NBER WORKING PAPER SERIES

WHEN INVESTOR INCENTIVES AND CONSUMER INTERESTS DIVERGE: PRIVATE EQUITY IN HIGHER EDUCATION

Charlie Eaton
Sabrina Howell
Constantine Yannelis

Working Paper 24976
http://www.nber.org/papers/w24976

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2018

We are grateful to discussants Pierre Azoulay, Michael Ewens, Jonah Rockoff, and Richard Thakor and for helpful comments from Raji Chakrabarti, Francesca Cornelli, David Deming, Michael Dinerstein, Michael Ewens, Caroline Hoxby, Will Gornall, Arpit Gupta, Justine Hastings, Steve Kaplan, Theresa Kuchler, Alexander Ljungqvist, Holger Mueller, Thomas Philippon, Larry Schmidt, Antoinette Schoar, Albert Sheen, Doug Staiger, Lesley Turner, Jeff Wurgler, David Yermack and seminar participants at the NBER Labor Studies and Education SI, MIT Sloan, Harvard Business School, Yale School of Management, Berkeley Haas School of Business, The Federal Reserve Bank of New York, the NBER Entrepreneurship meeting, the FMA Napa Conference, the SFS Cavalcade, and NYU Stern. We are also grateful to Suzanne Chang and Dalya Elmalt for superb research assistance. Sabrina Howell thanks the Kauffman foundation for generous financial support and the Stern Infrastructure Initiative for Preqin data. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

When Investor Incentives and Consumer Interests Diverge: Private Equity in Higher Education
Charlie Eaton, Sabrina Howell, and Constantine Yannelis
NBER Working Paper No. 24976
August 2018
JEL No. G24,G38,H52,I2,J01

ABSTRACT

This paper uses private equity buyouts to study a transition from lower- to higher-powered profit-maximizing incentives in higher education, a sector heavily dependent on government subsidy. Private equity owners have especially high-powered incentives to maximize profits. In a subsidized industry, this could intensify focus on capturing government aid at the expense of consumer outcomes. Employing novel data on 88 private equity deals and 994 schools with private equity ownership, we find that private equity buyouts lead to higher enrollment and profits, but also to lower education inputs, higher tuition, higher per-student debt, lower graduation rates, lower student loan repayment rates, and lower earnings among graduates. Neither changes to the student body composition nor a selection mechanism fully explain our results. In a series of tests exploiting regulatory events and thresholds, we find that private equity-owned schools are better able to capture government aid.

Charlie Eaton
University of California, Merced
Department of Sociology
5200 N. Lake Road
Merced, CA 95348
ceaton2@ucmerced.edu

Sabrina Howell
NYU Stern School of Business
KMC 9-93
44 West 4th Street
New York, NY 10012
and NBER
showell@stern.nyu.edu

Constantine Yannelis
Stern School of Business
New York University
44 West Fourth Street
Suite 9-190
New York, NY 10012-1126
cyanneli@stern.nyu.edu

An online appendix is available at http://www.nber.org/data-appendix/w24976

1 Introduction

This paper uses private equity buyouts to study a transition from lower- to higher-powered profit-maximizing incentives in higher education. Relative to closely-held private firms or diffusely-held publicly traded firms, private equity-owned firms have particularly high-powered incentives to maximize profits (Jensen 1989).[1] In education, one reason such high-powered incentives might be poorly aligned with student interests is that education is heavily subsidized. Federal grants and federally guaranteed loans comprise around 90 percent of for-profit schools' revenue and have little if any dependence on student outcomes. We find that student outcomes deteriorate after a school is bought by a private equity firm, and reliance on federal aid and guaranteed loans increases.

Private equity buyouts offer a useful setting to study high-powered incentives because private equity firms often acquire independent, privately-owned schools. We employ novel data on 88 private equity deals, which are associated with 557 school-level ownership changes. Private equity-owned school systems acquire or establish an additional 437 schools. Descriptive statistics indicate that other for-profits are in some ways more similar to community colleges than to private equity-owned schools, suggesting that private equity involvement may explain some of the negative attributes associated with for-profits in, for example, Deming, Goldin, and Katz (2012), Cellini and Goldin (2014), Cellini and Turner (2016), and Deming, Yuchtman, Abulafi, Goldin, and Katz (2016). We find sharp declines in student graduation rates, loan repayment rates, and labor market earnings after private equity buyouts using regressions with school and year fixed effects, as well as a matching estimator.

Private equity buyouts can affect student outcomes through two non-mutually exclusive channels. The first is changed operations that are detrimental to student success. The second is a changed student body composition; for example, students who attend after the buyout may be

[1] This is because private equity managers of buyout funds are compensated through a call option-like share of the profits, employ substantial amounts of leverage, and usually aim to liquidate investments within a short time frame. Private equity funds are financial intermediaries. In exchange for a profit share ("carried interest"), general partners invest third party capital in private firms, with the goal of achieving liquidity through a sale or IPO. Private equity contracts are complex and state-contingent, usually giving the investor substantial control rights (Lerner and Schoar 2005). For an overview, see Kaplan and Strömberg (2009) and Metrick and Yasuda (2010).

less-well prepared than those who attended before. This channel has ambiguous implications for student welfare and depends on school value-added, which we do not directly observe. To isolate the effect of operational changes, we hold composition effects fixed using partially treated cohorts. These are students in two-year programs who are already enrolled before a buyout occurs but have at least one year at the now private equity-owned school. We are able to compare the cohort with partial treatment to the previous one with no treatment for graduation and loan repayment rates. Partially treated cohorts experience more than half the negative effect on graduation rates, and the full effect on repayments rates, that fully treated cohorts experience. Further, any composition mechanism does not act through observable demographic changes.

We next examine evidence for federal aid capture. First, we establish that higher-powered incentives lead to higher profits; in fact, profits triple after a buyout. This concurs with existing work associating private equity buyouts with higher firm value, including Cao and Lerner (2009), Boucly, Sraer, and Thesmar (2011), Guo, Hotchkiss, and Song (2011), and Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014). Higher revenue comes in part from increases in tuition. These increases are about half average total tuition at community colleges.[2] Reliance on federal aid also increases after private equity buyouts and approaches the 90 percent of revenue threshold that is the statutory limit. Per-student borrowing and per-student federal grants increase by about 12 and 14 percent of their respective means.

We conduct three explicit tests for federal aid capture. First, we exploit an increase in student loan borrowing limits in 2007. Relative to other institutions, private equity-owned schools respond to the increase by raising tuition faster than other for-profit schools, which induces higher levels of borrowing. Second, private equity-owned schools bunch below federal aid sanction thresholds. Third, publicly traded for-profit school share prices fall precipitously after the announcement of rules aiming to tie federal aid to student labor market performance. This indicates that for-profits' future cash flows depend on their ability to access government aid irrespective of student outcomes. Superior capture of government aid is thus an important channel

[2]Tuition increases by about $1,600, relative to a mean at private equity-owned schools of $17,521 and a mean at community colleges of $3,673.

through which high-powered incentives translate to higher profits. This is a purely rent-seeking phenomenon and is unambiguously not in students' or taxpayers' interest.

Rent-seeking opportunities and heightened focus on capturing aid rather than on student outcomes may reflect the separation between consumers (students) and subsidy revenue in higher education. It seems likely that improved subsidy design could better align incentives. This might be one avenue towards addressing the growth in federally guaranteed student debt – which increased from $241 billion in 2003 to $1.4 trillion in 2018 – and possible accompanying adverse effects, including high levels of default and reduced entrepreneurship.[3]

Changes to operations may explain some of the effects we observe on student outcomes and school financials. Operational changes are consistent with existing literature finding better management among private equity-owned firms, including Kaplan (1989), Muscarella and Vetsuypens (1990), and Bloom, Sadun, and Van Reenen (2015). We find that education inputs decline after the buyout, while enrollment increases by almost 50 percent .[4] Aggressive marketing may help explain the apparent disconnect between deteriorating outcomes and increasing enrollment. Private equity-owned schools have twice the share of employees in sales as other for-profits, and law enforcement actions related to misrepresentation and recruiting violations increase dramatically after private equity buyouts. Aggressive marketing may be effective because education quality is opaque, tuition is not salient as students usually pay zero upfront costs, and the for-profit target population is extremely socioeconomically disadvantaged (Deming, Goldin, and Katz 2012).

Whether additional students enrolled as a result of post-buyout expansion are better or worse off depends in part on their outside option. A large literature finds that the expected labor market returns to for-profit education are lower than the returns to non-selective community college.[5] If the source of expansion is substitution away from community colleges, the new students are

[3] See Looney and Yannelis (2015), Bleemer et al. (2017), Goodman et al. (2017) and Krishnan and Wang (2017).

[4] Observed inputs include the ratio of faculty to students, the absolute number of faculty, the share of spending devoted to instruction, and the absolute dollars spent on instruction.

[5] See Deming et al. (2012), Liu and Belfield (2014), Cellini and Chaudhary (2014), Cellini and Turner (2016), Deming et al. (2016) and Armona, Chakrabarti, and Lovenheim (2017).

unlikely to be better off. Indeed, we find suggestive evidence that a new private equity-owned school in a commuting zone siphons student enrollment growth from community colleges. This is not surprising, as Cellini, Darolia, and Turner (2017) show that community colleges and for-profit schools are direct substitutes.

The alternative to a causal interpretation of our findings is a selection mechanism, in which private equity firms are skilled at selecting targets on trajectories to higher profits. For all outcomes, we show visual event studies among switcher-schools around the buyout year. We also plot the means of outcome variables around the buyout year for the matched target and control schools (control school plots serve as placebo tests). These visual event studies reveal discontinuous breaks in outcomes and and sharp changes to trends around the buyout year. There are no meaningful observable pre-trends. While private equity acquisitions are not random, this visual evidence makes a selection mechanism unlikely to fully explain the effects. As an alternative, we show that new governance may drive the operational changes. University chief executive turnover increases by about 36 percent in the three years following the buyout.

The adverse effects of private equity buyouts are stronger than the effects of being purchased by a for-profit chain that is not private equity-owned. This suggests a hierarchy of incentives, in which the highest-powered incentives are associated with the greatest incentive misalignment. This differs somewhat from Duggan (2000)'s finding that for-profit and nonprofit hospitals respond similarly to a new financial incentive.

Our findings also contrast with those in Bernstein and Sheen (2016) and Fracassi, Previtero, and Sheen (2017). They show that operational changes induced by private equity ownership improve consumer outcomes in sectors characterized by high competition, transparent product quality, and immediate market feedback: chain restaurants and chain retail stores. Profit-maximizing incentives may be less well aligned with consumer interests in sectors where intensive government subsidy separates revenue from the consumer. Such sectors typically also feature less competition, opaque product quality, and consumer outcomes measurable only many years after payment (Hansmann 1980). Other subsidized sectors with these characteristics, such as healthcare, infrastructure, and

defense, also receive large amounts of private equity investment (see Appendix B Figure 1).

At the same time, many institutions in these sectors and in education are nonprofit. Glaeser and Shleifer (2001) explain how weaker incentives to maximize profits can make nonprofit status optimal in settings where consumers depend on implicit contracts with the firm (also see Shleifer and Summers 1988). This mechanism requires consumers to rationally choose nonprofit firms over for-profit ones. In higher education, severe information frictions, a vulnerable target population, and government subsidy contribute to low price elasticity of demand, making high-powered incentives profitable for some firms but counter to students' and taxpayers' interests.

Our paper is related to the broader literature on private equity, including Lerner and Schoar (2005), Brown, Gredil, and Kaplan (2013), and Kerr, Nanda, and Rhodes-Kropf (2014). In addition to Bernstein and Sheen (2016) and Fracassi et al. (2017), two papers offer insights related to ours. Matsa (2011) shows that highly levered supermarket firms, which sometimes become highly levered through private equity buyouts, experience higher inventory stock-outs. Ljungqvist, Persson, and Tag (2016) study the misalignment between private and social incentives in private equity-backed stock delistings.

In Section 2, we discuss the institutional context and data. We present the estimation approaches in Section 3. The effects on student outcomes are in Section 4. We examine the relationship between buyouts and school finances, including the capture of government aid, in Section 5. Finally, operational mechanisms that may explain the effects are in Section 6.

2 Data and Descriptive Statistics

2.1 Institutional Context

For-profit schools ("for-profits") have existed in the U.S since the early 1900s, but enrollment has grown substantially in the past two decades, comprising around two million students and 10 percent of enrollment at the peak in 2011 (left graph of Figure 1). As of 2016, about 1.2 million

students were enrolled at for-profit schools. In 2011, the last year for which two-year default rates are available, for-profits accounted for about 40 percent of student loan defaults. For-profits attract more socioeconomically disadvantaged students than community colleges, which are the closest comparison (Deming, Goldin, and Katz 2012, Looney and Yannelis 2015).

For-profits devote far more resources to recruiting than other types of schools. Salespeople can market zero upfront costs to low-income students, despite higher average tuition among for-profits than alternatives. Figure 2 shows the share of school employees in sales (left graph) and non-instructional activities (right graph), by school type and ownership between 2012 and 2015 (this variable cannot be used in analysis because data are limited to these years). While public and non-profit schools have less than one percent of employees in sales, other for-profits have four percent, and private equity-owned schools over seven percent. Government investigations have found evidence of deceptive marketing practices among for-profits.[6] An absence of accessible information, the difficulty of assessing returns to education, and long lags between enrollment and job placement impede the transmission of product quality to future sales (Arcidiacono et al. 2016, Bettinger et al. 2012, Wiswall and Zafar 2014).

For-profits garner about 90 percent of their revenue from public sources (CFBP 2012, Kelchen 2017). They are incentivized to target low-income students, who qualify to pay tuition primarily with federal grants and loans and so need not be billed regularly. Tuition is the most important determinant of the amount of federal aid a student may receive, which incentivizes for-profits to increase tuition above cost (Cellini and Goldin 2014). Federal revenue arrives when the student begins school and is largely disconnected from graduation rates and labor market outcomes. The taxpayer bears the cost of student defaults.[7] Thus government aid and loan guarantees create a potential misalignment of incentives between for-profit school owners and consumers. We flesh out this point and the institutional context of the for-profit higher education sector in Appendix A

[6] Senate (2012), https://www.gao.gov/products/GAO-10-948T

[7] Legislation proposed in the U.S. Congress in November, 2017 would require schools to repay a portion of defaulted student loans. A Wall Street Journal article noted that "This so called skin-in-the-game proposal has been long fought by the powerful higher education lobby." See https://www.wsj.com/articles/house-gop-to-propose-sweeping-changes-to-higher-education-1511956800.

Sections 1-3.

2.2 Private Equity in Higher Education

Private equity and formerly private equity-owned schools account for approximately 35 percent of for-profit enrollment. Private equity buyouts can affect target firm operations and finances, and are often accomplished using debt, which increases the target's leverage (Metrick and Yasuda 2010). This paper focuses on student outcomes and does not address firm capital structure.[8]

Private equity investments in higher education have been either purchases of independent, private colleges, usually with consolidation intent, or acquisitions of existing chain institutions.[9] Figure 1 shows the private equity-owned share of total enrollment and defaults over time.[10] Most of the increase in the for-profit share of student loan defaults since 2000 has been among private equity-owned schools. Appendix B Figure 2 shows that on average, private equity-owned schools have higher default rates than any other type of school. Appendix A Section 4 describes the role of private equity in for-profit higher education in detail.

To collect higher education private equity deal data, we researched the parent ownership history of every for-profit college in the U.S. from 1987 through 2016 that was eligible for federal aid (termed "Title IV eligible"). Sources include online-course catalogs in which all Title IV colleges are required to disclose their ownership history, school and private equity firm websites, unpublished private equity investment portfolio documents gathered by the Senate Health, Education, Labor, and Pension (HELP) Committee, 10-K statements for publicly traded firms, and the ThomsonOne database of private equity investment. We identified 88 private equity buyouts of for-profit college companies before 2016. The left graph in Figure 3 shows the number

[8] We do not observe debt.

[9] An example of the first type, which illustrates the broader pattern we find, is TA Associates' buyout of Florida Career College for $53 million in 2004. At the time, Florida Career College had four campuses and 2,500 students. After adding three additional campuses and expanding enrollment to 4,000 students, TA Associates sold its stake in 2007 for $192 million, almost quadrupling its investment. Later in 2007, federal investigators found employees producing fraudulent high school diplomas for applicants and encouraging students to lie about their high school status. See the Chronicle for further information.

[10] Defaults are measured at least three years after graduation, so we terminate both plots in 2011. We include formerly private equity-owned publicly traded schools.

of private equity deals in the for-profit education sector over time.[11] The private equity firms in our data are roughly representative of the industry.[12]

2.3 School Characteristics and Student Outcomes

School characteristic and student outcome data come primarily from the Integrated Postsecondary Education Data System (IPEDS). All schools that are Title IV eligible must report to IPEDS.[13] Most variables are reported at the school level according to a unique "UnitID" that remains constant over time and across ownership changes. There are no UnitID mergers in our sample. We create a unique identifier, "SystemID", to represent the parent institution, including parent companies of for-profit college chains. This is important because for-profit college companies often operate multiple schools.

At the SystemID level, the 88 buyouts are associated with 88 SystemID switches of ownership. A parent company purchased in a buyout often owns multiple schools, and after the buyout the parent often purchases additional schools. We have 994 schools, or UnitIDs, that ever come under private equity ownership.[14] Of these, 557 are through ownership changes. They are graphed over time in the right plot of Figure 3. In turn, 326 of these are through the private equity deal, and 231 are through subsequent acquisitions by the now-private equity-owned school. The remaining 437

[11] Appendix B Table 1 Panel 1 shows that nearly 80 percent of the 88 deals are known buyouts, while the other 20 percent may be minority stake purchases. For simplicity, we use the term "buyout" in the remainder of the paper. Panel 2 shows that among the 43 deals where we can identify a liquidity event (an "exit"), the average time to exit was 6.8 years. Of these, 22 were sales to other private equity firms, and 7 were IPOs. Twenty-seven remain in the private equity firm's portfolio.

[12] Appendix B Table 1 Panel 4 describes the 118 firms we identify as participating in a private equity deal. We collected data about firm age, experience in other education deals outside our sample (courtesy of Mitch Leventhal), and data on firm performance from Preqin, a commercial private equity data provider. Preqin has data about just 62 of the firms. Within this group, the firms' funds had an average net multiple of 1.6, which is just under Preqin's benchmark for that firm's class (Preqin categorizes firms by investment type and stage). Their internal rates of return were about 15 percent, about 1.5 percentage points higher than their benchmarks'. These data suggest that the firms in our data are not especially high or low performing relative to their peers.

[13] This includes the majority of the higher education sector. Cellini and Goldin (2014) note that Title IV eligible schools made up 73% of the for-profit sector in 2010. Tuition in non-eligible schools is much lower, since students don't have access to federal loans and grants.

[14] The large difference between the number of SystemIDs and UnitIDs is somewhat specific to private equity-owned school systems. The vast majority of SystemIDs in our data have just one UnitID; these are standalone schools such as NYU or UC Merced. Private equity-owned parent companies often own many UnitIDs.

are new schools established by private equity-owned school systems.[15]

Table 1 summarizes the variables we use in analysis.[16] For comprehensive descriptions, sources, and years available for all variables used in analysis, see Appendix B Table 2. The graduation rate (fraction of students who graduate within 150 percent of normal time) averages 48 percent for private equity-owned schools, compared to 55 percent at other for-profits. We do not report IPEDS data on community college graduation rates because they are neither accurate nor comparable.[17] Average loans per borrower among full-time first year students is $7,456 at private equity-owned schools, compared to $5,711 for other for-profits and $3,543 at community colleges.

There are two measures of loan repayment. The first is the two-year cohort default rate (CDR), which is the loan default rate for exiting cohorts (graduates and drop outs) two years after the exit year. We use this in Figure 1 because it has the longest time series (1992 through 2011). It is, however, known to be subject to manipulation through the use of allowable non-repayment options like deferments and forbearances (ICAS 2012). Comparisons between for-profits and other types of colleges should therefore be made with caution. We use the CDR time series to analyze potential bunching of default rates close to regulatory limits. The second measure is the share of students in repayment. This is the fraction of borrowers from a school who have not defaulted and have repaid at least $1 of their initial balance three years after leaving school (by graduating or dropping out). Repayment rates are more sensitive than default rates, which measure only the worst-case scenario for repayment outcomes. The repayment rate averages 32 percent among private equity-owned schools, 41 percent at other for-profits, and 47 percent at community colleges. Motivating

[15]Some variables are reported at the OPEID level, which in some cases aggregates UnitIDs. There are a total of 374 switcher OPEIDs. This is smaller because OPEIDs sometimes encompass multiple UnitIDs and the data for which we use OPEIDs (repayment rates and earnings) are available for fewer years.

[16]Data are presented at the school (UnitID level), except for profits, which are at the SystemID level because financial data are reported to IPEDS through parent UnitIDs for multiple associated UnitIDs (see Jaquette and Parra 2014). The data span 1987 through 2016, but some variables are not available until the early 1990s. A year corresponds to the spring term of the academic year, which begins on August 1 and ends July 30. For example, observations for the 2008-2009 academic year are identified as 2009.

[17]The U.S. DOE recently revised these measures because they tend to over-count graduation rates at for-profits while substantially undercounting degree completion at community colleges by miscounting transfer students (DOE 2011, Carey 2017). IPEDS community college graduation rates also differ sharply from estimated graduation rates for community college students in other National Center for Education Statistics (NCES) surveys.

our analysis is the possibility that private equity involvement may explain some of the negative attributes commonly associated with for-profits (e.g. Deming et al. 2016). In fact, other for-profits resemble community colleges more than private equity-owned schools in some respects, such as loan repayment rates and faculty to student ratios.

Private equity-owned schools are larger, with mean enrollment of 748 students, compared to 387 at other for-profits. Per full-time equivalent student, tuition revenue averages $17,521 at private equity-owned schools relative to $14,210, $3,672, and $10,995 at non-private equity-owned for-profits, community colleges, and nonprofit/state schools, respectively. Community colleges and other for-profits respectively have 4.4 and 4.5 faculty per 100 full-time equivalent students, while private equity-owned schools have 3.6. Per-student Pell Grant revenue indicates the degree to which the student body is low-income. At private equity-owned schools, it is slightly higher than at other for-profits, but it is almost three times higher than at community colleges. We also compiled statistics on degrees and major types, though these are not reported for brevity. The most common degree type at a private equity target school (in the year before acquisition) is a 1-year Communications degree (18 percent of degrees awarded). Our online time-varying indicator variable follows Deming et al. (2012).[18] We observe 126 school switches from not-online to online.

Earnings data are from the NSLDS College Scorecard database. The source of the data is a link between students and salaried (W-2) and self-employed (Schedule SE) earnings data from Department of the Treasury tax records. Wage outcomes cover only those individuals who (a) borrowed from the federal government and (b) were employees in the Social Security system or were self-employed and filed a tax return. Average and median wages are therefore likely higher than they would be if unemployed or out of the labor force graduates were included. Earnings are measured six years after cohort exit for the 1998, 2000, 2002, 2004, 2006 and 2007 cohorts. For each institution in these years, we have the mean and median wage. Average earnings for graduates of private equity-owned schools are $26,829 (in 2015 dollars). Earnings for graduates

[18]It indicates that the school either has "online" in its name, or has no one state constituting more than one-third of freshman enrollment. For-profits usually draw primarily from the surrounding area.

of community colleges are slightly higher, while they are slightly lower for graduates of other for-profits.

2.4 Law Enforcement Actions

Law enforcement actions against higher education institutions are informative about college operations. We found 125 instances in which a state or federal agency initiated an investigation.[19] The largest number of allegations relate to misrepresentation and false claims. For example, there are 28 cases of job placement statistic misrepresentation, 23 of credentials or accreditation misrepresentation, and 31 of other types of false claims. Violations of sales and recruiting regulations and fraud also feature prominently (44 allegations). Our analysis employs an indicator variable at the school-year level that is one if the school experienced its first law enforcement action that year, because some schools experience multiple allegations. There are 45 such first-time actions. Although private equity-owned school-years comprise just 4 percent of all school-years in our data, they are 58 percent of the first-time actions.

3 Primary Estimation Approaches

3.1 Visual Event Studies

We begin by showing the effects of a private equity buyout on outcomes using regressions with separate coefficients for each year around the buyout. We plot the estimated coefficients β_t and associated 95 percent confidence intervals from the following regression:

$$Y_{it} = \alpha + \sum_{j=-4}^{6} \beta_t PE_{it} + \gamma \mathbf{X}_{it} + \varepsilon_{it}. \quad (1)$$

Here, i indexes schools and t indexes years. $\mathbf{X}_{it}$ is a vector of controls comprising fixed effects for the highest degree that the school offers, whether the school is selective, and whether it is publicly

[19] These are described in Appendix B Table 3. We collected data primarily from Republic Report. https://www.republicreport.org/2014/law-enforcement-for-profit-colleges/ .

traded (formerly private equity-owned schools are not identified as private equity-owned after they IPO). We restrict the sample to schools that existed in the year before the private equity buyout, so that there is a change of ownership, and do not include schools that the private equity-owned school system establishes afterwards. After this restriction, there are small variations in sample size across years as schools enter and exit. A school that is not present in a given year for a given variable is recorded as missing. The coefficient plots test for pre-trends, which would indicate a selection mechanism in which any results partially reflect private equity firms' targeting schools on trajectories towards specific outcomes. They also demonstrate the effect among switcher-schools.

3.2 Within-school Regressions

To assess whether private equity buyouts are associated with changing student and operational outcomes, we use variants of the following specification:

$$Y_{it} = \alpha_i + \alpha_t + \beta_1 PE_{it} + \gamma \mathbf{X}_{it} + \varepsilon_{it}. \tag{2}$$

We include school fixed effects (α_i) and year fixed effects (α_t). $\mathbf{X}_{it}$ is the same as above. The sample consists of all institutions in our data. We include non-profits. PE_{it} takes a value of one if the school is private equity-owned in year t. Our main specification uses all years of available data, but all our main results are robust to excluding pre-2000 data, as there may be concern that it is lower quality. The results are also robust to restricting to switcher schools and collapsing the years on either side of the buyout into single averages, as suggested in Bertrand, Duflo, and Mullainathan (2004).

Beyond the controls described above, for each outcome variable we present a second model with additional controls for the demographic composition of the student body. These include family income (Pell grants per student in 2015 dollars) and the shares of students who are black, white, and Hispanic. We two-way cluster standard errors by parent company (SystemID) and year in all specifications. This captures potential correlation across schools within the largest deals. Our

results are not sensitive to alternative clustering approaches.

3.3 Buyout Predictors and Matching Estimator

Our third approach is a matching estimator. Besides showing the effects by year and thus demonstrating an absence of pre-trends (the approach in Section 3.1), this is the best available means to try to rule out selection.[20] To identify appropriate matching variables, we examine buyout predictors. For this exercise, the sample is restricted to other for-profits. Further, among the target schools, the sample is restricted to the year before the buyout. In a logit model with year fixed effects, we tested a wide variety of observables at the school and commuting zone (proxy for the local labor market) levels. Variables with predictive power are shown in Table 2. Private equity firms target schools in areas with more community colleges and a larger number of total enrolled students, but a smaller number of existing for-profits. This suggests they are identifying areas with large target populations but few competitors. They target schools that have lower recent profit growth but higher profits than the average for-profit school. They also target schools with more students, a higher share of students who are white, and that have lower loan repayment rates. No other variables consistently predict being a target. These include education inputs, enrollment growth at the school and commuting zone level, the proximity of revenue to the 90 percent threshold that is the legal maximum, and other student outcomes.

We deploy the variables with predictive power in a nearest-neighbor matching (NNM) estimator.[21] For each private equity-owned school, we match target schools in the pre-buyout year to other for-profits. We assess outcomes two years after the buyout in the matched sample.[22]

[20] In other panel event-study settings, Freyaldenhoven, Hansen, and Shapiro (2018)'s 2SLS method might be a promising alternative. However, their approach requires a strong pre-trend in a covariate that is a proxy for unobserved confounds. We do not observe strong pre-trends in observable, relevant covariates, so in our context the method is unlikely to identify the parameter of interest.

[21] The variables used are number of community colleges in the commuting zone, number of pre-existing for-profits in the commuting zone besides the target, one-year profit growth, log profits, the log number of FTE students, the 3-year loan repayment rate, and the share of students who are white. In the final specification (column 5 of Table 2), where all variables are used, the sample size declines and some variables lose significance. We nonetheless match on these, as they appear to have some predictive power.

[22] Unlike propensity score matching, which uses the logit estimated probability of treatment, NNM flexibly (i.e., with no functional form assumption) uses the distance between covariate patterns to define "closest" control for a

Appendix B Table 4 shows that the imbalance decreases dramatically after the NNM procedure. In addition to presenting regression results using the matched datasets, we also plot the means of outcome variables around the buyout for the matched target schools and the matched control schools (for which the buyout year is chosen at random from the target distribution). The latter plot serves as a placebo test.

4 Buyouts and Student Outcomes

4.1 Effect of Buyouts on Student Outcomes

This section employs the estimation strategies in Section 3 to examine the relationship between buyouts and measures of student success. We begin with one measure of school performance, the graduation rate, which is the share of students who graduate within 150 percent of the degree's normal time. In an experimental setting, Deming et al. (2016) show that employers prefer candidates with degrees from programs that have higher graduation rates. The graduation rate metric only includes full-time students (who are also the vast majority of students at for-profit colleges), so taking longer to get a degree does not mean that a student is working in the labor force and taking a light course load (see Gilpin and Stoddard 2017). Relatedly, Bound, Lovenheim, and Turner (2007) show that lower graduation rates do not reflect a longer time to degree or greater human capital acquisition (i.e., more credits); instead, longer times to degree are associated with dropping out and worse labor market outcomes.

In Table 3 Panel 1, we show that private equity buyouts lead to a six-percentage point decline in graduation rates, or about 13 percent of the mean across all schools. This relationship is consistent across our baseline model (column 1), the model with composition controls (column 2), and the matching estimator (column 3). Figure 4 Panel A contains the event study plot. Like all subsequent plots, it shows four years before the buyout and six years after. It omits the year

given treated observation. The flexibility requires more data, and the data required grows with each additional matched covariate. Therefore, we match only on the variables that have some predictive power (omitting the outcome variable if it is one of the matching covariates), and adjust for bias in matching on multiple continuous covariates.

before the buyout because these students are partially treated, which we examine below. Panel B contains the matched treated and control plots, which show four years before the buyout and four years after. (Confidence intervals widen in later years as there are fewer observations.) As the control schools are not matched on graduation rates, the means are not exactly aligned pre-buyout. This reflects the fact that private equity firms do not target schools with certain graduation rates. After matching on variables that do predict buyouts, we observe a slight discrepancy. However, this does not exist for the other main outcomes (Figures 5, 9, and 10). Both panels reveal a strikingly immediate negative effect on graduation rates. This is not surprising because the buyout year is the first affected academic year; as the majority of programs are one-year programs, operational changes can take effect quickly.

A lower graduation rate is unambiguously detrimental to those students who fail to graduate. It may also harm their peers who do graduate if the degree is perceived as lower quality by employers. Falling graduation rates could be profit maximizing for schools, however. Particularly for one-year programs, the school receives tuition from the government (and the student acquires her debt) when the student has been in class for just one week. If the student drops out, the school no longer bears the instructional, service, and facilities costs associated with her attendance.

Defaulting on student loans is also an adverse outcome relative to repaying for the vast majority of borrowers. This is in part because federal student debt is non-dischargeable in bankruptcy, and wages may be garnished. The share of students in repayment, shown in Table 3 Panel 3 columns 4-6, decreases after the buyout by at least three percentage points, relative to a mean across all schools of 53 percent. The matching estimate is larger, at 7.7 percentage points. The visual event study in Figure 5 Panel A shows a downward trend after the buyout. The fall is more dramatic in the matched sample (Panel B); while we see some decline in the control schools, the decline is nearly double in the treated matched schools.

Private equity buyouts lead to 5.8 percent lower within-cohort average earnings six years after enrollment, relative to a mean across all schools of $31,269, in 2015 dollars (Table 3 Panel 2 columns 1-2). Median earnings decline by a similar, albeit slightly smaller amount (columns

3-4). Data limitations prevent us from using the matching estimator.[23] Earnings exhibit strong time trends, increasing over most of our sample period and decreasing in the Great Recession. Therefore, we graph coefficients from a fixed effects regression. Figure 6 shows the coefficients β_j from the following specification:

$$lnWages_{it} = \alpha_i + \alpha_t + \sum_{j=-4}^{3} \beta_j 1[Year = Year_{PE} + j] + \varepsilon_{it} \tag{3}$$

Here, $1[Year = Year_{PE} + j]$ is an indicator of a year before or after the buyout year. The year before the buyout (-1) is the baseline, normalized to zero. The sample is all schools, and the control group is all non-private equity-owned schools. The results in Figure 6 contain no pre-trends and indicate a deterioration after the buyout in log earnings.

4.2 Selection

Private equity firm screening ability could explain the effects on graduation rates, repayment rates, and earnings. Such a selection mechanism would be a threat to a causal interpretation, implying that private equity firms choose targets that would have changed anyway (i.e., with no buyout). This is most plausible when the target firm is on a trajectory towards the post-buyout outcomes during the pre-buyout years.

Instead, the visual event studies presented in this section as well as subsequently when we discuss operational changes are largely devoid of pre-trends. They instead reveal discontinuities in levels and trends immediately around the buyout year. Further, the matching estimator, based on variables that predict private equity targets, finds similar results to the OLS model. Though we cannot entirely rule out some influence of selection, these two pieces of evidence indicate that a selection mechanism is very unlikely to fully explain the results.

[23] This is because we only observe six cohorts (as described in Section 2.3). We would need to match on the year prior to the buyout only for schools where, two years later, we have cohort wage data. There is inadequate data to conduct a match that improves meaningfully on the within-school, composition-controlled regressions.

4.3 Student Body Composition

Private equity ownership may change the type of students that enroll. For example, additional students targeted by the expansion may be less well qualified, with poorer labor market potential. This would be a causal effect of the buyouts but has potentially different implications for value-added. For no outcomes do demographic controls significantly attenuate the results. We also find no effect of private equity buyouts on Pell grants per students or the share of students on federal grants, and the small negative effect on the share of students who are white is not robust to the matching estimator (Appendix B Table 5). Therefore, observable demographic changes to the student body do not explain the main results.

We hold fixed composition using cohorts that are already enrolled at the school before the private equity acquisition occurs. We restrict the sample to two-year programs at ultimately private equity-owned schools. We compare the cohort that enrolled the year before the first private equity-owned year with the earlier cohort that enrolled two years before. The former cohort had one year of private equity treatment, while the latter had zero. We can conduct this test only for graduation and repayment rates.[24] The results are in Table 4. The partially treated cohorts experience a 3.5 percentage point decline in graduation rates, slightly more than half the main effect among fully-treated cohorts. The effect on repayment rates is the same as that among fully-treated cohorts. Thus, a changing student body composition cannot explain the declines in graduation and repayment rates.

There may be concern that private equity owners reduce degree offerings, which could explain the immediate fall in graduation rates in the year after the buyout. Students already enrolled in a program might be forced to drop out if the school cuts that degree. Composition changes might then explain the persistently lower graduation rates in the following years. We test this by identifying for each year the number of degree programs that are cut. Private equity ownership does not lead to cuts to degrees offered, so this cannot explain the immediate decline in

[24]There is inadequate earnings data (it only exists for six cohorts spaced three years apart). It is also not possible for student loans, considered below, because they are measured only in the cohort's first year, in which they are either fully treated or not treated at all.

graduation rates.[25]

4.4 Enrollment

The number of full-time equivalent enrolled students increases by 48 percent, shown in Table 5 columns 1-3. The visual event study and matched schools plot reveal large and steady increases starting in the second year after the buyout (Appendix B Figure 4). As buyouts have negative effects on student success measures, they are unlikely to make existing student types (i.e., that would have enrolled before the buyout) weakly better off. However, whether additional students – regardless of their preparedness – are better or worse off as a result of the buyout depends on their outside option. At one extreme, new students may be drawn from a population that would not attend college otherwise. These students may benefit, as those who graduate may experience higher earnings and better labor market opportunities relative to having no higher education at all. At the other extreme, private equity-owned schools may draw students away from institutions with higher value-added.

The closest substitutes are other for-profits and community colleges. A rich education economics literature finds strong evidence that (a) community colleges are an available substitute to for-profits, and (b) the returns to for-profit education are zero or negative relative to community college education.[26] Therefore, if new students at private equity-owned schools would otherwise attend community colleges, this would be some evidence that they are not better off.

We examine evidence for substitution at the commuting zone (CZ) level, which roughly corresponds to a local labor market. We regress the change in all community college enrollment (Δ^{96-16}Enrollment) within a CZ on the change in private equity (Δ^{96-16}PE Enrollment) in that

[25]We define a degree cut as a school-year in which there were no graduates of the degree, following a previous year with positive graduates. Appendix B Figure 3 shows the number of degree cuts by year around the private equity buyout, within schools that switched to private equity-owned. We separately consider one, two, and four-year programs. In no case is there an observable increase in the years following the buyout. Appendix B Table 6 confirms this in regression analysis.

[26]See Jacobson et al. 2005, Jepsen et al. 2014, Liu and Belfield (2014), Cellini and Chaudhary (2014), Darolia et al. (2015), Deming et al. (2016), Cellini and Turner (2016), Armona et al. (2017), and Cellini, Darolia, and Turner (2017). These papers are summarized in Appendix A Section 2.

CZ.[27] The results are in Table 6. If there is no substitution, we expect a coefficient of zero. Conversely, if there is full substitution, we expect a coefficient of -1. In column 1, the point estimate is -.67. The second row shows the results from an F-test that the coefficient is equal to -1; it reveals that we cannot reject full substitution away from community colleges. The second column repeats the analysis using full-time enrollment and finds similar results. Corresponding graphical evidence is in Appendix B Figure 5 Panel A. We do not expect substitution from high quality institutions to private equity-owned schools, so we use them in a placebo test. We define high quality institutions as those with graduation rates higher than 50 percent. There is no effect for high quality institutions (Table 6 columns 3 and 4 and Appendix B Figure 5 Panel B). Thus, the effects in columns 1 and 2 are not driven by general population or other sources of enrollment growth.

Figure 7 takes an event study approach within CZs. It shows increasing community college enrollment over time before the entry of a private equity-owned school, and a flat line afterward. Superior marketing may enable private equity-owned schools to siphon enrollment away from community colleges.[28] Consistent with this explanation, the targeting analysis in section 3.3 found that private equity firms tend to choose commuting zones with a higher number of community colleges. To the degree that federal sources finance substitution from community colleges to higher-tuition schools, taxpayers are also negatively affected.

In summary, while the data do not permit us to explicitly assess value-added or estimate welfare effects directly, private equity buyouts lead to declines in measures of student success. We find effects that are not driven by changing student body composition and cannot be fully explained by a selection mechanism. Further, additional students enrolled as a result of expansion after buyouts seem to be drawn away from attending community colleges, suggesting that they are not better off.

[27] There were 709 commuting zones in the United States in 2000. We have a lower number in our sample, as some commuting zones do not have colleges in the sample.

[28] One possibility is that private equity backed schools draw the worst students away from local community colleges. If this is the case, we would expect to see an improvement in education outcomes at community colleges after private equity entry. To explore this possibility, we examine graduation rates at community colleges in commuting zones following a private equity buyout. We find no significant effect on graduation rates. Results are available upon request.

5 Capturing Government Aid

The central question of this paper is whether – in a setting with information frictions and high levels of government subsidy – high-powered profit maximizing incentives induce focus on subsidy capture, rather than on student outcomes. As mentioned above, over 90 percent of total revenue at for-profits comes from government sources. In Section 5.1, we establish that higher-powered incentives lead to higher profits, and we examine aid-related student outcomes. The following three subsections test whether private equity-owned schools better capture government aid.

5.1 Financials

Consistent with existing private equity literature, we find in Table 7 that buyouts lead to higher profits.[29] The coefficient of 1.2 implies a 332 percent increase (Panel 1 columns 1-2), while the matching estimate is considerably smaller, at 150 percent. The large effect mirrors the summary statistics; average profits are $6 million among both for-profits and community colleges, while they are $34 million among private equity-owned schools. This industry is in general quite profitable; between 2003 and 2012, profits (gross margins) among U.S. publicly traded for-profit schools averaged 55 percent, compared to 33 percent across across 99 major industries (Eaton et al. 2016). The visual event study and matched schools plot reveal a significant increase in the first year after the buyout, and steady growth thereafter (Appendix B Figure 6).

Schools increase their reliance on federal aid after private equity buyouts. Figure 8 shows the share of school revenue from Title IV sources (most federal grant and loan programs) around the buyout. Before the buyout, target schools receive approximately 60-70 percent of their revenue from Title IV programs. The fraction increases to slightly above 80 percent six years after a buyout. To be eligible for Title IV, this fraction must remain below 90 percent. Figure 8 shows that the variance of the fraction of revenue from Title IV programs also decreases markedly after the buyout. The tight clustering just below the statutory cutoff for aid eligibility suggests that

[29] While non-profit and public institutions do not accrue profits, profits are measured as gross operating margins which occur across all ownership forms.

management more consistently or successfully targets the threshold.

If higher profits derive from increased capture of government aid, we expect that one source is higher tuition, and in turn that this tuition is funded by higher student loans and grants. Indeed, we find that tuition per student increases by over $1,600, relative to a mean across all schools of $9,528 (note tuition at community colleges averages just $3,673). This is shown in Table 7 Panel 1 columns 4-5. The effect doubles in the matching estimator, to $3,306 (column 3). The visual event study and matched schools plot in Figure 9 both show discontinuous increases in the year after the buyout, and growth in subsequent years. There are no pre-trends, and the pre-buyout control matched schools have almost identical average tuition as the pre-buyout target matched schools.

After the buyout, average loans per borrower increase by nearly $600 (in 2015 dollars), or about 12 percent of the mean across all schools of $5,147 (Panel 2 columns 1-2). The matching model yields a larger effect of $833 (column 3).[30] The visual event study and matched schools plot in Figure 10 mimic the tuition graphs, except that there is somewhat less variance in the loans so the confidence intervals are tighter. Again, there are no pre-trends, and the pre-buyout control matched schools have almost identical average loans as the pre-buyout target matched schools. Less than 10 percent of loans at private equity-owned schools are non-federal, and some of these come from state government. By comparison, 24 percent of loans are non-federal at non-profit schools.[31]

Four non-mutually exclusive mechanisms could explain higher borrowing. One is that students are poorer and thus need to borrow more conditional on tuition. However, we do not find effects on a proxy for low family income. The second is that the school's degree mix changes after the buyout, such that students enroll in higher cost degrees. However, we do not find significant changes to the degree mix. The third is that the school induces students to take out more loans relative to their out-of-pocket contribution. It is believed that for-profits often urge students to pay less out-of-pocket and more with loans, because the government payments are immediate and guaranteed (Cottom 2017). The fourth possibility is that tuition increases, but the degree mix remains the same. Since

[30]Note that tuition and loan amounts are not directly comparable, as loans are measured for full-time first-year students while tuition is measured across all students on a full-time equivalent basis.

[31]We also observe large increases of about $800 in non-Pell federal grants per student (Table 7 Panel 2 column 4-6). The visual event study and matched plot are in Appendix B Figure 7.

tuition increases by more than loans increase, this also seems a viable explanation.

5.2 The Effect of the 2007 Loan Limit Increase

A regulatory change in 2007 in which the government increased student loan borrowing limits created growth options for for-profits. In 2007, Congress raised the Stafford loan limits for all types of students for the first time since 1993. The increase occurred in two stages, with roughly one-third of the increase affecting the 2007–08 academic year, and the rest beginning with the 2008-09 academic year.[32] We examine whether schools already under private equity ownership were more responsive than their counterparts to this opportunity.

We compare private equity-owned schools to other for-profits using the difference-in-differences specification in Equation 4.

$$L_{it} = \alpha_i + \alpha_t + \beta PE_i * Post2007 + \gamma \mathbf{X}_{it} + \varepsilon_{it} \tag{4}$$

The term L_{it} denotes average borrowing or headline tuition in school i in year t. The coefficient of interest is β, which captures the change in average borrowing at private equity-owned schools relative to other schools after the limit increase. If private equity-owned schools are better at capturing aid, we would expect average loan amounts to rise at a faster rate relative to other institutions, and the coefficient β should be positive and significant. We include school and year fixed effects (α_i and α_t), and school controls X_{it} as in Equation 2. Standard errors are clustered at the SystemID level to address potential serial correlation. The year 2007 is excluded from the analysis, as the two reforms took place in 2007 and 2008 and thus it is somewhat ambiguous

[32]There are two types of caps; for annual borrowing and for total borrowing over the course of the degree. One limit increase took effect in 2007 and another that took effect in 2008. The 2006 Higher Education Reauthorization Act (HERA) took effect in 2007. It increased annual Stafford loan limits for freshmen, sophomores and graduate students by about $1,000, but did not increase aggregate per-student limits. The Ensuring Continued Access to Student Loans Act of 2008 increased annual and aggregate unsubsidized Stafford loan limits for undergrads by about $2,000. Note that these loans are non-dischargeable in bankruptcy. At the time of the legislation the rate was 6.8 percent for unsubsidized Stafford loans, and 3.4 percent for the smaller unsubsidized loans. GAO (2014) found no effect on tuition or loans, in part because the recession had a strong negative effect on private student lending, while Lucca et al. (2016) argue that the loan limits led to increases in tuition, which is consistent with the "Bennett hypothesis" that schools raise tuition to capture federal loans and grants.

when the treatment occurs. The results are not sensitive to including 2007.

The main identifying assumption of the analysis is that, in the absence of the limit increases, private equity-owned schools and other for-profit colleges would have had similar student borrowing trends. This assumption implies parallel trends before 2007. Appendix B Figure 8 restricts the treatment group to institutions that were private equity-owned prior to 2007. Before the 2007 limit increase, there are parallel trends between private equity and non-private equity-owned for-profits, with the latter persistently below the former. Following the increase in borrowing limits, the two series diverge, with a larger increase in average borrowing among private equity-owned schools.

Table 8 presents estimates of Equation 4. Consistent with the graphical evidence, the results indicate that after the loan limit increases, average borrowing increased by about $800 at private equity-owned institutions relative to other schools (Panel 1). Columns 1-3 include all schools, while columns 3-6 include only for-profits. Reflecting general increases in borrowing, the year trend is positive. The independent coefficient on being private equity-owned is also positive, as borrowing was higher at private equity-owned schools before the reform.

To further explore the timing of the effects, and to test the validity of the parallel trends assumption underlying the results, we estimate the following specification, interacting the private equity-ownership treatment with indicators for each year:

$$L_{it} = \alpha_i + \alpha_t + \sum_{j=2002}^{2012} \beta_j PE_i * 1[Year = j] + \gamma \mathbf{X}_{it} + \varepsilon_{it}. \tag{5}$$

The treatment is restricted to schools that were acquired by a private equity group before 2007. The results are plotted in top panel of Figure 11. The solid line shows point estimates of the coefficients β_j, while the area contains a 95 percent confidence interval. We do not observe any significant differences between the private equity-owned and other for-profit schools before 2007. The coefficients are near and not distinguishable from zero. After 2007, borrowing increases faster at private equity-owned schools, and this difference becomes significant at the .05 level three years after the reform.

It is possible that this increase in borrowing is beneficial to students. Goodman et al. (2017) find that many young borrowers are credit constrained and use student loans as an additional source of liquidity. However, students are unlikely to benefit from credit expansion if schools raise tuition commensurately. Table 8 Panel 2 presents regression results for tuition. We see sharp increases in tuition that completely offset the increase in borrowing. The bottom plot in Figure 11 also shows that there was no pre-trend; the timing of the limit increase coincides with the tuition hike.[33]

In summary, we find that private equity-owned schools raise tuition and borrowing at a faster rate following loan limit increases, consistent with these institutions being better at capturing government aid. Their superior ability to capture this strategic opportunity is also evidence of operational changes; in particular, different management that engages in rent-seeking behavior.

5.3 Cohort Default Rate Bunching

A key determinant of federal aid eligibility in force for decades is a limit on the extent to which students can default. Before 2012, the government's measure of default was the two-year cohort default rate (CDR). Specifically, the policy stated that the share of students who default in the fiscal year after the fiscal year in which they graduated cannot exceed 25 percent for three years in a row and cannot be higher than 40 percent in a single year. After 2012, the policy changed somewhat.[34] School survival depends on not triggering these thresholds. As explained in Section 2.3, it is known that CDRs are vulnerable to manipulation.

We find evidence that private equity-owned schools are better at avoiding the threshold. Appendix B Figure 9 shows the density of two-year cohort default rates by institution type. We restrict the sample to pre-2012, as the policy changed somewhat in that year. The solid line shows private equity-owned schools, and the other two lines show other for-profit and non-profit

[33]Further, Appendix B Table 7 shows no effect on the ratio of faculty to students, suggesting that the positive effects on tuition are not associated with improved education quality.

[34]In 2012, the CDR calculation was changed from a two-year to a three-year window (that is, default is now measured in the second fiscal year after graduation). To partially compensate for this more onerous policy, the 25 percent was changed to 30 percent. The rule change was expected to be detrimental to for-profit colleges (see e.g. http://www.finaid.org/loans/cohortdefaultrates.phtml).

schools. The vertical line is the 25 percent two-year CDR threshold. CDRs largely evolve smoothly across the threshold among independent for-profits and other schools. In contrast, there is a sharp drop in the default density right before the threshold at private equity-owned schools, consistent with avoiding the threshold. This helps explain why private equity-owned schools have slightly lower CDRs than other for-profits in Table 1 Panel 2.

5.4 Gainful Employment Announcement

We are interested in the sensitivity of a school's profit to its ability to access federal aid regardless of student outcomes. One approach is to examine whether the school's valuation responds to surprise regulatory events that would affect this access. The Gainful Employment (GE) rule aimed to tie a school's Title IV eligibility to student labor market performance. The rule was announced in 2010, watered down following court cases, and ultimately suspended in 2017. Since private equity-owned schools have illiquid and unobservable value, we turn to publicly traded schools to study the effect of the GE rule. Many of the largest public firms were once private equity-owned, including Devry, EDMC, and Corinthian. Others have been acquired by private equity in public-to-private reverse LBOs, such as Apollo, which owns the University of Phoenix. Details about these events are in Appendix C Table 1.

We find that the market values of publicly traded for-profits fell sharply when the GE rule was announced. Conversely, affected firms experienced positive abnormal returns when the rules were weakened in 2011. Appendix C contains a detailed explanation of the rule, our estimation approach, and the results. Appendix C Figure 1 shows dramatic changes in abnormal returns around the events, while there are no changes for a group of control firms. Difference-in-differences regressions confirm the effect (Appendix C Table 2). This analysis suggests that a major component of for-profit market value is rent-seeking capture of government aid.

In sum, superior federal aid capture is an important channel through which high-powered incentives translate to higher profits. Firm focus on maximizing revenue from subsidies may help

explain why we do not observe improvements in student outcomes accompanying buyouts. Greater rent-seeking capture of government aid is unambiguously not in the public interest and is relevant to education policy and public good procurement more generally.

6 Operational Mechanisms

This section explores operational mechanisms that may explain the deterioration in student outcomes observed in Section 4, and the increase in revenue and profits shown in Section 5. Further, we conduct several robustness tests in Section 6.5.

6.1 Education Inputs

One possible mechanism is that education quality declines. Though we do not observe education quality directly, we show that measures of education inputs fall after private equity buyouts in Table 9. The number of faculty per 100 full time students falls by 0.45 (Panel 1 column 1), relative to a mean of 5.3 across all schools, and 3.6 among private equity-owned schools. There is also an absolute decline in the number of faculty (Panel 1 columns 4-6). The share of expenditure devoted to instruction declines by about three percentage points (Panel 2 columns 1-2), relative to a mean across all schools of 48 percent. Similarly, absolute instruction spending declines, conditional on enrollment (Panel 2 columns 4-5). For all the education input variables, the matching estimates are imprecise, but their magnitudes are in all cases in the same direction as the main estimates, and much larger for faculty per 100 students and instruction spending. Their lack of statistical significance may reflect the small samples for these variables. The visual event study and matched schools plot confirm the regression evidence for both faculty and instruction spending (Appendix B Figures 10 and 11).

The decline in education inputs is consistent with case studies in a U.S. Senate report on private equity-owned for-profits (Senate 2012). In Appendix A Section 4, we summarize the report's evidence that reductions in student support following private equity buyouts had negative

impacts on educational quality with implications for student outcomes. Our own interviews with two former high level for-profit college managers also support this mechanism. For example, a former high-level manager with Florida Career College said that after a 2012 buyout, "they started decimating faculty and student services." Relatedly, Bound, Lovenheim, and Turner (2010) argue that lower institutional resources per student have contributed more than compositional changes to the overall decline in college graduation rates, and find that declines in the faculty to student ratio account for over three-quarters of graduation rate reductions in their sample.

Following Bound et al. (2010), we seek to link education inputs to graduation rate declines. The effects we find on graduation rates – highlighted by the partially treated cohort analysis – are very immediate. If immediate education quality declines are associated with immediate graduation rate declines within a school, this would support the hypothesis that operational changes are responsible for deteriorating outcomes. We examine whether changes in graduation rates in the year after the buyout year are correlated with changes in education inputs in the same year. Appendix B Figure 12 Panels A and B reveal a strong positive relationship; schools that decrease their faculty-to-student ratio or instruction share of spending experience graduation rate declines, while schools that increase these education inputs experience graduation rate increases. Regressions in Appendix B Table 8 also find that the negative effect of private equity on graduation rates in the year after the buyout is much larger for schools with large negative changes in their faculty to student ratios.[35]

Tuition hikes could also explain the fall in graduation rates. However, there is evidence that students are quite price inelastic because the size of their loan package is not salient at the time of borrowing, and they have no reason to be sensitive to grant amounts (e.g. Bleemer et al. 2017). Indeed, we find no relationship between changes in tuition and graduation rates immediately around the buyout year (Appendix B Figure 12 Panel C).

[35]However, the interaction between PE and an indicator for change in faculty being below 25th percentile is not significant (column 3). For instruction share of spending, the magnitude of the coefficient is much larger among schools with large negative changes (columns 4 and 5), though both are noisy. The interaction specification yields a large coefficient of -.06, significant at the .1 level, suggesting that schools with relatively large, immediate cuts in instruction spending share (<-.018) experience about twice the decline in graduation rates as other private equity-owned schools.

6.2 Law Enforcement Actions

Recall from Section 4.4 that buyouts have large positive effects on enrollment. It may initially seem inconsistent that education quality and student outcomes decline yet demand increases. The higher share of employees in sales (Figure 2) suggests that private equity-owned schools improve sales and marketing operations to attract more students. The law enforcement actions at private equity schools are primarily related to recruiting violations, including predatory and misleading marketing, and the use of excessive commissions or quotas for salespeople. These actions provide a second source of evidence that for a product with opaque quality, aggressive recruiting may be a successful strategy.

The chances of a school having its first law enforcement action increases dramatically after a private equity buyout. The dependent variable in columns 4-5 of Table 5 is one if the school experienced its first action in a given year. The coefficient is .003, significant at the .01 level, relative to a mean of .004. We have only 58 such instances (of which private equity-owned schools were responsible for 41), so there are insufficient observations for the matching estimator.[36] There may be concern about endogeneity in the law enforcement actions, many of which occurred between 2010 and 2014; perhaps the federal government targeted private equity-owned schools for political reasons. Such politicization is less likely at the state level, where cases typically originate directly from student complaints. We therefore limit the law enforcement actions to those brought by state attorneys general in column 6. The effect persists, though it is attenuated.

6.3 Online Schools

Online education, with its low marginal costs per student, presents opportunities for economies of scale. We examine whether private equity ownership is associated with a transition from being primarily a brick-and-mortar school to being an online school in Table 5 columns 7–8. We find a positive, albeit imprecise effect of 1.2 percentage points. As above, the small number of schools

[36] A visual comparison is in Appendix B Figure 13.

that switch to being online prevents us from using the matching estimator. We find no effect on the intensive margin (number of out-of-state students). Thus taking schools online is one way that buyouts increase enrollment, but it affects a very small number of schools.

6.4 Governance

Private equity investors often change governance in their portfolio companies (Cornelli and Karakas 2008, Kaplan and Strömberg 2009, Bloom et al. 2015). Gompers, Kaplan, and Mukharlyamov (2016) find that 31 percent of private equity investors recruit their own senior management teams before investing, which then replace the pre-buyout management team. We expect that private equity buyouts may affect operations through changes in management.

We test this hypothesis in Appendix B Table 9. The dependent variable is an indicator for whether a school's Chief Executive changes within three years of the buyout.[37] Our most conservative model uses school and year fixed effects, controls for composition, and limits the sample to for-profits. This model (column 6) finds a 3.8 percentage point effect. The sample mean is 10.5 percent, indicating that private equity buyouts, using the more conservative estimates, increase CEO turnover by around 36 percent. This is consistent with private equity firms changing management by bringing in new executives. Therefore, new management is one channel for changed operations.

6.5 Robustness

6.5.1 PE Compared to Chain Acquisitions

A final exercise examines whether the effects we observe occur following transitions from independent to chain ownership more generally. We define a chain as any parent company (SystemID) that is not private equity-owned and that owns at least two schools (UnitIDs). These

[37] College Chief Executives are defined in IPEDS. They are typically university presidents or other senior academic officials. We define a Chief Executive change as an indicator of whether the last name of the Chief Executive listed in IPEDS changes from the previous year.

ownership changes, as well as PE buyouts, are included as separate indicator variables in versions of Equation 2.

The results for our primary outcome variables are in Table 10. For graduation rates, repayment rates, average loans, tuition, and the faculty to student ratio, the effects of chain acquisitions are small and imprecise, particularly relative to the effects of private equity buyouts. However, chain acquisitions have a substantial negative effect on earnings (column 3), and a positive effect on enrollment (column 7), though both are smaller than the effects of private equity buyouts. These results suggest a hierarchy of incentives and outcomes. Chains likely have more sophisticated corporate structures and arms-length owners than stand-alone, independent schools, giving them somewhat higher-powered incentives. Private equity ownership yields higher-powered incentives and leads to more adverse student outcomes.

6.5.2 Private Equity Firm Variation

We also examined how the private equity firms behind the deals may affect outcomes. First, we find very similar results to the main model when we include lead private equity firm fixed effects. Second, we examined whether our effects vary by private equity firm characteristics, such as having a specialty in education, or being especially high- or low-performing. We found no variation in the effects by these measures.[38] Third, we omit the largest three deals. We define "large" as the number of schools (UnitIDs) purchased in the deal and subsequently acquired by the private equity-owned school system.[39] The results are generally as strong as our main specification, both in magnitude and statistical significance.

6.5.3 Restriction to post-2000 data

There is concern that school data quality improved post-2000, and that this may affect our results. In Appendix B Table 10, we present the main effects on student outcomes using only post-2000

[38]The results of both these exercises are available on request.

[39]These are Empire Beauty Schools, which ultimately consisted of 82 schools, Corinthian (63 schools), and EDMC (49 schools).

data and the OLS specifications. The effects are extremely similar to those in Table 3, and the coefficients are statistically indistinguishable from our main results. We created similar tables for all our outcomes and the cohort partial treatment analysis, and continued to find that the results are broadly robust to excluding pre-2000 data. These tables are available upon request.

7 Conclusion

For-profit schools were originally based on an implicit contract: In exchange for federal grant and loan inputs, the school would increase the human capital of its students. This relates to the implicit contracts discussed in Shleifer and Summers (1988). They argue that hostile takeovers increase firm market value because they enable a transfer of rents from stakeholders (e.g., employees) to shareholders, and that such redistribution can destroy value from a social perspective. The stakeholder in our setting is the consumer – students and the government. From the private equity investor's perspective, it may be ex-post optimal to renege on the implicit contract. In fact, students and the government differ from employees in ways that may increase the appeal of reneging; students typically purchase a program's degree only once, and the government has largely not been a demanding counter-party. New shareholders can maximize value by reducing quality and increasing cost.

Indeed, we find that private equity buyouts lead to expanded enrollment and increased profits, but also to higher tuition, lower education inputs, lower graduation rates, higher student borrowing, lower repayment rates, and lower wage earnings. We also use regulatory changes to show that private equity-owned schools raise tuition following credit expansions faster than other schools, which leads to increased levels of debt. Further, we show that changed operations appear to lead to the detrimental effects on student outcomes.

We cannot directly assess the welfare effects of buyouts as we do not observe school value-added and buyouts are not randomly assigned, but the sum of the evidence points to high-powered incentives to maximize profit in the education industry operating counter to consumers' interest.

We demonstrate that an important channel for the better performance of private equity-owned schools is superior capture of government aid, suggesting that intensive government subsidy leads to the misalignment of incentives. Future research in multiple sectors is needed to understand how high-powered incentives interact with other potentially relevant characteristics, such as product opacity.

References

Arcidiacono, P., E. Aucejo, A. Maurel, and T. Ransom (2016). College Attrition and the Dynamics of Information Revelation. *Working Paper*.

Armona, L., R. Chakrabarti, and M. F. Lovenheim (2017). How does for-profit college attendance affect student loans, defaults and earnings?

Bernstein, S. and A. Sheen (2016). The operational consequences of private equity buyouts: Evidence from the restaurant industry. *Review of Financial Studies 9*(29), 2387–2418.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Bettinger, E. P., B. T. Long, P. Oreopoulos, and L. Sanbonmatsu (2012). The role of application assistance and information in college decisions results from the H&R block fafsa experiment. *The Quarterly Journal of Economics 127*(3), 1205–1242.

Bleemer, Z., M. Brown, D. Lee, K. Strair, and W. van der Klaauw (2017). Echoes of rising tuition in students borrowing, educational attainment, and homeownership in post-recession america. *Working Paper*.

Bloom, N., R. Sadun, and J. Van Reenen (2015). Do private equity owned firms have better management practices? *The American Economic Review 105*(5), 442–446.

Boucly, Q., D. Sraer, and D. Thesmar (2011). Growth LBOs. *Journal of Financial Economics 102*(2), 432–453.

Bound, J., M. Lovenheim, and S. Turner (2007). Understanding the decrease in college completion rates and the increased time to the baccalaureate degree. *Population Studies Center Research Report 7*, 626.

Bound, J., M. F. Lovenheim, and S. Turner (2010). Why have college completion rates declined? an analysis of changing student preparation and collegiate resources. *American Economic Journal: Applied Economics 2*(3), 129–57.

Brown, G. W., O. Gredil, and S. N. Kaplan (2013). Do private equity funds game returns? *Working Paper*.

Cao, J. and J. Lerner (2009). The performance of reverse leveraged buyouts. *Journal of Financial Economics 91*(2), 139–157.

Carey, K. (2017, oct). Revised Data Shows Community Colleges Have Been Underappreciated.

Cellini, S., R. Darolia, and L. Turner (2017). Where do students go when for-profit colleges lose federal aid? *Working Paper*.

Cellini, S. R. and L. Chaudhary (2014). The labor market returns to a for-profit college education. *Economics of Education Review 43*, 125–140.

Cellini, S. R. and C. Goldin (2014). Does federal student aid raise tuition? New evidence on for-profit colleges. *American Economic Journal: Economic Policy 6*(4), 174–206.

Cellini, S. R. and N. Turner (2016). Gainfully employed? Assessing the employment and earnings of for-profit college students using administrative data. Technical report, National Bureau of

Economic Research.

CFBP (2012). Private student loans report. *Report to the U.S. Senate*.

Cornelli, F. and O. Karakas (2008). Private equity and corporate governance: Do lbos have more effective boards? *Working Paper*.

Cottom, T. M. (2017). *Lower ed: The troubling rise of for-profit colleges in the new economy*. New Press, The.

Darolia, R., C. Koedel, P. Martorell, K. Wilson, and F. Perez-Arce (2015). Do employers prefer workers who attend for-profit colleges? Evidence from a field experiment. *Journal of Policy Analysis and Management 34*(4), 881–903.

Davis, S. J., J. Haltiwanger, K. Handley, R. Jarmin, J. Lerner, and J. Miranda (2014). Private equity, jobs, and productivity. *The American Economic Review 104*(12), 3956–3990.

Deming, D. J., C. Goldin, and L. F. Katz (2012, dec). The for-profit postsecondary school sector: Nimble critters or agile predators? *Journal of Economic Perspectives 26*(1), 139–64.

Deming, D. J., N. Yuchtman, A. Abulafi, C. Goldin, and L. F. Katz (2016). The value of postsecondary credentials in the labor market: An experimental study. *The American Economic Review 106*(3), 778–806.

DOE (2011). Committee on Measures of Student Success: A Report to Secretary of Education Arne Duncan. Technical report, U.S. Department of Education, Washington, DC.

Duggan, M. G. (2000). Hospital ownership and public medical spending. *The Quarterly Journal of Economics 115*(4), 1343–1373.

Eaton, C., J. Habinek, A. Goldstein, C. Dioun, D. G. Santibáñez Godoy, and R. Osley-Thomas (2016). The financialization of us higher education. *Socio-Economic Review 14*(3), 507–535.

Fracassi, C., A. Previtero, and A. Sheen (2017). Is private equity good for consumers? *Working Paper*.

Freyaldenhoven, S., C. Hansen, and J. M. Shapiro (2018). Pre-event trends in the panel event-study design. Technical report, National Bureau of Economic Research.

GAO (2014). Federal Student Loans: Impact of Loan Limit Increases on College Prices Is Difficult to Discern. *United States Government Accountability Office Report GAO-14-7*.

Gilpin, G. and C. Stoddard (2017). Does regulating for-profit colleges improve educational outcomes? what we know, what we don't know, and what we need to find out. *Journal of Policy Analysis and Management 36*(4), 942–950.

Glaeser, E. L. and A. Shleifer (2001). Not-for-profit entrepreneurs. *Journal of Public Economics 81*(1), 99–115.

Gompers, P., S. N. Kaplan, and V. Mukharlyamov (2016). What do private equity firms say they do? *Journal of Financial Economics 121*(3), 449–476.

Goodman, S., A. Isen, and C. Yannelis (2017). A day late and a dollar short: Limits, liquidity and household formation for student borrowers. *Working Paper*.

Guo, S., E. S. Hotchkiss, and W. Song (2011). Do buyouts (still) create value? *The Journal of*

Finance 66(2), 479–517.

Hansmann, H. B. (1980). The role of nonprofit enterprise. *The Yale Law Journal 89*(5), 835–901.

ICAS (2012). Steps the Education Department Should Immediately Take to Curb Default Rate Manipulation. Technical report, The Institution for College Access & Success, Washington, DC.

Jacobson, L., R. LaLonde, and D. G. Sullivan (2005). Estimating the returns to community college schooling for displaced workers. *Journal of Econometrics 125*(1), 271–304.

Jaquette, O. and E. E. Parra (2014). Using IPEDS for Panel Analyses: Core Concepts, Data Challenges, and Empirical Applications. In *Higher Esducation: Handbook of Theory and Research*, pp. 467–533. New York: Springer.

Jensen, M. C. (1989). Eclipse of the public corporation. *Harvard Business Review*.

Jepsen, C., K. Troske, and P. Coomes (2014). The labor-market returns to community college degrees, diplomas, and certificates. *Journal of Labor Economics 32*(1), 95–121.

Kaplan, S. (1989). The effects of management buyouts on operating performance and value. *Journal of Financial Economics 24*(2), 217–254.

Kaplan, S. N. and P. Strömberg (2009). Leveraged buyouts and private equity. *The Journal of Economic Perspectives 23*(1), 121–146.

Kelchen, R. (2017). How much do for-profit colleges rely on federal funds? *Brookings Institution Chalkboard*.

Kerr, W. R., R. Nanda, and M. Rhodes-Kropf (2014). Entrepreneurship as experimentation. *Journal of Economic Perspectives 28*(3), 25–48.

Krishnan, K. and P. Wang (2017). The cost of financing education: Can student debt hinder entrepreneurship? *Working Paper*.

Lerner, J. and A. Schoar (2005). Does legal enforcement affect financial transactions? the contractual channel in private equity. *The Quarterly Journal of Economics 120*(1), 223–246.

Liu, Y. T. and C. Belfield (2014). The Labor Market Returns to For-Profit Higher Education: Evidence for Transfer Students. A CAPSEE Working Paper. *Center for Analysis of Postsecondary Education and Employment*.

Ljungqvist, A., L. Persson, and J. Tag (2016). Private equity's unintended dark side: On the economic consequences of excessive delistings. *Working Paper*.

Looney, A. and C. Yannelis (2015). A crisis in student loans? How changes in the characteristics of borrowers and in the institutions they attended contributed to rising loan defaults. *Brookings Papers on Economic Activity*, 1–89.

Lucca, D. O., T. Nadauld, and K. Chen (2016). Credit supply and the rise in college tuition: Evidence from the expansion in federal student aid programs.

Matsa, D. A. (2011). Running on empty? financial leverage and product quality in the supermarket industry. *American Economic Journal: Microeconomics 3*(1), 137–73.

Metrick, A. and A. Yasuda (2010). The economics of private equity funds. *The Review of Financial*

Studies 23(6), 2303–2341.

Muscarella, C. J. and M. R. Vetsuypens (1990). Efficiency and organizational structure: A study of reverse LBOs. *The Journal of Finance 45*(5), 1389–1413.

Senate (2012). For Profit Higher Education: The Failure to Safeguard the Federal Investment and Ensure Student Success.

Shleifer, A. and L. H. Summers (1988). Breach of trust in hostile takeovers. In *Corporate Takeovers: Causes and Consequences*, pp. 33–68. University of Chicago Press.

Wiswall, M. and B. Zafar (2014). Determinants of college major choice: Identification using an information experiment. *Review of Economic Studies 82*(2), 791–824.

Table 1: Descriptive Statistics by Institution Type

	Nonprofit, State	**Community**	**For profit, not PE**	**PE owned**
	Mean (Std Dev)	**Mean (Std Dev)**	**Mean (Std Dev)**	**Mean (Std Dev)**
Profits (mill 2015$)	40	6	6	34
	(102)	(16)	(34)	(63)
Publicly traded	0.00	0.00	0.14	0.000
	(0.000)	(0.00)	(0.35)	(0.00)
Selective admissions	0.68	0.089	0.092	0.077
	(0.467)	(0.29)	(0.29)	(0.27)
Highest degree offered**	1.12	2.12	2.32	2.01
	(0.40)	(0.33)	(0.75)	(0.723)
Graduation rate	0.52		0.55	0.48
	(0.21)		(0.25)	(0.21)
Repayment rate (3 year)†	0.66	0.47	0.41	0.32
	(0.17)	(0.121)	(0.16)	(0.13)
Mean earnings after school (2015 $)	37,667	28,321	24,275	26,829
	(11,117)	(4,920)	(7,959)	(8,219)
Full-time faculty per 100 students±	6.28	4.38	4.48	3.62
	(4.653)	(4.26)	(4.1)	(2.66)
Full-time faculty	261	109	16.8	24.8
	(465)	(110)	(32.2)	(45)
Share spending on instruction	0.47	0.54	0.41	0.36
	(0.14)	(0.13)	(0.24)	(0.15)
Spending on instruction (mill 2015 $)	71.4	19.5	2.31	4.55
	(219)	(26.9)	(6.01)	(7.37)
Students‡	3,885	3,148	387	748
	(5,656)	(3,866)	(1,232)	(1,413)

Continued on following page

Table 1 continued

	Nonprofit, State	Community	For profit, not PE	PE owned
	Mean (Std Dev)	**Mean (Std Dev)**	**Mean (Std Dev)**	**Mean (Std Dev)**
1st law enforcement action	0.00	0.00	0.00	0.004
	(0.007)	(0.006)	(0.018)	(0.061)
Online	0.000	0.000	0.012	0.017
	(0.000)	(0.000)	(0.109)	(0.127)
Share students white	0.67	0.69	0.51	0.44
	(0.28)	(0.25)	(0.32)	(0.26)
Loan per borrower (2015 $)	5,179	3,543	5,711	7,456
	(2,320)	(1,911)	(2,822)	(2,719)
Tuition revenue per student (2015$)	10,995	3,673	14,210	17,521
	(7,110)	(3,883)	(7,678)	(7,303)
Pell grants per student (2015 $)	1,350	1,725	4,109	4,609
	(1,682)	(1,292)	(3,193)	(3,104)
Federal grants per student (2015$)	1,980	2,335	6,115	5,814
	(2,183)	(2,219)	(5,816)	(5,152)
N (school-year obs)	55,103	29,678	34,286	4,540

Note: This table contains summary statistics at the school (UnitID, or campus) level. The exception is profits, which are at the firm/institution-year (SystemID) level; from left, N=47,834; 23,929; 8,254; and 438. ‡Full-time equivalent (applies to all below). *Grad rate at 150pct normal time for programs of 2 years or less duration. $^{\pm}$Full time faculty. †Share of students in repayment after three years (have paid back at least $1 in principal). **Highest degree offered is 1 for 4-year degrees and higher, 2 for 2-year degrees, and 3 for less-than-2-year degrees and certificates.

Table 2: Private equity targeting

Dependent variable: Indicator for school experiencing PE buyout in following year

	(1)	(2)	(3)	(4)
Community colleges in CZ	.022**			.0043
	(.0093)			(.015)
For-profits in CZ	-.014***			-.018**
	(.0044)			(.0072)
Log FTE students in CZ	.12*			.46***
	(.065)			(.11)
Profit growth (last year)	-.00013*		-.00014**	-.000016
	(.000076)		(.000068)	(.00022)
Log profits	.093***		.051*	.27***
	(.032)		(.028)	(.052)
Log FTE students	.65***		.7***	.24***
	(.046)		(.042)	(.073)
3-yr repayment rate		-4.9***		-4***
		(.4)		(.55)
Share students white			.26*	.59**
			(.15)	(.29)
Year f.e.	Y	Y	Y	Y
N	28,250	14,846	35,388	11,472
Pseudo R^2	.15	.092	.14	.14

Note: This table shows estimates from logit regressions in which the dependent variable is an indicator for the school-year immediately preceding a private equity buyout. All other years for target schools are excluded from the sample. The sample is restricted to for-profit, non-publicly traded schools. Only variables with predictive power over buyouts are shown.

Table 3: Private Equity Ownership and Student Outcomes

Panel 1

Dependent variable:	Graduation rate (share graduate in 150% normal time)			Repayment rate (3 year)		
	OLS		NNM$^{\pm}$	OLS		NNM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)
PE buyout	-.06***	-.059***	-.071**	-.033**	-.031*	-.077***
	(.012)	(.012)	(.031)	(.012)	(.011)	(.011)
Composition controls‡	N	Y	-	N	Y	-
School type controls†	Y	Y	-	Y	Y	-
School Fixed Effects	Y	Y	-	Y	Y	-
Year Fixed Effects	Y	Y	-	Y	Y	-
N	56,965	56,839	3,458	19,746	19,746	12,663
R^2	0.8	0.81	-	0.96	0.96	-

Panel 2

Dependent variable:	Log mean earnings		Log 50th pctile earnings	
	(1)	(2)	(3)	(4)
PE buyout	-.056**	-.046**	-.052**	-.041*
	(.013)	(.012)	(.017)	(.016)
Composition controls‡	N	Y	N	Y
School type controls†	Y	Y	Y	Y
School Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
N	16,861	16,861	16,861	16,861
R^2	0.97	0.97	0.96	0.97

Note: These panels show regression estimates (OLS) of the effect of private equity ownership on student outcomes, at the school (UnitID)-year level. $^{\pm}$Nearest-neighbor matching is done within the sample of other for-profit schools. The dependent variable is measured the year after the treated school's buyout. Matching is exactly on the year before the treated school's buyout, and then on characteristics (see Section 3.3). ‡We control for the share of students who are white, black, and Hispanic, as well as the average amount of federal Pell grants per student, a proxy for low-income students. †Indicators for having selective admissions, public ownership, and fixed effects for highest degree offered. The latter includes less than 2-year (certificate), 2-year, or 4-year. Standard errors two-way clustered by SystemID and year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 4: Cohort Partial Treatment Effect of Private Equity Ownership on Student Outcomes

Dependent variable:	Graduation rate (share graduate in 150% normal time)	Repayment rate (3 year)
	(1)	(2)
PE buyout (partially treated cohort)	-.035**	-.035**
	(.013)	(.014)
School type controls†	Y	Y
School Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
N	737	644
R^2	0.78	.87

Note: These panels show regression estimates (OLS) of the partial treatment effect of private equity ownership on student outcomes, at the school (UnitID)-year level. We limit the sample to two cohorts in two-year programs: the cohort that enrolled in the year before the first private equity-owned year, and the cohort that enrolled two years before the first private equity-owned year. The variable "PE owned" is one for the former cohort, which had one year of private equity treatment, and zero for the earlier cohort, which had no private equity treatment. †Indicators for having selective admissions and public ownership. Standard errors two-way clustered by SystemID and year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 5: Private Equity Ownership and Operational Outcomes

Dependent variable:	Log FTE students			1st law enforcement action		1st AG law enf. action	Online	
	OLS		NNM$^{\pm}$	OLS			OLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PE owned	.39***	.37***	.34**	.0031***	.0031***	.0012*	.012*	.012*
	(.057)	(.055)	(.14)	(.00074)	(.00073)	(.00065)	(.0063)	(.0063)
Composition controls‡	N	Y	-	N	Y	N	N	Y
School type controls†	Y	Y	-	Y	Y	Y	Y	Y
School Fixed Effects	Y	Y	-	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	-	Y	Y	Y	Y	Y
N	123,052	123,052	13,062	123,052	123,052	123,052	123,052	12,3052
R^2	0.97	0.97	-	0.14	0.14	.078	.58	.58

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school operational outcomes. Observations are at the school (UnitID)-year level. $^{\pm}$Nearest-neighbor matching is done within the sample of other for-profit schools. The dependent variable is measured the year after the treated school's buyout. Matching is exactly on the year before the treated school's buyout, and then on characteristics (see Section 3.3). ‡We control for the share of students who are white, black, and Hispanic, and the average amount of federal Pell grants per student, a proxy for low-income students. †These are indicators for having selective admissions, public ownership, and are fixed effects for highest degree offered. The latter includes less than 2-year (certificate), 2-year, or 4-year. Standard errors two-way clustered by SystemID and year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 6: Relationship Between Entry and Community College Enrollment

	Community Colleges		High Quality Schools	
	Δ^{96-16} Enrollment	Δ^{96-16} FTE	Δ^{96-16} Enrollment	Δ^{96-16} FTE
	(1)	(2)	(3)	(4)
Δ^{96-16} PE Enrollment	-0.67**		1.09	
	(0.3)		(0.90)	
Δ^{96-16} PE FTE		-1.121**		0.9
		(0.49)		(0.7)
P-Value (=-1)	.27	.81	.0036	.002
Observations	451	451	301	301
R^2	.03	.03	.10	.09

Note: This table shows the relationship between changes in private equity-owned and community college enrollment at the commuting zone level between 1996 and 2016. Columns 1 and 3 include all enrollment, while columns 2 and 4 include only full time enrollment. Columns 3 and 4 are placebo tests, which replace community college enrollment with enrollment at institutions that graduate more than half of their students with 150% of the normal time ("high quality schools"). We also show the p-value from an F-test that the coefficient equals -1, which is consistent with full substitution. Community colleges are defined as public institutions granting two year or lower degrees. Huber-White robust standard errors are presented in parentheses. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 7: Private Equity Ownership and Financial Outcomes

Panel 1

Dependent variable:	Log profits			Tuition per student		
	OLS		NNM$^{\pm}$	OLS		NNM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)
PE buyout	1.2***	1.1***	.4*	1610**	1637***	3306***
	(.22)	(.22)	(.22)	(607)	(565)	(1039)
Composition controls‡	N	Y	-	N	Y	-
School type controls†	Y	Y	-	Y	Y	-
School Fixed Effects	Y	Y	-	Y	Y	-
Year Fixed Effects	Y	Y	-	Y	Y	-
N	80,119	80,119	10,804	102,354	102,354	5,193
R^2	0.83	0.83	-	0.82	0.84	-

Panel 2

Dependent variable:	Loan per borrower			Federal grants per student		
	OLS		NNM$^{\pm}$	OLS		NNM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)
PE buyout	586***	592***	833**	837***	784***	1267*
	(185)	(185)	(374)	(176)	(219)	(746)
Composition controls‡	N	Y	-	N	Y	-
School type controls†	Y	Y	-	Y	Y	-
School Fixed Effects	Y	Y	-	Y	Y	-
Year Fixed Effects	Y	Y	-	Y	Y	-
N	75,022	75,022	11,482	86,412	86,412	12,333
R^2	0.65	0.65	-	.53	.55	-

Note: This table shows estimates of the effect of private equity ownership on financials (panel 1) and on government aid-related student outcomes (panel 2). Dependent variables are in millions of 2015$ in panel 1, and 2015$ in panel 2. $^{\pm}$Nearest-neighbor matching is done as in previous tables. Observations are at the SystemID-year level for profits, and the UnitID-year level elsewhere. ‡We control for the share of students who are white, black, and Hispanic, and the average amount of federal Pell grants per student, a proxy for low-income students. †These are indicators for having selective admissions, public ownership, and are fixed effects for highest degree offered. The latter includes less than 2-year (certificate), 2-year, or 4-year. Standard errors two-way clustered by SystemID and year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 8: Effect of Loan Limit Increase

Panel 1: Borrowing

Dependent Variable: Average loan per borrower (2015$)

	(1)	(2)	(3)	(4)	(5)	(6)
PE owned·Post 2007	824.5***	790.7***	786.0***	591.2***	663.3***	656.4***
	(116.0)	(131.8)	(131.4)	(120.8)	(144.0)	(261.4)
PE owned	1501.9***			800.2***		
	(97.88)			(97.93)		
Post 2007	2477.2***			2557.6***		
	(23.43)			(47.83)		
Controls	N	N	Y	N	N	Y
Sample	All	All	All	For-Profits	For-Profits	For-Profits
School Fixed Effects	N	Y	Y	N	Y	Y
Year Fixed Effects	N	Y	Y	N	Y	Y
N	66,252	66,252	66,252	26,598	26,598	26,598
R^2	.342	.681	.681	.305	.613	.613

Panel 2: Tuition

Dependent Variable: Average tuition (2015$)

	(1)	(2)	(3)	(4)	(5)	(6)
PE owned·Post 2007	1305.3***	1606.7***	1645.1***	816.1**	717.9*	733.1*
	(311.5)	(343.7)	(346.3)	(321.3)	(382.5)	(386.5)
PE owned	4665.7***			1754.7***		
	(292.7)			(297.6)		
Post 2007	3197.1***			5707.7***		
	(51.68)			(98.72)		
Controls	N	N	Y	N	N	Y
Sample	All	All	All	For-Profits	For-Profits	For-Profits
School Fixed Effects	N	Y	Y	N	Y	Y
Year Fixed Effects	N	Y	Y	N	Y	Y
N	61,501	61,501	61,501	12,534	12,534	12,534
R^2	.254	.831	.819	.195	.622	.620

Note: This table shows the difference-in-difference estimate of the effect of the 2007 loan limit increase on borrowing, in panel 1, and tuition, in panel 2. Standard errors are clustered at the systemID level. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 9: Private Equity Ownership and Education Inputs

Panel 1

Dependent variable:	Faculty per 100 students			Number of Faculty		
	OLS		NNM$^{\pm}$	OLS		NNM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)
PE buyout	-.45**	-.36*	-.9	-21***	-19***	-2.9
	(.19)	(.18)	(1.5)	(4.4)	(4.3)	(8.7)
Composition controls‡	N	Y	-	N	Y	-
School type controls†	Y	Y	-	Y	Y	-
School Fixed Effects	Y	Y	-	Y	Y	-
Year Fixed Effects	Y	Y	-	Y	Y	-
N	62,432	62,432	5,352	62,432	62,432	5,352
R^2	0.83	0.83	-	.95	.95	-

Panel 2

Dependent variable:	Instruction spending share			Instruction spending (mill 2015$)		
	OLS		NNM$^{\pm}$	OLS		NNM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)
PE buyout	-.03*	-.029*	-.02	-8***	-7.2***	-.21
	(.017)	(.016)	(.038)	(1.7)	(1.5)	(1.7)
Composition controls‡	N	Y	-	N	Y	-
School type controls†	Y	Y	-	Y	Y	-
School Fixed Effects	Y	Y	-	Y	Y	-
Year Fixed Effects	Y	Y	-	Y	Y	-
N	97,401	97,401	5,191	97,401	97,401	5,191
R^2	0.75	0.75	-	.94	.94	-

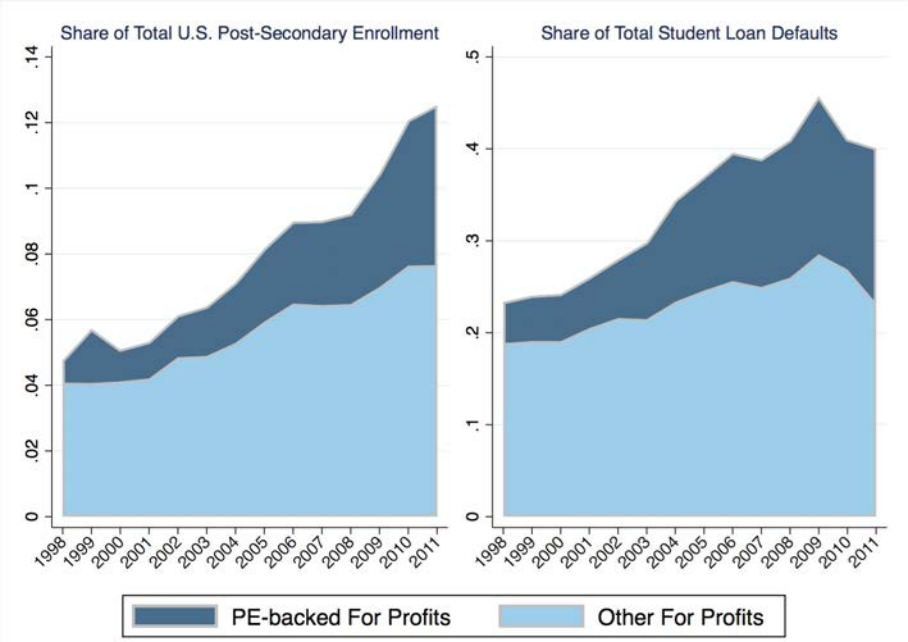
Note: This table shows regression estimates (OLS) of the effect of private equity ownership on measures of education inputs. Observations are at the school (UnitID)-year level. $^{\pm}$Nearest-neighbor matching is done within the sample of other for-profit schools. The dependent variable is measured the year after the treated school's buyout. Matching is exactly on the year before the treated school's buyout, and then on characteristics (see Section 3.3). ‡We control for the share of students who are white, black, and Hispanic, and the average amount of federal Pell grants per student, a proxy for low-income students. †These are indicators for having selective admissions, public ownership, and are fixed effects for highest degree offered. The latter includes less than 2-year (certificate), 2-year, or 4-year. Standard errors two-way clustered by SystemID and year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 10: Private Equity Ownership Compared to Chain Acquisitions

Dependent variable:	Graduation rate	Repayment rate	Log mean earnings	Average loan per borrower	Tuition per student (2015$)	Faculty per 100 students	Log number of FTE students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PE buyout	-.06***	-.022***	-.046**	564***	1632**	-.45**	.38***
	(.012)	(.006)	(.015)	(185)	(616)	(.19)	(.056)
Non-PE chain*	-.03	-.009	-.044***	472	254	-.002	.17***
	(.018)	(.006)	(.0089)	(318)	(502)	(.27)	(.05)
School type controls†	Y	Y	Y	Y	Y	Y	Y
School Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y
N	56965	31888	23322	77497	102353	62432	123051
R^2	.8	.97	.96	.67	.82	.83	.97

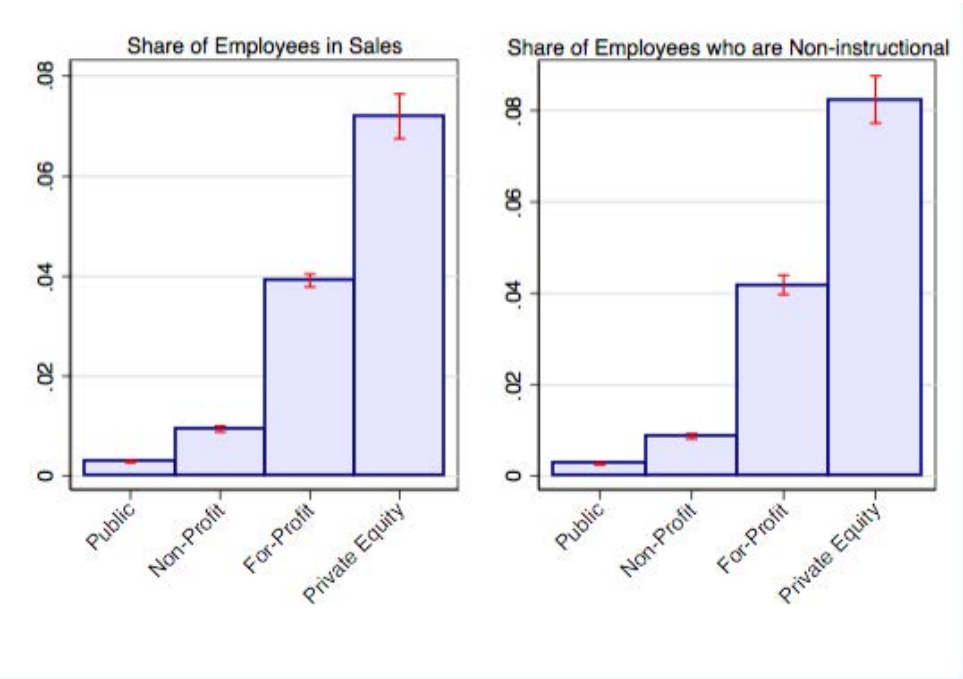
Note: This table shows regression estimates (OLS) of the effect of ownership type on primary outcomes. *This is an indicator for an independent school being purchased by a non-private equity owned chain. We define a "chain" as any parent company (SystemID) that is neither publicly traded nor private equity-owned and that owns at least two schools (UnitIDs). Observations are at the school (UnitID)-year level. Standard errors two-way clustered by SystemID and year. †Defined as in previous tables. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Figure 1: For Profit Schools Share of Loan Defaults and Enrollment



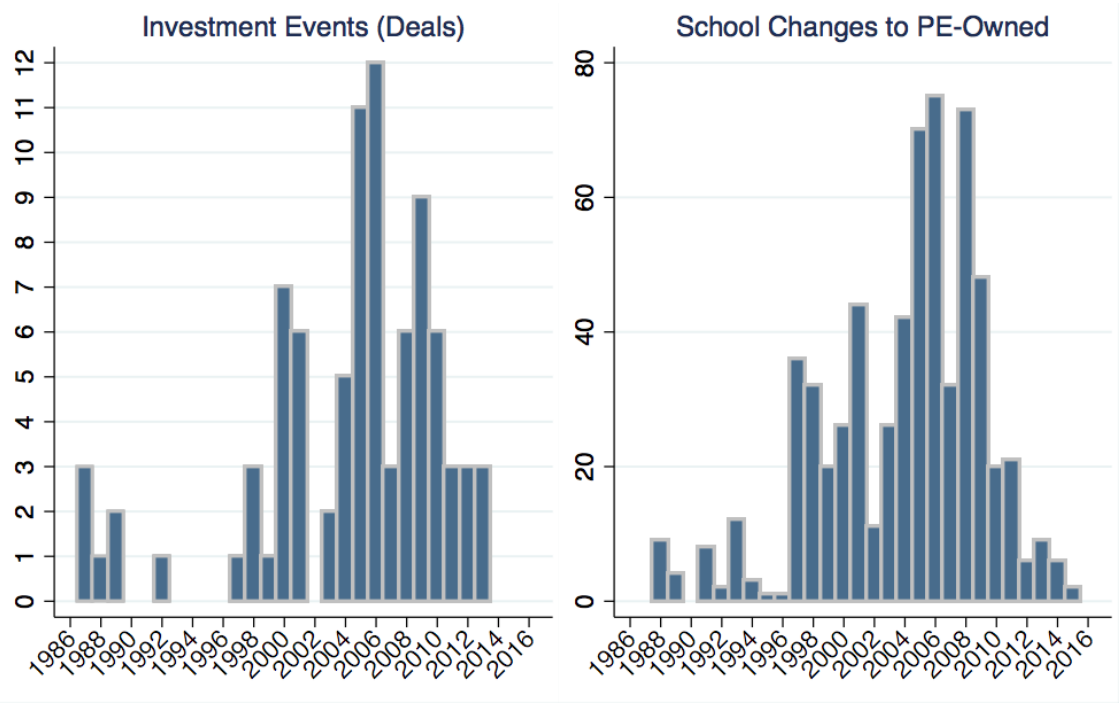
Note: The left graph shows the for-profit share of total US postsecondary enrollment by whether a school was ever private equity-owned. The right graph shows the share of total student loan defaults within two years of entering repayment, by whether a school was ever private equity-owned.

Figure 2: Employees in Sales and Non-Instructional Activities



Note: The figure above shows the share of employees who do sales and non non-instructional activities by institution type from 2012 to 2015 (data available only for these years).

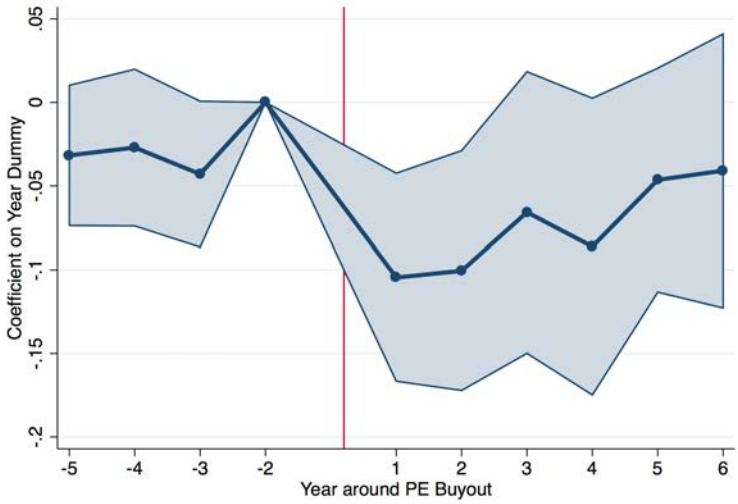
Figure 3: Private Equity Deals and School Ownership



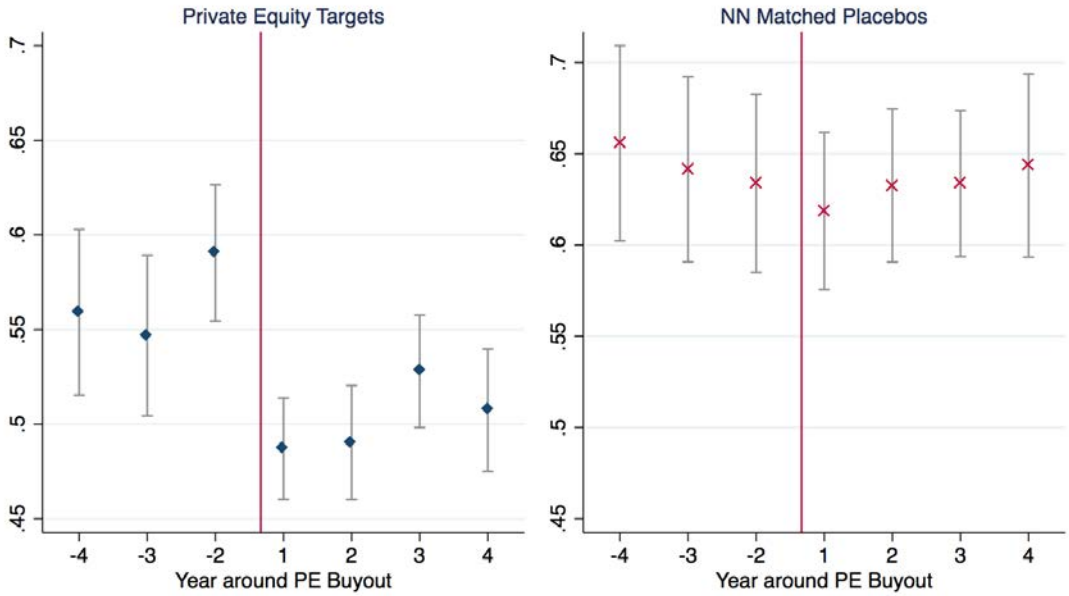
Note: This figure shows the ownership changes used in analysis. The left graph shows the 88 private equity buyouts of independent schools or chains of schools. The right graph shows school (UnitID)-level changes to private equity ownership.

Figure 4: Graduation Rate

Panel A: Graduation Rate Event Study (Regression Coefficients by Year)



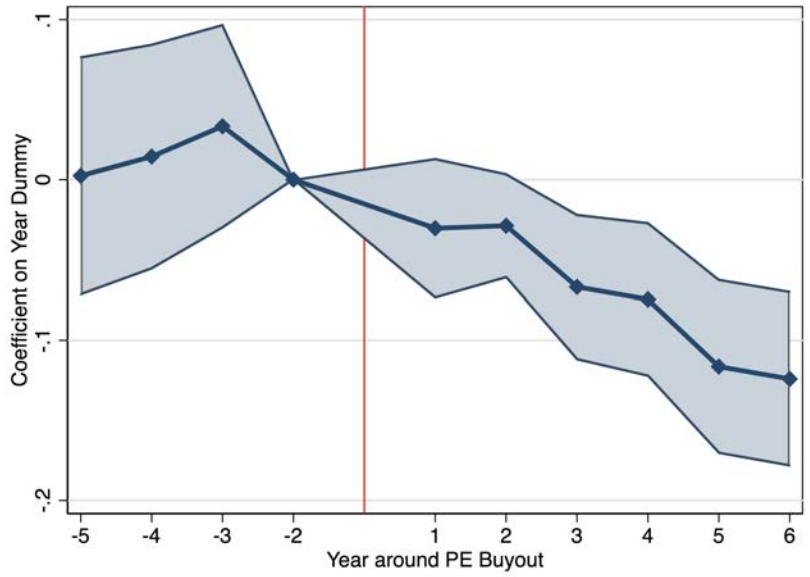
Panel B: Graduation Rate Around Buyout Year for Matched Treated and Control Schools



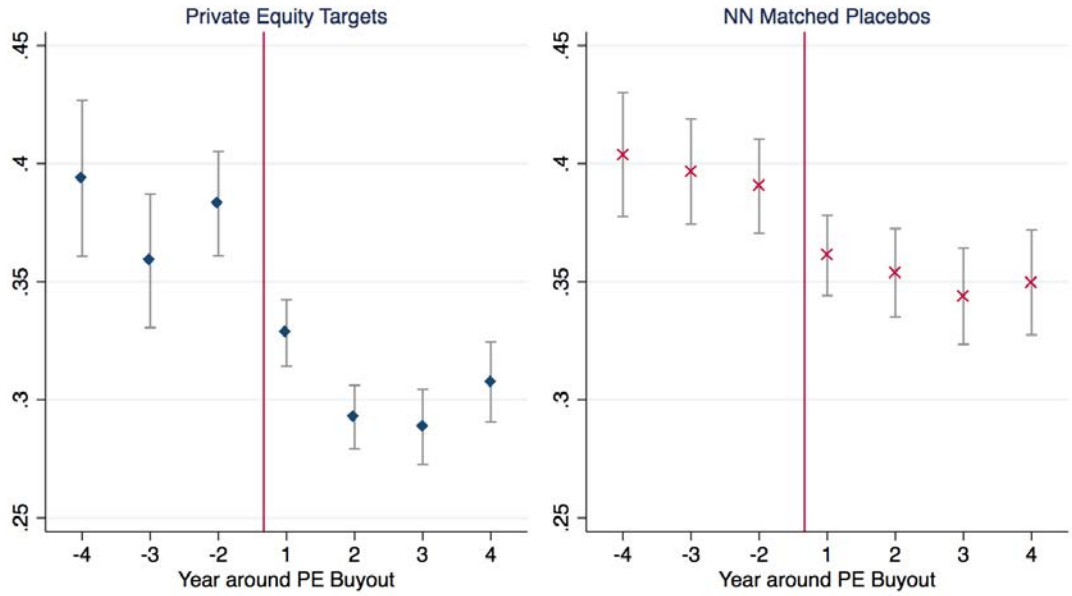
Note: The figure in Panel A shows the coefficient on a time dummy around the private equity buyout. The dependent variable, the graduation rate (share of students in an entering cohort who graduate within 150% of normal time), is on the y-axis. The second year before the buyout (-2) is the baseline, normalized to zero. Year -1 is omitted as this cohort is partially treated. The estimating equation is Equation 1. The area denotes a 95% confidence interval. The figures in Panel B show the mean graduation rate around the buyout year, using the subset of for-profit schools employed by the nearest neighbor matching estimator. The left-hand figure shows private equity targets (a subsample of all targets, as the matching estimator does not identify a match for every target). The right-hand figure shows matched control schools (placebos), for which the buyout year is chosen at random from the target distribution. Both are restricted to schools that existed in the year before the buyout. Year -1 is omitted as this cohort is partially treated. The level of observation is the school, or UnitID level. 95% confidence intervals shown.

Figure 5: Loan Repayment Rate

Panel A: Loan Repayment Rate Event Study (Regression Coefficients by Year)



Panel B: Loan Repayment Rate Around Buyout Year for Matched Treated and Control Schools

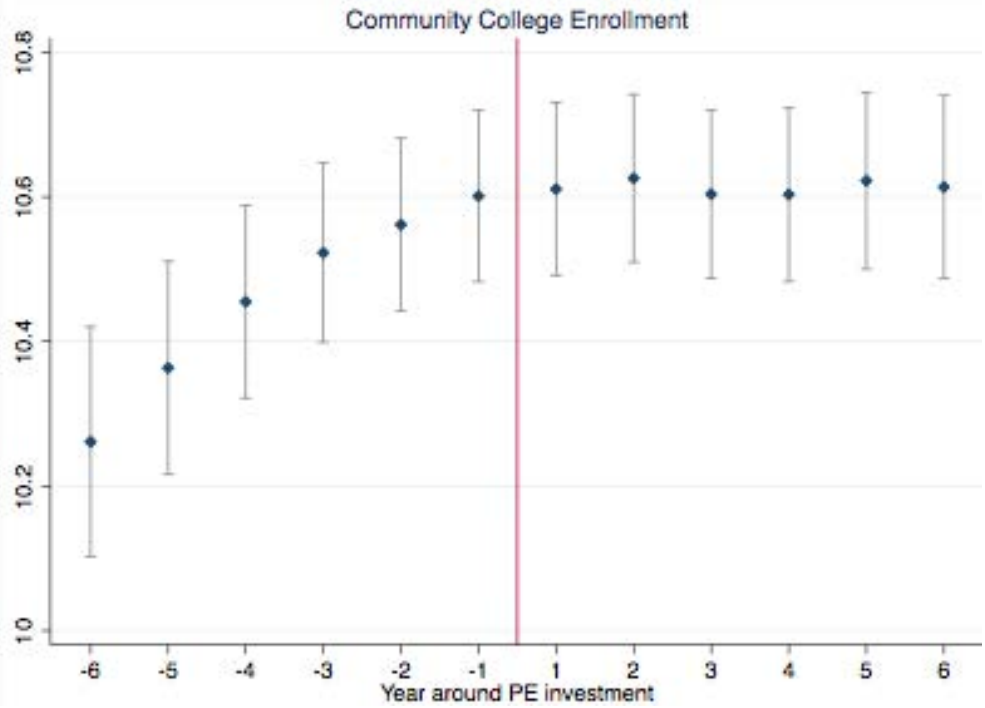


Note: The figure in Panel A shows the coefficient on a time dummy around the private equity buyout. The dependent variable, the loan repayment rate (share of students who have repaid at least $1 of principal within three years of entering repayment), is on the y-axis. The second year before the buyout (-2) is the baseline, normalized to zero. Year -1 is omitted as this cohort is partially treated. The estimating equation is Equation 1. The area denotes a 95% confidence interval. The figures in Panel B show the mean loan repayment rate around the buyout year, using the subset of for-profit schools employed by the nearest neighbor matching estimator. The left-hand figure shows private equity targets (a subsample of all targets, as the matching estimator does not identify a match for every target). The right-hand figure shows matched control schools (placebos), for which the buyout year is chosen at random from the target distribution. Both are restricted to schools that existed in the year before the buyout. Year -1 is omitted as this cohort is partially treated. The level of observation is the school, or UnitID level. 95% confidence intervals shown.

Figure 6: Earnings Event Study (Time Demeaned)

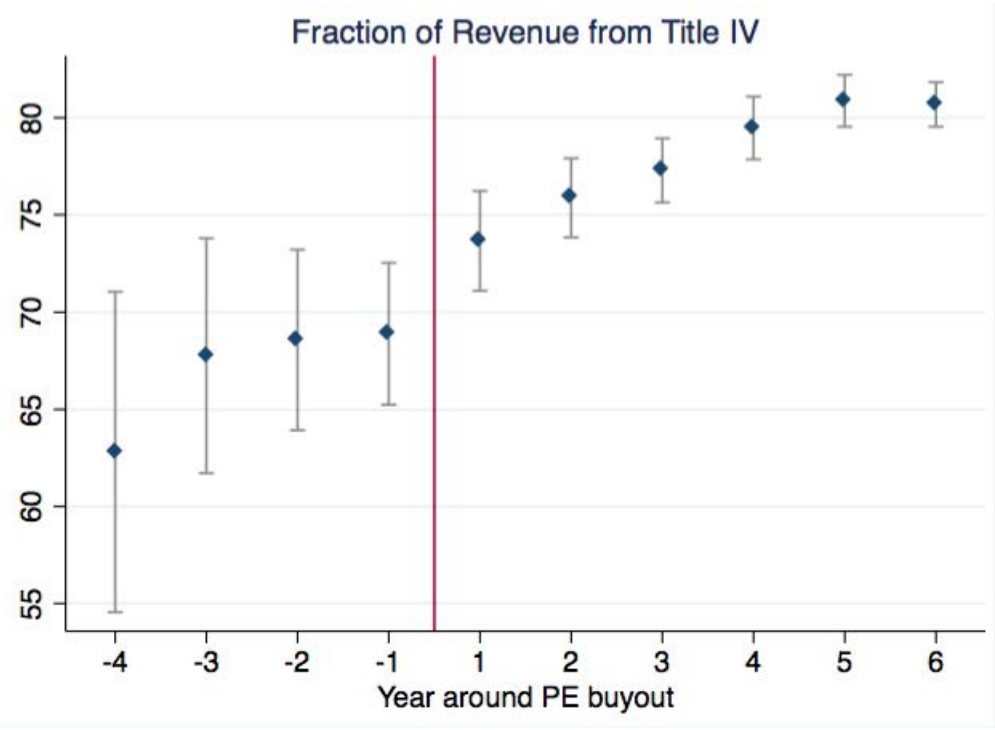
Note: The figure above shows the coefficient on a time dummy around the private equity buyout, where the dependent variable (y-axis) is log earnings. The year before the buyout (-1) is the baseline, normalized to zero. The estimating equation is Equation 3. The area denotes a 95% confidence interval. This data is at the school, or UnitID level (N=697). We restrict the observations to schools that existed in the year prior to the buyout.

Figure 7: Private Equity-Owned School Entry and Community College Enrollment



Note: This figure shows log enrollment in community colleges before and after the entry of a private equity backed for-profit college, within a commuting zone. Community colleges are defined as public institutions that grant two year or lower degrees.

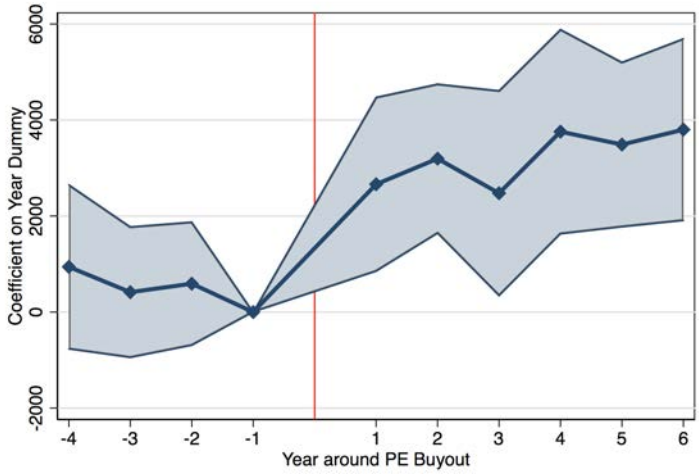
Figure 8: Distance from 90/10 Threshold



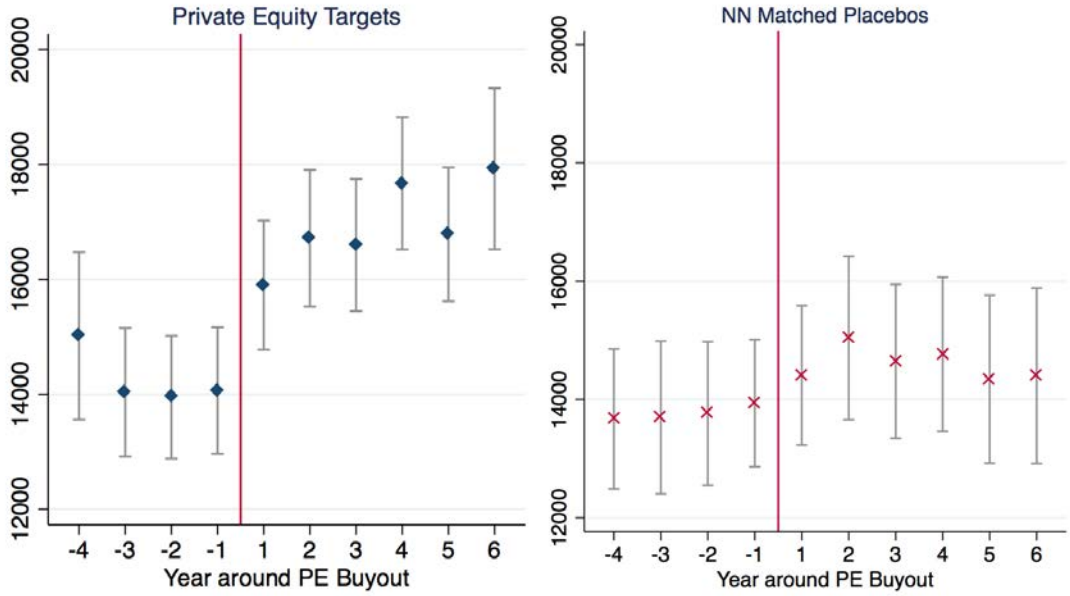
Note: The figure above show, within the sample of school systems bought by PE, the average fraction of school revenue from Title IV programs in the years around the ownership change. The level of observation is the SystemID. We restrict the observations to schools that existed in the year prior to the buyout. 95% confidence intervals shown. The data source is the Department of Education FSA Proprietary School 90/10 Revenue Percentages. Data are available from 2007 to 2016.

Figure 9: Per-student Tuition

Panel A: Per-student Tuition Event Study (Regression Coefficients by Year)



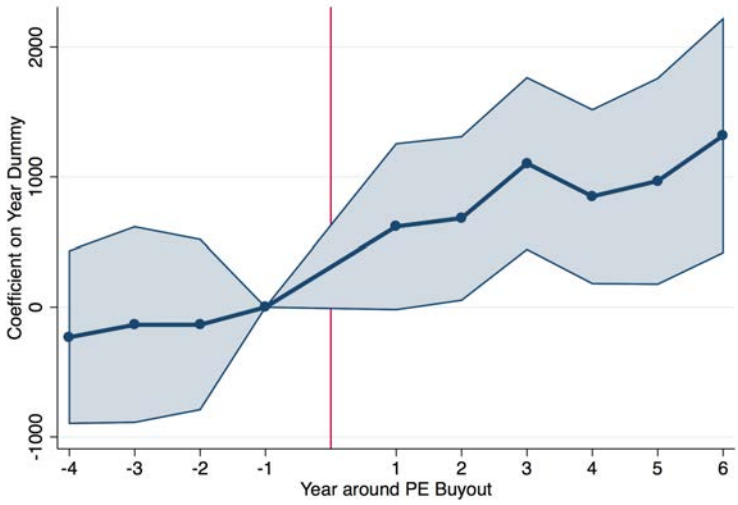
Panel B: Per-student Tuition Around Buyout Year for Matched Treated and Control Schools



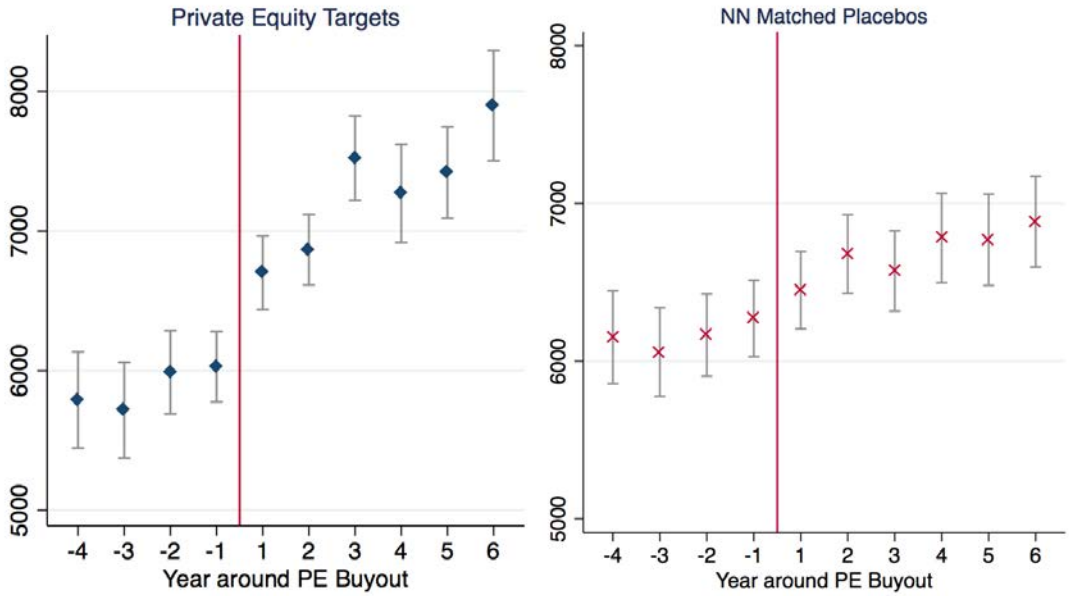
Note: The figure in Panel A shows the coefficient on a time dummy around the private equity buyout. The dependent variable, the per-student tuition (2015$), is on the y-axis. The year before the buyout (-1) is the baseline, normalized to zero. The estimating equation is Equation 1. The area denotes a 95% confidence interval. The figures in Panel B show the mean per-student tuition (2015$) around the buyout year, using the subset of for-profit schools employed by the nearest neighbor matching estimator. The left-hand figure shows private equity targets (a subsample of all targets, as the matching estimator does not identify a match for every target). The right-hand figure shows matched control schools (placebos), for which the buyout year is chosen at random from the target distribution. Both are restricted to schools that existed in the year before the buyout. The level of observation is the school, or UnitID level. 95% confidence intervals shown.

Figure 10: Per-Student Loans

Panel A: Per-Student Loans Event Study (Regression Coefficients by Year)

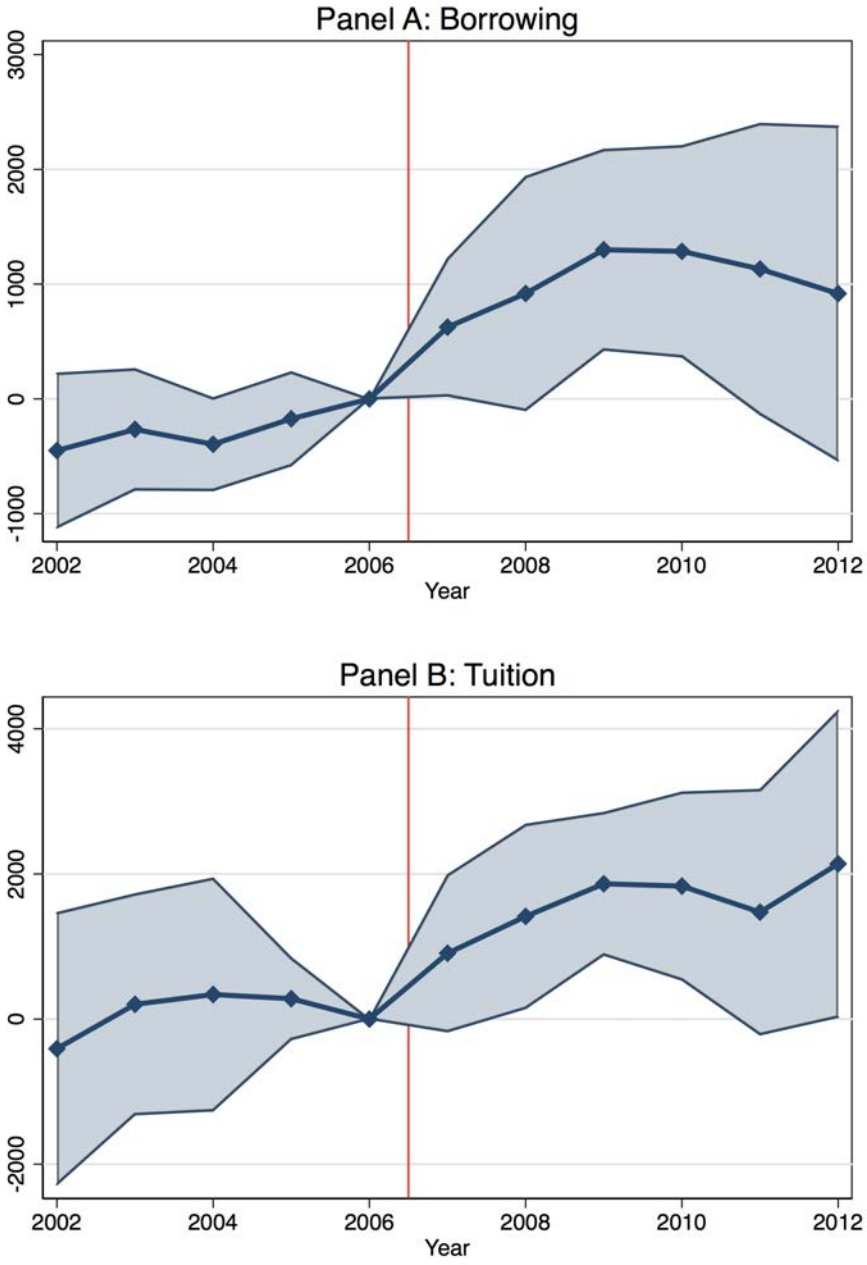


Panel B: Per-Student Loans Around Buyout Year for Matched Treated and Control Schools



Note: The figure in Panel A shows the coefficient on a time dummy around the private equity buyout. The dependent variable, the per-student loan (2015$), is on the y-axis. The year before the buyout (-1) is the baseline, normalized to zero. The estimating equation is Equation 1. The area denotes a 95% confidence interval. The figures in Panel B show the mean per-student loan (2015$) around the buyout year, using the subset of for-profit schools employed by the nearest neighbor matching estimator. The left-hand figure shows private equity targets (a subsample of all targets, as the matching estimator does not identify a match for every target). The right-hand figure shows matched control schools (placebos), for which the buyout year is chosen at random from the target distribution. Both are restricted to schools that existed in the year before the buyout. The level of observation is the school, or UnitID level. 95% confidence intervals shown.

Figure 11: Loan Limit Increase Diff-in-diff Coefficients over Time



Note: The figure above shows coefficients β_j from the following specification $L_{it} = \alpha_i + \alpha_t + \sum_{j=2001}^{2015} \beta_j PE_i * 1[Year = j] + \gamma X_{it} + \varepsilon_{it}$, where 2006 is the base year. The areas represent 95% confidence intervals. Results are enrollment weighted. The vertical line is positioned before 2007, when student borrowing limits were increased. Standard errors are clustered at the school system level.