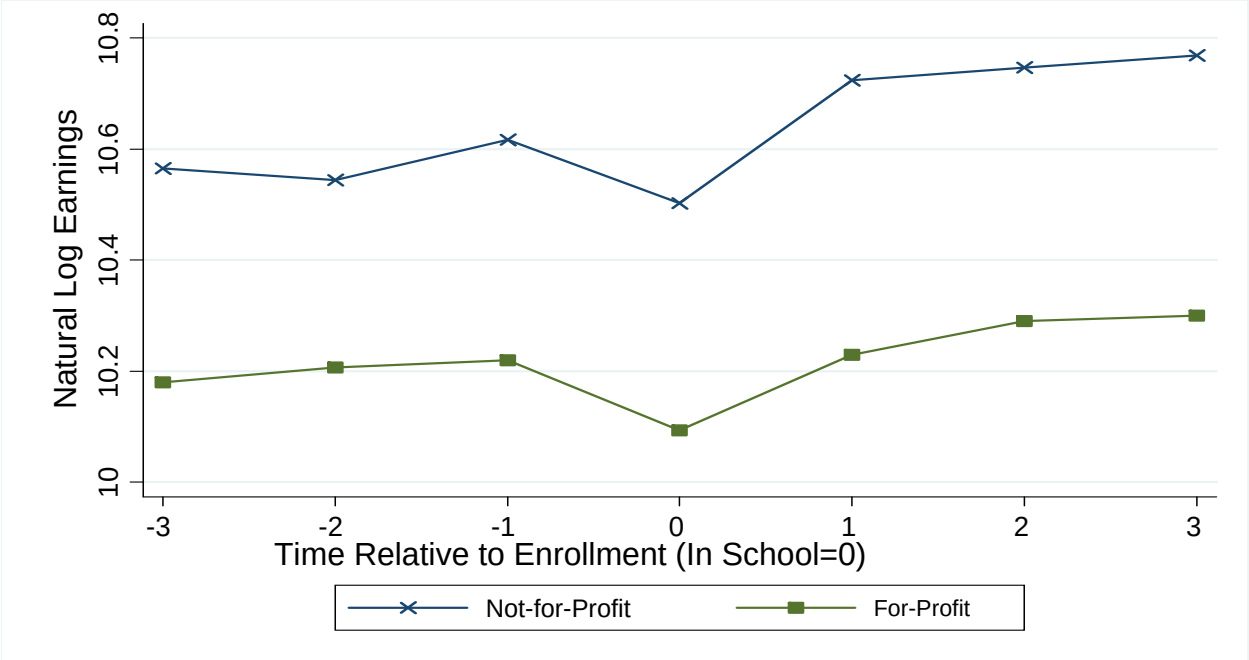


Figure 1: Natural Log of Earnings over Time

Note: Earnings at time equal to zero are the average across all years of enrollment. All other times represent a single year of earnings.

A second way that I examine the identifying assumption of similar patterns in pre-enrollment earnings is by testing for differential trends during the years before enrollment. I limit the sample to the years prior to enrollment and I include a linear time trend and interactions of the trend variable with measures of institutional control. Panel B of Table 8 shows these results. There is a significant and meaningful positive trend in earnings during the pre-enrollment years. The finding that offers support for the identification is that there is not a significant or substantive differential trend in earnings for individuals who attend a not-for-profit school compared to individuals who attend a for-profit school.

Table 8: Trends in Earnings Prior to Enrollment	
	(1)
School Type	All
Panel A: Differences in Earnings Relative to the Year Before Enrollment	
2 Years Prior to Enrollment	0.049
	[0.060]
3 Years Prior to Enrollment	0.011
	[0.08]
4 Years Prior to Enrollment	0.127
	[0.104]
5+ Years Prior to Enrollment	0.136
	[0.124]
Not-for-Profit*2 Years Prior	-0.064
	[0.057]
Not-for-Profit *3 Years Prior	-0.022
	[0.072]
Not-for-Profit *4 Years Prior	-0.111
	[0.082]
Not-for-Profit *5+Years Prior	-0.150*
	[0.077]
Panel B: Trends in Earnings	
Trend*Not-for-Profit	0.033
	[0.026]
Trend	0.227***
	[0.030]
Observations	7,677
Individuals	1,996
Sample includes only the years prior to the first year of enrollment. In Panel A, one year prior to enrollment is the omitted category. Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects age, age-squared, other income and indicator variables for year. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels. Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.	

I find that the differential earnings effects are robust to changes in the specification (including indicator variables for years after enrollment rather than a single indicator variable for the entire post-enrollment period) to alternate measures of income (wage income in place of wages plus self-employment income) and to alternate controls for age (age fixed effects). Using a rough measure of part-time employment, I find evidence that there is a significantly negative differential effect on the likelihood of part-time employment for individuals who enroll in not-for-profit schools. I do not find evidence that there is a differential effect on the probability of

having self-reported income. Conditional on having self-employment income, the results suggest that there is a negative differential effect of not-for-profit attendance. This may be evidence that for-profit enrollment better prepared individuals for self-employment. I discuss these results in the Appendix. Using sample splits, I do not find evidence that the earnings effects across not-for-profit and for-profit schools are substantively different by gender (not shown).[26]

V. Conclusion

This paper is the second analysis of the returns to for-profit education, building on work by Cellini and Chaudhary (2011). The results suggest that the relative effect of attending a for-profit institution, relative to a not-for-profit institution, impacts earnings in two important ways. One, the results suggest that there is a large positive significant differential effect on earnings in the years after attendance, compared to years before attendance, for individuals who attend not-for-profit schools. Two, the results imply that the decrease in earnings during the years of enrollment, relative to earnings in the years before attendance, is relatively smaller for individuals who enroll in not-for-profit schools.

Combined with differences in the cost of attendance, the estimated differences in the return to education across not-for-profit and for-profit schools suggest that the net private benefit from attendance is lower at for-profit schools compared to public or private schools. If restrictions in federal aid at for-profit schools cause students to enroll at public or private institutions rather than for-profit schools, this finding suggests that some students will be better off by realizing higher lifetime earnings and incurring lower education costs. A shift in

[26] I merge information from the Social Security Administration to obtain information on gender. I remove roughly 100 observations that have missing or invalid gender information.

enrollment towards not-for-profit schools may also have important effects for the federal government. Future individual income tax revenues may increase as individuals shift towards not-for-profit schools that are estimated to result in larger earnings growth compared to for-profit schools. Total expenditures on student aid may also decrease. On average, individuals who attend for-profit schools receive relatively more federal aid and are more likely to default on their student loans, compared to individuals who enroll at not-for-profit schools.

While this paper is among the first to explore the labor market outcomes for individuals who attend for-profit institutions, there are several important limitations of this study. First, the results here represent the "treatment on the treated" effect, which potentially includes selection into schools based on student characteristics. For policymakers interested in studying the labor market outcomes of individuals who enroll at for-profit institutions, this is an important effect. However, because the results do not rely on exogenous variation in enrollment across for-profit and not-for-profit schools, the results may reflect both the causal effect of postsecondary attendance on earnings and selection effects that result from the decision to attend a not-for-profit institution. Second, the analysis considers only short-term labor market outcomes. On average, individuals are in the sample for four years after enrollment. It is possible that the differential earnings effects observed for this post-enrollment period do not hold over longer periods of time. Third, the analysis considers only the effect on additional years of attendance, rather than the total number of years of higher education, or degree attainment. Absent information on degree-type or program of study, differences in the returns to education across school types also include differences in programs across schools. Lastly, while the data are drawn from a nationally representative random sample of tax returns, the analysis sample is relatively small. The sample includes a total of about 2,000 individuals, roughly 200 of which

attend for-profit schools. As a result, the findings may not accurately characterize differences in the returns to for-profit and not-for-profit attendance generally.

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Methodological Appendix
A1. Appendix Tables and Figures

Table A1: Number of Individuals by Institutional Control during First and Last Years of Enrollment

		Last Year of Enrollment					
		Public Four-year	Not-for-Profit Private Four-year	For-Profit Four-year	Not-for-Profit Two-year	For-Profit Two-year	*Total (Row)*
First Year of Enrollment	Public Four-year	266	1	1	2	1	*271*
	Private Four-year	3	236	2	3	1	*245*
	For-Profit Four-year	0	2	97	1	0	*100*
	Not-for-Profit Two-year	8	4	1	1,263	3	*1,279*
	For-Profit Two-year	1	2	2	0	96	*101*
	Total (column)	*278*	*245*	*103*	*1,269*	*101*	*1,996*
	Diagonal Element as Share of Column Total	**95.7**	**96.3**	**94.2**	**99.5**	**95.0**	

Table A2: Percent of Students Pursuing Two-year Degrees, 2007-08 School Year

	(1) Not Working on Associate's Degree	(2) Pursuing AA, AS	(3) Pursuing AAS, Occupational, or Technical Program
Public 4-year	95.3	3.2	1.5
Public 2-year	21.1	54.9	24
Public less-than-2-year	100	0	0
Private not-for-profit 4-year	96.9	2	1.1
Private not-for-profit 2-year	12.8	37.7	49.5
Private not-for-profit less-than-2-year	100	0	0
Private for-profit 4-year	57.4	24	18.5
Private for-profit 2-year	38.2	21.3	40.5
Private for-profit less-than-2-year	100	0	0

Source: National Postsecondary Student Aid Study, 2008 for undergraduate students. Table created using the DAS online system 9/22/2011.

Table A3: Estimated Returns to Education without Individual Fixed Effects

	(1) All	(2) Two-year	(3) Four-year
Panel A: Include Enrollment Years			
Not-for-Profit*Post	0.236***	0.338***	0.140
	[0.057]	[0.072]	[0.091]
Post	-0.1666**	-0.221**	-0.156
	[0.069]	[0.088]	[0.115]
Enrolled	-0.347***	-0.506***	-0.261***
	[0.066]	[0.087]	[0.097]
Enrolled*Not-for-Profit	0.262***	0.455***	0.091
	[0.067]	[0.088]	[0.102]
Observations	18,094	12,458	5,636
Individuals	1,996	1,370	626
Panel B: Drop Enrollment Years			
Not-for-Profit*Post	0.236***	0.338***	0.139
	[0.057]	[0.072]	[0.091]
Post	-0.167**	-0.229***	-0.155
	[0.069]	[0.085]	[0.116]
Observations	14,981	10,433	4,471
Individuals	1,996	1,370	626

Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

Table A4: Estimated Returns to Education with Separate Interactions for Public and Not-for-Profit Private Schools

	(1)	(2)
	All	Four-year
Panel A: Include Enrollment Years		
Public*Post	0.065	0.132*
	[0.046]	[0.075]
Not-for-Profit Private*Post	0.146**	0.174**
	[0.064]	[0.079]
Post	-0.018	-0.080
	[0.049]	[0.078]
Enrolled	-0.187***	-0.155**
	[0.049]	[0.061]
Enrolled*Public	0.113**	0.017
	[0.050]	[0.072]
Enrolled*Not-for-Profit Private	0.150**	0.090
	[0.067]	[0.075]
Observations	18,094	5,636
Panel B: Drop Enrollment Years		
Public*Post	0.066	0.133*
	[0.045]	[0.074]
Private*Post	0.143**	0.169**
	[0.064]	[0.078]
Post	-0.030	-0.105
	[0.049]	[0.080]
Observations	14,981	4,548

Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

Table A4: Trends in Earnings Prior to Enrollment			
	(1)	(2)	(3)
School Type	All	Two-year	Four-Year
Panel A: Differences in Earnings Relative to the Year Before Enrollment			
2 Years Prior to Enrollment	0.049	0.153	-0.035
	[0.060]	[0.101]	[0.077]
3 Years Prior to Enrollment	0.011	0.100	-0.053
	[0.08]	[0.131]	[0.123]
4 Years Prior to Enrollment	0.127	0.204	0.085
	[0.104]	[0.150]	[0.170]
5+ Years Prior to Enrollment	0.136	0.178	0.142
	[0.124]	[0.149]	[0.245]
Not-for-Profit*2 Years Prior	-0.064	-0.178	0.043
	[0.057]	[0.100]	[0.060]
Not-for-Profit *3 Years Prior	-0.022	-0.136	0.087
	[0.072]	[0.123]	[0.078]
Not-for-Profit *4 Years Prior	-0.111	-0.211	-0.056
	[0.082]	[0.132]	[0.092]
Not-for-Profit *5+Years Prior	-0.150*	-0.222**	-0.135
	[0.077]	[0.111]	[0.116]
Panel B: Trends in Earnings			
Trend*Not-for-Profit	0.033	0.047**	0.027
	[0.026]	[0.024]	[0.024]
Trend	0.227***	0.198***	0.234***
	[0.030]	[0.037]	[0.047]
Observations	7,677	5,326	2,351
Individuals	1,996	1,370	626

Sample includes only the years prior to the first year of enrollment. In Panel A, one year prior to enrollment is the omitted category. Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

A2. Degree Attainment Effects

To address the possibility that the differential effect in the years after attendance is attributable to differences in degree attainment across for-profit and not-for-profit schools, I split the sample based on the duration of attendance. For this approach to demonstrate the potential difference in earnings attributable to sheepskin effects, I make the strong assumption that students who enroll during the sample period for less than the normal time required for degree attainment are less likely to have completed their program, compared to students who enroll for at least the normal

time.[27] Further, I assume that the relative likelihood of degree attainment within these groups is similar across individuals who enroll in for-profit and not-for-profit schools. I define individuals attending less than the normal time as those who enroll at a two-year school for one year or who attend a four-year school for less than three years. [28] In practice, individuals who attend for less than the normal time may be completing their degree following an interrupted spell of enrollment, or may complete their degree more quickly than the normal time.

Assuming that the sample split based on duration of attendance is a good proxy for a sample split based on degree completion, the results suggest that there is a positive sheepskin effect for both for-profit and not-for-profit schools. Column (1) of Table A5 shows the results for individuals who attend less than the normal time during the sample period, and Column (2) shows the results for individuals who attend at least the normal time during the sample period. The return to for-profit education is larger in Column (2) relative to Column (1), consistent with a sheepskin effect, although this difference is not significant. Likewise, the total effect of not-for-profit attendance is larger in Column (2) compared to Column (1). For both two-year and four-year schools, the differential results suggest that the effect of not-for-profit enrollment increases with degree attainment. However, it is also possible that the differential effect is driven by differences in graduation rates across for-profit and not-for-profit schools within each of the sub-samples. More generally, this approach offers only a rough approximation of potential sheepskin effects, and does not restrict the comparison to comparable degrees across for-profit and not-for-profit schools. Further, this approach relies on sample splits based on two-year and four-year schools, which is only a rough proxy for the length of the degree program.

[27] Jaeger and Page (1996) use data from the 1991 and 1992 March CPS and find that 87 percent of individuals with 16 years of education have a bachelors degree, suggesting that sample splits based on normal time to degree is a rough proxy for sample splits based on degree attainment for four-year schools.

[28] For four-year schools the distribution of years of enrollment is: 1 year, 53 percent; 2 years, 23 percent; 3 years, 14 percent; 4 years, 6 percent; 5 years, 4 percent. For two-year schools the distribution of years of enrollment is 1 year, 66 percent; 2 years, 19 percent; 3 years, 8 percent; 4 years, 6 percent; 5 years, 2 percent.

Table A5: Returns by Duration of Attendance		
School Type	(1) Attend Less than Normal Time	(2) Attend At Least Normal Time
Panel A: Two-year Schools		
Not-for-Profit*Post	0.070	0.038
	[0.079]	[0.100]
Post	-0.021	0.125
	[0.080]	[0.083]
Observations	7,314	3,119
Individuals	914	456
Panel B: Four-year Schools		
Not-for-Profit*Post	0.118	0.309*
	[0.076]	[0.163]
Post	-0.086	0.025
	[0.086]	[0.279]
Observations	3,679	869
Individuals	483	143
Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year. Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels. Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.		

A3. Additional Robustness Checks and Alternate Labor Market Outcomes

The baseline results represent the average change in earnings during the post-enrollment period, relative to earlier years. To determine if the differential earnings effects vary over time in the post-enrollment period, I replace the *Post* variable with indicator variables equal to one for each of the following periods after attendance: years 1 and 2; years 3 and 4; years 5 and 6. I interact these variables with indicator variables of institutional control to measure how the differential earnings effects evolve over time. Table A6 shows these results, which suggest that the differential effects are largest in the first four years after enrollment, relative to later years. However, the effects for later years are not precise because they are identified by a relatively smaller number of individuals who have longer earnings histories.

Table A6: Estimated Returns to Education over Time			
	(1) All	(2) Two-year	(3) Four-year
Years 1 & 2	-0.051	0.011	-0.141
	[0.053]	[0.072]	[0.087]
Years 3 & 4	-0.078	-0.011	-0.174
	[0.070]	[0.088]	[0.124]
Years 5 & 6	0.111	0.141	0.088
	[0.096]	[0.106]	[0.184]
Not-For-Profit*Years 1 & 2	0.088*	0.036	0.172**
	[0.046]	[0.068]	[0.067]
Not-For-Profit*Years 3 & 4	0.111*	0.070	0.175*
	[0.060]	[0.078]	[0.096]
Not-For-Profit*Years 5 & 6	-0.128	-0.125	-0.150
	[0.085]	[0.096]	[0.158]
Observations	14,981	10,433	4,548

The results remove the years of enrollment, following the approach used in Panel B of Table 5. The results are substantively and statistically similar including years of enrollment and interactions of years of enrollment with indicators for institutional control.
Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.

Using the number of W2s to identify individuals who are likely employed part-time,[29] I find evidence that there is negative differential impact on the probability of part-time employment for individuals who attend not-for-profit schools, relative to individuals who attend for-profit schools. However, the measure of part-time employment that I use is likely to be a noisy measure of actual part-time employment, so that this implication is unclear. Column (1) of Table A7 shows these results. I find no evidence of a significant differential effect of not-for-profit attendance on the likelihood of reporting self-employment income. Column (2) shows the results for the likelihood of reporting self-employment income. Conditional on having self-employment income, I find that there is a large negative differential effect on the value of self-employment income. This may be evidence that not-for-profit schools better prepare students for

[29] I identify individuals with three or more W2s as likely to be employed part-time. The results are the same if I consider only individuals with three or more W2s in consecutive years as likely to be part-time employees.

entrepreneurial ventures, or it could be that individuals who attend not-for-profit schools do not purse self-employment as their primary source of earnings. The baseline earnings effects are robust to using the natural log of wages instead of the natural log of earnings. Column (4) of Table A6 reports the effects on wages. Similar to the baseline effects, there are positive and meaningful differential effects found for individuals who attend a public or private school, compared to individuals who attend for-profit schools.

Table A7: Alternate Labor Market Outcomes

School Type	(1) Probability of Part-time Employment	(2) Probability of Self-Employment Income	(3) Value of Self Employment Income	(4) Wage Income
Not-for-Profit*Post	-0.054*	-0.015	-0.206	0.129**
	[0.030]	[0.023]	[0.316]	[0.063]
Post	0.088***	0.041	0.042	-0.120*
	[0.033]	[0.025]	[0.309]	[0.068]
Observations	14,981	14,981	2,020	14,626

The results remove the years of enrollment, following the approach used in Panel B of Table 5. The results are substantively and statistically similar including years of enrollment and interactions of years of enrollment with indicators for institutional control.
Column (3) is conditional on having positive self-employment income.
Column (4) includes only observations with non-zero wage income.
Standard errors, clustered at the tax return level, are reported in brackets. Control variables include individual fixed effects, age, age-squared, other income and indicator variables for year.
Asterisks denote significance at the 1% (***), 5% (**), and 10% (*) levels.
Data from the Internal Revenue Service and the Integrated Postsecondary Education Data System.