When Investor Incentives and Consumer Interests Diverge:

Private Equity in Higher Education

Charlie Eaton, Sabrina T. Howell & Constantine Yannelis*

November 24, 2017

Abstract

In competitive industries, private equity's high-powered incentives have been found to increase firm productivity and benefit consumers. However, in sectors such as education, infrastructure, and health care, profit-maximizing incentives may be misaligned with consumer interests because of intensive government subsidy, opaque product quality, and long time frames for evaluation. This paper studies the effect of private equity buyouts in the for-profit postsecondary education sector. Employing novel, hand collected data on 88 private equity deals and 639 school-level ownership changes, we find that private equity buyouts lead to expanded enrollment and higher profits, but also to higher per-student debt, lower graduation rates, lower loan repayment rates, and lower wages. These results may reflect selection, but we find operational changes that point to a treatment effect. Supporting a treatment channel, we also show that following the expansion of federal credit limits for students, tuition and student debt at private equity owned schools rise faster than at other for-profit schools. Several analyses indicate that superior capture of federal aid is an important channel through which high-powered incentives translate to higher profits.

*UC Merced, NYU Stern and NYU Stern. Emails: ceaton2@ucmerced.edu, sabrina.howell@nyu.edu, constantine.yannelis@stern.nyu.edu.

Acknowledgements: We are grateful to Caroline Hoxby, Michael Dinerstein, Arpit Gupta, Holger Mueller, Thomas Philippon, Larry Schmidt, Antoinette Schoar, David Yermack and seminar participants at NYU Stern for helpful comments. We are also grateful to Suzanne Chang and Dalya Elmalt for superb research assistance. Sabrina Howell is grateful to the Kauffman foundation for generous financial support, and to the Stern Infrastructure Initiative for Preqin data.

1 Introduction

There is considerable evidence that private equity buyouts add value to target firms (Kaplan 1989, Lerner, Sorensen, and Strömberg 2011, Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda 2014, Bloom, Sadun, and Van Reenen 2015, and Bernstein and Sheen 2016).[1] One reason is that the incentives of private equity (PE) investors are shorter term and higher-powered than those of either closely held private firms or diffusely held public firms (Jensen 1989, Kaplan and Strömberg 2009). In sectors characterized by high competition, transparent product quality and immediate market feedback, the incentives of the private equity-owned firm are often well aligned with those of the consumer.[2]

However, there has been little examination of private equity investment in sectors with intensive government subsidy. Beyond a high share of revenue from public sources, these sectors, particularly healthcare, infrastructure, defense, and education, tend to be less competitive, have complex or opaque product quality, and have customer outcomes measurable only many years after payment. These features may make the high-powered and typically short-term incentives of private equity less well aligned with customer interests. As the share of private equity investment in these sectors is substantial (see Appendix Figure A.1), it is important to evaluate the role of private equity in highly subsidized industries.

This paper studies the impact of private equity acquisitions in the for-profit higher education industry, where firms derive over 90 percent of revenue from federal grants and federally guaranteed loans (Deming, Goldin, and Katz 2012, Kelchen 2017).[3] Schools receive federal grants and loans when the student enters school. So long eligibility and accreditation are maintained, revenue

[1] Also see Muscarella and Vetsuypens [1990], Kaplan and Schoar [2005], Gottschalg et al. [2007] and Cao and Lerner 2009. However, some literature has come to somewhat more nuanced conclusions; for example, Chevalier and Scharfstein [1996] find that LBO targets' high leverage makes them more liquidity constrained.

[2] A good example is the restaurant industry; Bernstein and Sheen [2016] show that private equity buyouts improve health inspection scores in chain restaurants.

[3] Institutions lose access to Title IV funds, which include federal student loans and grants, if more than 90 percent of their revenue comes from Title IV programs. This rule is binding or near binding for many for-profit institutions (Deming et al. 2012). Kelchen 2017 shows that many institutions are able to obtain more than 90 percent of their revenue from federal programs because veteran and military education benefits are not included under the Title IV threshold.

is disconnected from graduation rates and labor market performance. The cost of student defaults is borne by the taxpayer.

For-profit colleges are thus incentivized to target prospective students whose low incomes qualify them to pay tuition primarily with federal grants and loans. Poor student outcomes convert to lower future sales much less easily than other industries, because of the lag between enrollment and labor market outcomes, an absence of accessible information about earnings and outcomes, and the difficulty in measuring returns to education (Arcidiacono et al. 2016, Bettinger et al. 2012, Wiswall and Zafar 2014). In this way, government aid and loan guarantees create a potential misalignment of incentives between firm owners and customers.

We employ a novel, hand-collected dataset on 88 private equity deals and 639 campus-level ownership changes, which we match to Department of Education data. Our data covers 1990 to 2015, though some variables are available in more limited years. In our main analysis, we use four empirical approaches. The first regresses outcomes on private equity ownership, with school and year fixed effects, and standard errors doubly clustered by parent company and year. The next two approaches are nearest-neighbor matching (NNM) and coarse-exact matching (CEM). Specifically, we match each private equity owned school to a non private equity owned for-profit with similar characteristics and student composition in the year prior to the buyout event. We then assess outcomes two years after the buyout in the matched sample. The fourth approach is limit the sample to the "switcher" schools that are acquired in private equity buyouts, and for each outcome variable, plot its simple average across these schools in the years immediately before and after the buyout.

Broadly, we find that following private equity buyouts, profits increase, educational outcomes deteriorate, and students become more reliant on federal credit and grant programs. The effects are mostly visible in the year after the buyout and increase steadily afterward. We begin with financials. Private equity buyouts lead to a doubling of profits, consistent with the existing literature. Much higher revenue drives higher profits, which is in turn explained both by enrollment expansion of about one third, and higher tuition (though the effect on per student tuition is not robust

to the matching estimators). Costs also increase, which appears due to increased marketing and recruiting.

We find tentative evidence that education inputs, including the ratio of faculty to full-time equivalent students and the instruction share of expenditures, deteriorate, but these results are not robust to all our approaches. We also show that law enforcement actions are overwhelmingly concentrated in private equity-owned schools; private equity owned school-years comprise 4 percent of all school-years in our data, but 58 percent of first-time law enforcement actions.

Private equity ownership leads schools to target populations more likely to be eligible for federal aid. The ethnic composition changes after the buyout; an especially robust result is that the share of white students declines. Per student Pell grants, a proxy for the extent to which the student body is very low income, increases after private equity buyouts. Average annual loan amounts increase after private equity buyouts, by about $800 per borrower.

We find strong evidence that following a private equity buyout, graduation rates decline by 5-6 percentage points, relative to a mean among private equity owned schools of 38 percent. Likely related, we find that the repayment rate (students not in default and having repaid at least $1 in principal by three years after they leave school) falls by about 3 percentage points, relative to a mean among private equity owned schools of 35 percent. Finally, we find that private equity buyouts lead to 2.3-2.7 percent lower wages (measured six years after graduation), relative to a mean among private equity owned schools of $26,659, in 2015 dollars.

We explore five channels through which the high-powered incentives of private equity ownership may lead to the negative student outcomes that we observe (lower graduation rates, higher loan amounts, higher default rates, and lower earnings). The first is the selection effect, in which private equity firms are simply extremely good at screening. The second and third channels are closely tied to expansion of enrollment. One is that the composition of the student body may change after expansion, because of new funds, better management, or both. The other is an increase in misleading or obfuscating marketing and recruiting. The fourth channel is that cost reductions lower education quality. The final channel is a superior ability to capture government aid, which may

translate to higher per-student debt.

It is difficult to disentangle selection and treatment effects in private equity buyouts. Pre-trends are essentially absent in our visual event studies for most outcome variables, requiring a selection effect to be solely predictive. To more formally test for selection, we exploit the 2007 increase in student loan borrowing limits. We find that, relative to other institutions, private equity backed schools responded to the increase by raising tuition faster than other schools, which induced higher levels of borrowing. Thus a pure selection mechanism requires private equity firms to target schools that are not yet better managed, but will be in the future, without influencing management, which is implausible.

While enrollment expands after private equity buyouts, leading to a more socioeconomically disadvantaged student body, compositional changes do not explain the effects on student outcomes. First, the top end of the within-school wage distribution drives the effect on average wages. This is impossible if the average effect on wages is explained by additional students who are ex-ante lower quality than the average pre-buyout student. Second, for each outcome variable we include a specification that controls for the income and ethnic composition of the student body. We find that the negative effects persist or grow stronger in this model.

We find evidence for the third channel, that private equity owned schools employ more misleading or obfuscating marketing and recruiting techniques. First, the law enforcement actions are predominantly related to misrepresentation. Second, we find that many of the negative effects on outcomes are driven by largely driven by two to four year degree programs, rather than certificate programs. Outcomes are more easily observable to prospective students in the latter, because there is less time between payment and job placement, and because the product is usually more skill or job-specific. We have some evidence for the fourth channel, reduced education quality, but it is weaker and more tentative than the others.

Several tests support the fifth channel, that private equity owned schools are better at maximizing revenue from federal aid. First, the 2007 loan limit increase results is evidence of this. In a similar vein, Cellini and Goldin [2014] show that for-profit schools respond to federal student aid

increases by raising tuition. Second, we exploit the announcement of regulatory changes to show that publicly traded for-profit school share prices fall when rules are announced that could lead to limits on federally guaranteed loans and aid. The largest of these publicly traded firms were once private equity owned, and likely have higher-powered incentives than independent, privately held for-profit schools. Finally, we show that private equity owned schools are better at bunching below thresholds to facing sanctions, such as the loss of federally guaranteed loans and aid.

Together, these results indicate that superior capture of federal aid, a purely rent-seeking phenomenon, is an important channel through which higher powered incentives translate to higher profits. This is unambiguously not in the interest of students or taxpayers. It does not imply that private equity buyouts do not improve education quality. The sum of our results, however, show that private equity buyouts increase profitability through expanded enrollment, higher tuition, more predatory recruiting practices, and probably lower education quality. These operational changes lead to worse student graduation rates, higher loan burdens, higher defaults, and lower earnings.

Our main contribution is to show how private equity buyouts in a highly subsidized industry can yield outcomes at odds with consumer interests, contrary to previous literature about private equity. Relatedly, Ljungqvist, Persson, and Tag [2016] study the misalignment between private and social incentives in private equity delistings. Other related work includes Boucly, Sraer, and Thesmar [2011], Davis et al. [2014] and Bloom et al. [2015]. Boucly et al. [2011] find that French LBOs led to increased profitability, as well as higher employment, investment, and sales growth. Their focus is on testing whether LBO targets were credit constrained. Davis et al. [2014] examine only employment and TFP in manufacturing plants. Bloom et al. [2015] describe results from a survey of management practices. In the context of the private equity literature, an advantage of our data is that it contains both profits and operational data.

Our secondary contribution is to contribute to the literature on for-profit higher education. Especially relevant is the consensus that the returns to for-profit education are lower than the returns to community college, and may be negative (Deming, Goldin, and Katz 2012, Liu and Belfield 2014, Cellini and Chaudhary 2014, Cellini and Turner 2016). Cellini, Darolia, and Turner [2017]

show that community colleges substitute for for-profit colleges. In experiments, Darolia, Koedel, Martorell, Wilson, and Perez-Arce [2015] and Deming, Yuchtman, Abulafi, Goldin, and Katz [2016] assess employer perceptions of higher education institutions. Employers prefer applicants from non-selective public community colleges, and many for-profit degrees are no better than no degree at all. This paper sheds light on the role that private equity may have played in the larger deterioration in quality across the for-profit higher education industry. Table 10 shows that all major publicly traded for-profit higher education companies once had private equity investment.

The remainder of this paper is organized as follows. Section 2 provides institutional background. Section 3 discusses the data used in the paper. Section 4 presents the main analytical approach and examines the effect of private equity buyouts on school outcomes. Section 5 examines potential mechanisms for the observed effects. Section 6 concludes.

2 Institutional context

This section first briefly describes the history and defining characteristics of the for-profit higher education sector. Then, in Section 2.2, we provide evidence from existing literature that returns to for-profit education are likely worse – and definitely no better than – similarly selective public community colleges. We explain how the federal student loan and grant programs create misaligned incentives in Section 2.3. Finally, in Section 2.4, we discuss the role of private equity in for-profit higher education.

2.1 History and defining features of for-profit higher education

Proprietary, or for-profit, schools have existed in the U.S since the early 1900s. For much of the 20th century, they offered primarily technical and business skills, such as typing. They were also mostly independent (i.e. single-unit businesses) and privately held. In 1981, for profit enrollment was just 0.2 percent of total enrollment. Consolidation and increases in external equity financing began in the 1980s, with substantial private equity involvement. Substantial growth accompanied

these changes; between 1990 and 1995, for-profit enrollment was between 0.35 and 0.82 million, or 2-5 percent of total enrollment. The largest for-profits today are publicly traded, and all had private equity investment at one time (see Table 10). The sector has continued to grow. Between 2010 and 2016, annual total enrollment at for profit schools has been between 1.5 and 2.7 million students, or between 8 percent and 11 percent of total enrollment in all higher education.

While the sector is heterogenous, compared to their nonprofit and public counterparts, for-profits have smaller and leaner physical plants, have far more students in online learning programs, have few non-instructional services like athletics, typically have no research activities, hire most faculty on short-term contracts, and spend more on career counseling (Lang and Weinstein 2013).[4] In lieu of large humanities programs, for-profits focus on teaching specific, often vocational, skills designed to meet specific job descriptions, such as hair stylist or IT specialist.

To minimize costs, successful for-profits typically offer structured, focused programs of study with few electives. The material is standardized and replicated across a company's campuses and online programs. This approach has been quite successful; chains and online institutions were responsible for almost 90 percent of the growth of the for-profit sector in the 2000s (Deming, Goldin, and Katz 2012).

Resources are focused on sales and marketing. A U.S. Senate staff report found that in 2010, 30 representative for-profit schools employed about one recruiter for every 53 students, ten times the number of career services staff and 2.5 times the number of support services staff (Senate 2012). Recruiters' compensation was closely tied to new enrollments. The report found that public for-profits spend 23 percent of their revenue on marketing and recruiting, and cited evidence of large-scale student deception about completion rates, placement rates, and other statistics.

The student body at for-profit schools is quite different than that at other schools, even the closest comparison, public community colleges. Deming, Goldin, and Katz 2012 compare for-profit schools to community colleges, public, and nonprofit institutions. They note that on average,

[4]While the sector is dominated by a few large chains, such as the University of Phoenix, there are many small schools providing niche vocational certificates for jobs such as dog grooming (Deming et al., 2012). Just over half of the degrees awarded by for-profits are certificates, but for-profits offer undergraduate, doctorate, and many other degrees.

students at for-profits come from lower-income families and are more likely to be single parents than students in community colleges (two-year public schools). Other evidence that students at for-profit schools are disproportionately less well-prepared, and more likely to be ethnic minorities, is in Chung [2012]. Similarly, Looney and Yannelis [2015] show that for-profit borrowers tend to be poorer, older, and have worse labor post-school market outcomes.[5]

The 2000s saw a dramatic increase in student loan volumes and defaults. After 2008, high rates of student defaults and the new political environment led to increased scrutiny and regulatory oversight of for-profit colleges. Enrollment growth slowed, and the large chains saw substantial declines in new student volumes. The Obama Administration sought to implement tighter controls over eligibility for federal student aid, and together with local law enforcement, began to aggressively pursue for-profit higher education companies for various types of fraud.

2.2 Returns to For-Profit Education

With perfect information and competition, labor market returns to education should not vary with institution type. However, both information and market frictions exist. Importantly, students may not be well-informed about which programs are optimal for them, leaving aside deceptive recruiting practices (Lang and Weinstein 2013). Programs are difficult to compare to each other, and prospective students rarely have visibility into previous cohorts' outcomes. Also, students targeted by for-profits are among the most stressed and disadvantaged portions of the population, making them more prone to manipulative advertising than other groups; 29 percent are single parents, compared to 12 percent at community colleges, and their family income is about half that of students at community colleges (Deming, Goldin, and Katz 2012, Schilbach, Schofield, and Mullainathan

[5]For example, among dependent borrowers, median family income of students at for-profit schools was $30,000, compared to $48,000 at 2-year and nonselective 4-year schools. In 2011, only 37 percent of borrowers at for profit schools were dependents, compared to 50 percent(70 percent) at 2-year (nonselective 4-year) institutions. Deming et al. [2012] find that for-profits leave students with higher unemployment, lower earnings, and higher loan default rates than comparable students who graduated from other types of schools. Looney and Yannelis [2015] find that for the cohort of students that left school in 2011, over 20 percent were unemployed two years later, and median earnings were about $20,000. The former is higher, and the latter is lower, than for any other institution type, and furthermore increased (decreased) more relative to 2000 than for any other institution type. The five-year cohort default rate in 2011 was 47 percent, compared to 38 percent (27 percent) at 2-year (nonselective 4-year) institutions.

2016). Despite these differences, Cellini, Darolia, and Turner [2017] show that community colleges, which have open enrollment (i.e. are not selective or capacity constrained), are substitutes to for-profit schools. These public institutions devote far fewer resources to advertising (see Figure 1), and thus do not compete in a meaningful way for students.

There is accumulating causal evidence that relative to their substitutes – public community colleges – the returns to for-profit education are zero or negative. Deming et al. [2016] assess employer perceptions of higher education institutions using an experiment in which they sent resumes with different types of degrees to job openings. They found that applicants with business BAs from large online for-profit schools were roughly 22 percent less likely to be contacted than the same applicants with similar degrees from nonselective public schools. Within health jobs, this discrepancy was 57 percent. Having a for-profit associates degree made a person no more likely to be contacted than the same resume with no postsecondary degree at all. Deming et al. [2016] conclude that "employers appear to view for-profit postsecondary credentials as a negative signal of applicant quality, particularly when objective measures of quality such as a licensing exam are unavailable."

In a similar experiment in which resumes were randomly sent to employers, Darolia et al. [2015] found that employers did not prefer applicants with a for-profit degree to those with no college at all. Further, they found that employers seemed to slightly prefer (albeit not significantly) applicants with public community college degrees over those with comparable for-profit degrees.

Using administrative data from the National Student Clearinghouse between 2000 and 2012, Liu and Belfield [2014] find large wage penalties when community college students transfer to a for-profit college rather than a nonprofit college. They use transcript and other data to control for selection into for-profits. Cellini and Chaudhary [2014] use data from the NLSY97 to show that for-profit graduates are not more likely to be employed than comparable people with only high school degrees; though they find a positive effect on earnings (of about 4 percent per year), this is contingent on program completion, which many enrollees do not accomplish. The upper bound on their findings are substantially lower than the returns that other studies have calculated to public

community colleges (e.g. Jacobson et al. 2005, Jepsen et al. 2014).[6]

Cellini and Turner [2016] address the selection problem by examining within-student wages before and after attending college, using administrative data on about 1.4 million students. They find that despite much higher tuition, for-profit students experience smaller earnings increases than students at comparable public community colleges. The vast majority of for-profit students experience both lower earnings and higher debt after college than they did before college.

Finally, Armona et al. [2017] assess the effect of attending a for-profit college relative to a local public college or university using an instrumental variables strategy. They combine local labor market shocks with local school supply to instrument for enrollment in a for-profit relative to a community college. They find that students at for profits are less likely to be employed, have lower earnings, and have higher debt and higher default rates than students at public counterparts.

2.3 Federal Student Loans and Grants

For-profit higher education companies depend heavily on federal student loans and grants; the largest chains get over 80 percent of their revenue from federal sources. This fraction would be even higher if it were not for the 90-10 rules, and a statutory limit that 90 percent of revenue can come from Title IV loan and grant programs, which exclude veteran and military benefits. When these sources are included, many for-profits exceed the 90 percent threshold (Kelchen 2017). Maintaining Title IV eligibility is crucial for most higher education institutions, and it requires maintaining accreditation with one of a number of private accrediting agencies, and meeting certain standards, notably limits on the share of students that default over a three-year period.

Federal student loan programs were established in the 1960s and 1970s, and were targeted to upper middle class students attending higher tuition private colleges (Shireman, 2017). Government budget rules made it difficult for the federal government to lend directly to students without

[6] In contrast to the above studies, Lang and Weinstein [2013] find no difference in returns to certificate programs across for-profits and non-profits. They compare labor market outcomes for completers and non-completers across institution types, arguing that if this difference is similar, lower earnings for for-profit graduates are likely explained by the more disadvantaged student body at for-profit schools.

having to report the loans as adding to the deficit. Congress therefore subsidized the provision of federal loans by private lenders by legislating that the U.S. Department of Education would provide guarantees to private lenders (Berman and Stivers, 2016). That is, the federal government would cover bank losses when students fail to repay loans. The federal government also created the Student Loan Marketing Association, commonly known as Sallie Mae, in 1973. Sallie Mae raised capital to buy and offer student loans by securitizing loans and selling those securities to investors.

The banking industry aggressively lobbied for the expansion of the guaranteed student loan program during the 1980s (Wilson 1987, Berman and Stivers 2016, Shireman, 2017). This was accomplished in 1991, when unsubsidized Stafford loans were introduced. These were unsubsidized because the federal government would not pay interest accrued while the borrower was in school, but would guarantee against non-repayment. The limit for total borrowing with both subsidized and unsubsidized Stafford loans doubled from about $30,000 to over $70,000 (in 2015 dollars).[7]

The Federal Credit Reform Act of 1990 had changed federal accounting rule and made it possible for the federal government to directly lend to students at a much lower cost (Berman and Stivers 2016). However, the government opted to maintain the more costly guarantee subsidies from the federal government to private lenders, so that Sallie Mae and commercial banks would receive support to provide most of the expanded federal student loan programs.

Private lenders and for-profit colleges receiving the loans now had direct incentives to promote the expansion of student borrowing. This was achieved though regular increases in borrowing caps, higher interest rates, and restrictions on borrowers' ability to discharge debt in bankruptcy. The result was a large increase in federally guaranteed student debt disbursements from about $20 billion per year during the 1980s to $120 billion at the peak in 2011. Per student annual borrowing flows increased more than three-fold from a little less than $2,000 per student in the 1980s to over $7,000 in 2011.[8]

Looney and Yannelis [2015] find evidence that the massive increases in student loan defaults

[7] See Financial Aid for more information.
[8] Per full time enrolled student. Available at the College Board.

between 2000 and 2011 was concentrated in for-profit schools, and arose in part because of their growth. Federal loans to undergraduate borrowers at for-profit schools increased from $3.6 billion in 2000 to $18 billion in 2011. Borrowers entering repayment at for-profit schools increased from just over 200,000 individuals in 2000 to about 900,000 in 2011.

Today, Title IV programs consist of Stafford loans, Perkins loans, PLUS loans for parents, Pell Grants, and work study programs. The amount of federal aid a student may receive depends on family-specific factors as well as the cost of attendance, of which the most important element is tuition. Cellini and Goldin [2014] point out that this creates an incentive for for-profit schools to increase tuition above cost. They evaluate whether for-profits increase tuition in response to increases in federal loan subsidies, and find some evidence for federal aid capture. Using administrative data from California between 1989 and 2003, Cellini [2010] finds that increases in federal and state grants and loans is strongly correlated with for-profit school entry, particularly in high poverty counties.[9]

Pell Grants are need-based awards that depend on a student's family income, the cost of school attendance, and the length and type of program.[10] The average Pell grant is about $3,724 per year, and the maximum is $5,775.[11] In 2008-09, for-profits enrolled 12 percent of students but accounted for 24 percent of Pell grant disbursement, and 26 percent of federal student loan disbursements (Deming et al. 2012).

2.4 Private Equity in Higher Education

A private equity buyout usually affects the target firm's finances, its operations, or both. The key financial innovation of the typical leveraged buyout is to pay for much of the acquisition with debt issued by the target firm. That is, the acquired company is the borrower, and the borrowed funds

[9] In the aftermath of the 2008 financial crisis, Sallie Mae and the major consumer banks found themselves unable to raise adequate capital from securities markets to fund federal student loans. The Obama administration responded by eliminating the provision of federal student loans through private lenders. Instead, the Department of Education would provide loans directly to students. It used savings from this change to fund a significant expansion of Pell Grants (Shireman, 2017).

[10] The Department of Education has more information on the Pell grant program.

[11] See theCollege Board for more information.

pay for its acquisition. Beyond changing in the target's capital structure, usually dramatically increasing its leverage (which theory has suggested can help discipline managers (Bloom et al. [2015]), private equity firms also impose transaction and monitoring fees on the target. Metrick and Yasuda [2010] find that that these fees can represent as much as 90 percent of compensation to the private equity firm, suggesting that they could be material costs to the target firm. They are, however, difficult to observe (Metrick and Yasuda 2011).

In operations, Bloom et al. [2015] directly measure management practices and find that private equity owned firms have better management, equaled only by public firms and family firms run by external CEOs. In manufacturing, Davis et al. [2014] find that private equity owned firms expand productive plants and shutter underperforming ones. Bernstein and Sheen [2016] also find evidence of better operations in private equity owned restaurants, in part through better worker training and incentive alignment.

The left graph in Figure 2 shows the number of private equity deals in the for-profit education sector over time, while middle and right graphs show new and cumulative private equity ownership at the school (UnitID) level. Private equity investments in higher education have generally taken one of two forms. One is the purchase of independent (small, private) colleges, usually with consolidation intent. The second is the large buyout of an existing chain institution; the biggest have taken public companies private. For example, in 2007 KKR and SAC Capital took Laureate Education private for $3.8 billion.[12]

An example of the first type of investment, and which illustrates the broader pattern we find in the data, 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

[12]This is discussed in more detail in Section 5; other examples include Goldman Sachs taking Education Management Corp (EDMC) private in 2006 for $3.4 billion, and various investors, including Vistria Group, taking Apollo Education Group (University of Phoenix) private in 2017 for $1.1 billion.

their high school status.[13]

Private equity has played a role in a large fraction of for-profit higher education by enrollment. Figure 3 shows the share of private equity enrollment and defaults over time. Since the late 1990s, private equity schools have contributed to a large portion of the growth in enrollment. Private equity owned schools have also contributed significantly to the increase in defaults. In the late 2000s, despite being only approximately 10 percent of enrollments, for-profits schools accounting approximately 40 percent of student loan defaults. As is shown in the right panel of Figure 3, most of this increase is attributable to the growth in the default share at private equity backed for-profits. The share of defaults has remained relatively flat at non private equity backed for profit schools.

Education-related deals comprise between 2 and 3 percent of total private equity deal volume and number (Appendix Figure A.1). However, other sectors with similar issues of incentive alignment are remarkably large shares of the industry. Appendix Figure A.1 shows that healthcare, infrastructure, and defense have at different times comprised significant shares of total private equity deals. For example, since 2010, health-related deals have comprised about 40 percent of total private equity deal value and volume, and infrastructure has comprised about 14 percent of deal value, and 23 percent of deal volume. These sectors also feature intensive government subsidy, opaque outcomes that are distant in time from payment for service, and diffuse customers who may not have the ability to "vote with their feet".

Private equity ownership may increase profitability through operational changes, or may yield returns to investors through financial engineering. We do not observe debt, and are in any event interested in student outcomes, so we focus on operations. Profit growth in higher education, as in many industries, comes from increasing scale (enrolling more students) and increasing margins (the gap between costs and revenues). This differs markedly from most nonprofit higher education institutions, which are primarily concerned with increasing prestige and attracting those students most likely to succeed in labor markets (Hentschke 2010). It also differs from public institutions, which are typically capacity constrained by state and local funding limits (Hentschke 2010).

[13] See the Chronicle for further information.

We study four categories of outcomes. These will tell us whether, consistent with prior research, private equity buyouts lead to increased profitability, will shed light on how they do so, and finally will indicate whether these changes are in the interests of students. Specifically, we examine financial outcomes, operations outcomes, and student demographics as well outcomes. In terms of financial outcomes, we have detail information on school revenue, expenditures and profits. We also examine data on school operations including total and undergraduate enrollment, as well as faculty per hundred full-time equivalent students. Finally, we also examine student information on demographics and outcomes. We have detailed information on the ethnic composition of students as well as family financial background, as well as information on student outcomes including earnings, graduation rates, borrowing, and loan repayment.

3 Data and descriptive statistics

We assembled data from multiple sources on private equity deals in the for-profit college sector, for-profit college characteristics, student outcomes, and law enforcement actions against for-profits. We first describe our private equity deal data, and compare the firms in our sample to benchmarks in the overall industry (Section 3.1). Second, we detail available data on for-profit college characteristics and compare raw means across school types (Section 3.2). Finally, in section 3.3 we describe our law enforcement action data. Descriptions, sources, and years available for can be found for all data in the appendix.

3.1 Private equity data

We summarize the higher education private equity deal data that we have collected in Appendix Table A.2. These were derived by reviewing the parent ownership history of every for-profit college in the U.S. from 1987 through 2016 that was eligible to enroll students with federal Title IV student aid. Ownership histories for each college were determined by reviewing unpublished private equity investment portfolio documents gathered by the Senate Health, Education, Labor, and

Pension (HELP) Committee, 10-K statements for publicly traded firms, the ThomsonOne database of private equity investment, and online-course catalogs in which all Title IV colleges are required to disclose their ownership history.

We have identified 88 for-profit college companies involved private equity deals prior to 2016. The first known private equity deal in the sector occurred in 1986. Appendix Table A.2 shows that nearly 80 percent of the 88 deals are buyouts in which the private equity firm takes a majority and controlling stake in the company. The remaining deals involve minority investments by private equity. For simplicity, we use the term "buyout" in the remainder of the paper to refer to both "buyout or minority investment." Because for-profit college companies often operate multiple schools, we observe a total of 639 school-level changes to private equity. First, the 88 buyouts of for-profit college companies led to a change to private equity ownership for 384 schools. Second, for-profit college companies under private equity ownership acquired another 255 schools previously owned by other companies that were not under private equity ownership.

For buyout deals where we were able to ascertain the value, the average was $38.9 million (nominal). Thirty-four of the deals are categorized as growth or buyout deals, and 28 as leveraged buyouts. Among the 43 deals where we can identify an exit, or liquidity event, the average time to exit was 6.8 years. Of these, 22 were sales to other private equity firms, and 7 were IPOs. We identified 10 instances where the school failed (i.e., closed), and 27 that are still in the private equity firm's portfolio. Panel 3 shows the top private equity firms as well as the top private equity-backed acquirers.

The private equity firms in our data are roughly representative of the industry. Appendix Table A.2 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 per-

cent, 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.

3.2 School characteristics and student outcomes

Private equity data is matched to school characteristic and student outcome variables by parent company and year. Year corresponds to the spring term of the academic year (August 1 to July 30); for example, observations for the 2008-2009 academic year are identified as 2009. Most college characteristic and student outcome data is reported at the school level according to a unique "UnitID" from the Integrated Postsecondary Education Data System (IPEDS). UnitIDs remain constant over time, following schools through ownership changes. We report means and standard deviations in Table 1 for four ownership categories: 1) state and non-profit schools that offer 4-year degrees or higher, 2) community colleges that offer 2-year degrees and lower, 3) for-profit colleges of all degree levels that are not backed by private equity, and 4) private equity-owned for-profit colleges.

College financial data is available from 1987 through 2016. We analyze the data at a "SystemID" level. SystemId is unique identifier created by the authors for the parent system of postsecondary institutions including parent companies of for-profit college chains. We aggregate financial data to the SystemId level because financial data is often reported in IPEDS through parent UnitIDs for multiple associated UnitIDs (Jaquette and Parra 2014). Also, aggregating financial data at the SystemID level yields more consistent financial measures across years.

Our data include 570 firm-year observations of private equity owned for-profit school systems, and 80,585 firm-year observations of all other institutions. Table 1 Panel 1 shows that total annual profits at private equity-owned school systems are 20 times higher than at other for-profit (this is calculated as total line revenue minus total expenditure). Private equity-owned school systems also have much higher total revenue, total expenditures, revenue from Pell grants, and net tuition revenue. Per full-time equivalent student, private equity owned schools have higher tuition on average, at $12,988 compared to $12,044, $3,607, and $11,131 at non-private equity owned for-

profits, community colleges, and nonprofit/state schools, respectively.

Panel 2 of Table 1 provides summary statistics on college characteristics and student outcomes at the school, or UnitID level. Student loan default rates are obtained from the National Student Loan Data System (NSLDS). They are reported at an OPEID level that sometimes includes multiple UnitID level schools within a for-profit college company. Our data include 8,611 school-year observations for schools with ultimate private equity ownership, and 119,115 school-year observations for other types of schools.

Highest degrees offered is coded as 1 for 4-year degrees and higher, 2 for 2-year degrees, and 3 for less-than-2-year degrees and certificates. The mean highest degree level offered is 2 for private equity backed schools and closer to 3 for other for-profits. Private equity owned schools are larger, with mean enrollments of 778 students, compared to 298 students for schools not owned by private equity. Private equity owned schools also have a lower count of 3.3 full-time faculty per 100 full-time equivalent students compared to 4.33 faculty per 100 students at for-profit schools without private equity ownership. Faculty counts are higher still at community colleges and twice as high at non-profit and state schools.

The second section of Panel 2 reports means and standard deviations relating to the demographics of the undergraduate student body. Schools owned by private equity have the highest share of undergraduate students who are black, at 27 percent compared to 20 percent at other for-profits. Data on the share of students by income group was not recorded by IPEDS prior to 2009. We instead use data on colleges' federal Pell Grant revenue per FTE undergraduate student from 1987 to 2015 as the best available longitudinal indicator of low-income student enrollments. Private equity owned schools brought in $3,875 in Pell Grant revenue compared to $3,501 for other for-profits.

The final section of Panel 2 reports statistics for student outcome variables. IPEDS data on graduation rates is available for two groupings of degree programs. First, graduation rates within 150 percent of normal time is available for entering academic year cohorts from 1995 through 2013 for full-time first-year students in degree and certificate programs of no more than 2 years normal duration. At 60 percent, mean 2-year graduation rates for private equity owned colleges are 10

percentage points lower than those at other for-profit colleges.

Mean community college 2-year graduation rates are lower, at 28 percent. However, these are not comparable. The U.S. Department of Education 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). The 2-year degree graduation rate is useful for comparisons between types of for-profit colleges and within for-profit colleges over time. The mean 4-year graduation rate is similar across for-profits, but substantially higher at state and non-profit schools.

The most consistent data on student borrowing is IPEDS data for full-time, first-year students, which is available from 2000 through 2015. The mean borrowing per first-year fulltime student at private equity-owned schools is $3,247, more than 10 times higher than at state and non-profit schools and almost $1,000 higher than at other for-profit colleges. The average borrowing per borrower among fulltime first year students is $8,008 at private equity-owned schools, compared to $6,288 for other for-profits and $3,858 for state and non-profit schools.

NSLDS provides data on default rates 2 years after exit-year for exiting cohorts (graduates and drop outs) from 1992 through 2011 at the OPEID level. The U.S. Department of Education moved from a 2-year cohort default rate (CDR) to a 3-year CDR in 2012 because of concerns that 2-year CDRs were more subject to manipulation by for-profits (The Institute for College Access & Success 2012). Comparisons between for-profits and other types of colleges should therefore be made with caution. Nevertheless, the large 2-year CDR time series is useful for analyzing changes in default rates within schools after owner changes as well as potential bunching of default rates close to regulatory limits. The mean 2-year CDR for private equity owned schools is 11 percent compared to 12 percent for other for-profits.

It is known more broadly that CDR rates are vulnerable to manipulation, through the use of allowable non-repayment options like deferments and forbearances. A more reliable measure may be 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 3 years after leaving school by

either graduating or dropping out. Repayment rates are not only more reliable, but are also more sensitive than default rates, which measure only the worst-case scenario for repayment outcomes.

Finally, we use wage data from the NSLDS College Scorecard database. Wages are measured six years after cohort exit at the OPEID level for the 1998, 2000, 2002, 2004, 2006 and 2007 cohorts. Mean earnings six years after students leave school are $34,789 for private equity owned schools and $,210 at other for-profits. Mean earnings are higher for community colleges at $28,328 and for state and non-profits schools at $37,905.

3.3 Law enforcement actions

We also collected data on law enforcement actions against higher education institutions as a factor in college operations, primarily from Republic Report.[14] The actions range from 2007 to 2016, but the vast majority are post-2010, when the Obama Administration increased oversight over for-profit colleges. We were able to find 125 instances in which a state or federal agency initiated an investigation. These are described in Appendix Table A.3. The modal prosecutors are State Attorneys General, with 56 cases, followed by the Department of Justice (24) and the Department of Education (23). The FBI brought five cases.

The largest number of allegations related 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). There are 35 allegations of student loan fraud, 5 cases of fraudulent high school diplomas, and 11 cases of embezzlement and illegal use of funds.

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 58 such first-time actions; although private equity owned school-years comprise just 4 percent of all school-years in our data (including all institution types), they are 58

[14] https://www.republicreport.org/2014/law-enforcement-for-profit-colleges/ .

percent of the first-time actions.

4 The effect of private equity ownership

This section first explains the empirical approaches we use to assess how private equity ownership affects school and student outcomes: OLS, matching, and visual event studies within switcher schools. The results are in Section 4.2, organized by category of outcome.

4.1 Estimation approaches

4.1.1 OLS

We use within-school regressions and matching estimators to assess the effect of private equity buyouts on school and student outcomes. This analysis will be strongly suggestive of causality, but does not eliminate the possibility of a selection mechanism. We discuss this further, and test whether selection can fully explain our results, in Section 5.

Our first regression approach uses variants of the following specification:

$$Y_{it} = \beta_1 PE_{it} + \beta_2 Public_{it} + \beta_3 PE_{it} Public_{it} + \gamma \mathbf{X}_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \quad (1)$$

Here, i indexes schools and t indexes years. We include school fixed effects (α_i) and year fixed effects (α_t). PE_{it} takes a value of one if the school is private equity owned in year t. $Public_{it}$ takes a value of one if the school is publicly traded in year t. PE_{it} is one for schools that are publicly traded and formerly private equity owned, so that we can examine whether publicly traded firms that were private equity owned behave differently than publicly traded schools that never had private equity ownership, through the interaction $PE_{it}Public_{it}$. The sample includes all institutions in our data. Some variables exist earlier than others in the IPEDS data, so the sample size varies somewhat across outcome variables. Schools are observed at the UnitID level except for financial outcomes, which are at the SystemID level (see Section 3 for explanation of these identifiers). $\mathbf{X}_{it}$

is a vector of controls, which always includes fixed effects for the highest degree that the school offers and whether the school is selective.

For each outcome variable, we consider three versions of equation 1. The first omits $\beta_3 PE_{it} Public_{it}$. The second adds the interaction. The third repeats the first, but adds controls for the composition of the student body. Specifically, we control for the share of students who are black, the share of students who are white, the share of students who are Hispanic , and amount of Pell grants per full-time equivalent student, in 2015 dollars. The amount of Pell grants is a way to proxy for family income, as only very low-income students are eligible. The goal of this third regression is to assess whether a changing student body is a major explanation for any effect in the first regression. For example, if private equity owned schools expand enrollment to include more socioeconomically disadvantaged students, we might expect outcomes to deteriorate holding education quality fixed.

There may be concern that our results are driven by a few large private equity deals, and the schools subsequently acquired by those private equity targets. These are likely to have correlated outcomes. We address this in two ways. First, we double cluster by parent company (SystemID) and year in all specifications. Our results are in general not very sensitive to alternative cluster approaches. Second, we present robustness tests in which we omit the three largest school systems associated with a single private equity deal. These are Empire Beauty Schools, which ultimately consisted of 82 schools or UnitIDs, Corinthian (63 colleges), and EDMC (49 colleges).

4.1.2 Matching estimation approach

Our second approach consists of two matching estimators, nearest-neighbor matching (NNM) and coarse-exact matching (CEM). For each private equity-backed school, we identify the year prior to the buyout event, and match the subsequently private equity-backed schools in that year to other for-profits. We then assess outcomes two years after the buyout in the matched sample. Therefore, in both CEM and NNM, the samples are by construction matched exactly on year.

The NNM was conducted with the aim of matching on student composition. Our variables include the student body size, the percent on federal grants, the percent who are white, the highest

type of degree offered by the school, and whether the school is publicly traded. We adjust for large-sample bias that exists when matching on more than one continuous covariate using the Abadie and Imbens [2011] bias-corrected estimator. In Appendix Table A.4, we show that the covariate imbalance declines after the matching, though it does not entirely disappear, because the schools must first be matched exactly on year. If the effects are attenuated in the NNM, it will suggest that the main results are more likely primarily driven by selection effects. In practice, we find similar NNM results using a variety of covariate mixes.

The second matching estimator, CEM has some of the benefits of exact matching without the curse-of-dimensionality that produces few matches with continuous covariates. The algorithm coarsens continuous variables into bins, and then matches treated and control observations exactly in strata defined by these bins. Iacus, King, and Porro [2012] note that CEM has a number of attractive properties, such as the ability to adjust imbalance on one variable without affecting the maximum imbalance of others, and handing high dimensionality without a large sample. Again after matching on year, we match on the total number of students, the percent of students on federal grants, the share black, the share white, the highest degree type offered, whether the school is primarily online, whether the school is selective, and whether the school is publicly traded. Table A.5 shows that the imbalance across strata decreases dramatically after the CEM procedure. In both NNM and CEM, we do not match on the outcome variable. For example, when we examine the effect on share black, we omit the share black from the matching criteria.

4.1.3 Visual event studies

Our final approach is to simply graph the means of variables around the year of the buyout, within the sample of schools that change ownership from being for-profit, privately owned schools to being for-profit, private equity owned schools. There are two goals in this analysis. One is to test for pre-trends, which sheds light on whether a selection mechanism most likely explains our results. The second is to show the raw effect within switcher-schools, without any controls. We take a slightly different graphical approach to showing earnings around the buyout, because of the

strong time trends in wages.

4.2 Results

We organize our results by outcome category, beginning with financials, operations, and student body demographics, and then moving to measures of student outcomes, including graduation rates, loan default rates, and wages.

4.2.1 Financials

We begin by showing that private equity buyouts are associated with higher subsequent revenue and profits, consistent with the existing literature. Table 2 shows school financial outcomes, with all dependent variables in logged millions of dollars. The OLS regressions show that private equity ownership leads to a doubling of profits (Panel 1 columns 1-3), significant at the .01 level.

Public ownership is also associated with much higher profits. In column 2, where we include the interaction coefficient, we can see whether schools that were private equity owned and then are publicly traded explain the results. They do not. Note that the base case here is all schools that are neither private equity owned nor publicly traded. The total effect of a public, formerly private equity owned school (i.e. PE_{it} and $Public_{it}$ are both one) relative to this base case is the sum of the three coefficients ($\beta_1 + \beta_2 + \beta_3$). Here in column 2, the coefficient on PE_{it}, β_1, is the effect of a private equity buyout when that school is not subsequently publicly traded, relative to the base case. In columns 1 and 3, the coefficient on PE_{it}, β_1, is the average effect of public equity buyouts after controlling for whether the firm is publicly traded (that is, holding public ownership constant).

The matching estimation results are in columns 4-5, and find larger effects. For CEM, the number of matched schools and the covariate balance before and after matching for these results as well as all subsequent ones are reported in Appendix Table A.5. Appendix Table A.6 omits the largest three deals for the crucial outcome variables, and finds a slightly larger and equally robust effect on profits. Appendix Table A.7 Panel 3 shows that using state and year fixed effects rather

than school and year fixed effects also yields essentially the same effects of private equity buyouts on financial outcomes. In the remainder of Section 4, we do not directly refer to these two tables. Our findings are broadly robust to both.

We find that private equity ownership leads to much higher revenue, both overall (Panel 1 columns 6-10) and from tuition (Panel 2 columns 1-5). When we normalize net tuition by the number of students, we find statistically significant increases per student following the private equity buyout in all three of the OLS specification (not reported). However, the effect disappears in the matching estimates. Total expenditure also roughly doubles (Panel 2 columns 6-10). The increases in total revenue and total expenditure are extremely robust, with nine of the ten specifications significant at the .01 level.

To test for pre-trends, to show the raw effect within switcher-schools, and to understand how the magnitudes of profit, revenue, and cost increases relate to one another, we show visual event studies in Figure 4. The plots show that the increase in revenue is much higher than the increase in costs, leading to higher profitability. Net tuition, which is primarily paid for with federal loans, is the dominant source of increased revenues.

4.2.2 Operations

The increased revenues and costs after private equity buyouts stem from dramatic enrollment growth after the buyout and the requisite accompanying spending on marketing and recruiting. We find expansions in the number of students and a reduction of resources per capita. Table 3 shows regression estimates of the effect of private equity ownership on operations. In Panel 1, the dependent variables are the log total number of full-time equivalent enrolled students, and the logged number of first-time, full-time undergraduates. The OLS regressions show that both increase by about one third. The matching estimates are higher, at about two thirds. The effects are all highly statistically significant at the .01 level.

In Panel 2, we consider proxies for education inputs. First columns 1-5 estimate the effect of private equity on the number of faculty per 100 full time students. Buyouts are associated

with a decline in faculty to student ratios; in column 2, which includes the interaction with public ownership, a private equity buyout leads to 0.35 fewer faculty per 100 students, relative to a sample mean among private equity owned schools of 3.3 (see Table 1). This result is less robust than the others; it is not significant at all in column 1, or in either of the matching estimates. However, in a robustness test, we examine the sample of all for-profits and use state and year fixed effects instead of school fixed effects. The results are in Appendix Table A.7. This specification finds that faculty per hundred students declined by 0.5 after a buyout, significant at the .01 percent level.

The table also indicates that law enforcement actions (described in Section 3.3) increase following a private equity takeover. The dependent variable in Table 3 Panel 2 columns 6-10 is one if the school experienced its first action in a given year, and we have only 58 such instances (of which private equity-owned schools were responsible for 41). These are identified off very few observations in which the dependent variable is one, which may explain the lack of a result in the NNM estimate. The CEM matching algorithm found no incidence of the first actions in matched strata.

Visual event studies in Figure 5 confirm that following private equity buyouts, schools experience expansions in enrollment, though the effect lags the private equity buyout by a few years. The decrease in the ratio of faculty to students is also apparent.

For-profit schools tend to see a higher proportion of their employees involved in sales rather than instruction and research, and of those private equity owned schools tend to see the highest portion of employees involved in sales and recruiting. Figure 1 shows the share of sales and non-instructional staff, by school type and ownership between 2012 and 2015 (we cannot use this variable in regressions because of its limited availability). The top panel shows the share of full-time employees in sales. For-profit schools have a much higher portion of employees focusing on sales relative to non-profit public and private colleges. Private equity owned schools have twice the fraction of sales employees as other for-profits. We see a similar pattern with a percentage of non-instructional staff in the second panel, with private equity owned schools having roughly a fifth of their employees being involved in sales.

4.2.3 Demographics

Rather than compete directly with state and non-profit schools for better off students, we expect that private equity owned colleges will find it profitable to focus on enrolling underserved and disadvantaged students using federal student loans and grant aid (Cottom 2017). In Table 4, we provide evidence that private equity buyouts lead to higher shares of socioeconomically disadvantaged students. Here, we do not include the specification with student composition controls. We find in columns 1-4 that the share of students who are black rises by about three percentage points after private equity buyouts, significant at the .01 level. The NNM model does not yield an effect, but the CEM model yields a very robust 4 percentage point effect. The share of white students falls significantly in all four specifications, by between 3 and 7 percentage points (columns 5-8).

Federal Pell Grant revenue per FTE student is a proxy for low-income student enrollment. Only very low-income students are eligible at all, and the quantity of Pell grants a student receives is inversely related to her income and wealth (and also reflects tuition). While our descriptive statistics showed that Pell Grant revenue per student was highest under private equity, we find no consistent increase in Pell revenue after buyouts. The visual event studies, in Figure 6, are strongly consistent with the main results on ethnicity and show a stronger postive trend in Pell Grant revenue per student after buyouts.

4.2.4 Graduation Rates

We find strong, large, negative effects of private equity buyouts on graduation rates. In Table 5, the dependent variable is the share of students graduating in 150% of normal time. Our three OLS specifications find that private equity buyouts reduce graduation rates by 5-6 percentage points, all significant at the .01 level, relative to a mean among private equity owned schools of 38%. Public ownership is much less consistently associated with lower graduation rates. The NNM model finds a treatment effect of negative 5.3 percentage points, almost identical to the OLS result. The CEM model finds a somewhat larger negative effect of 7.4 percentage points. Both matching estimates are significant at the .01 level. The first plot in Figure 7 shows the event study within switchers.

This is less clear than the regression results, with some appearance of a pre-trend in the year prior to the buyout.

The decline in graduation rates is obviously an unambiguously negative outcome for students who fail to graduate, and may also harm their peers who do graduate. In their experiment sending resumes with different degrees to employers, Deming et al. [2016] found that employers' positive response to degrees were strongly related to differences in graduation rates, among other measures of school quality.

4.2.5 Borrowing

An especially important outcome is student borrowing. We show in Table 6 that the average loan per borrower increases dramatically following a private equity buyout. It increases by $796-$961 in the OLS specifications, and $639 in the CEM model, all significant at the .01 level. The NNM result, of $502, is significant only at the .1 level. The average loan per borrower at private equity owned for-profits is $8,008 (compared to $6,288 at other for-profits, and just $3,902 at community colleges). We also see a striking raw increase in borrowing in the visual analysis, in Figure 7.

Higher borrowing could reflect four different channels. One is that students are poorer and thus need to borrow more conditional on tuition. The second 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 in loans (Cottom 2017). This is because the government payments are guaranteed and immediate, while the school must follow up with the student for regular tuition payments, which may not be forthcoming if the student either has a constrained spell or determines that the education is not worthwhile. The third possibility is that the school's degree mix changes after the buyout, such that students enroll in higher cost degrees. Finally, the fourth possibility is that tuition increases, but the degree mix remains the same.

When we normalize net tuition by the number of students, we find statistically significant increases of about $1,000 per student following the private equity buyout in all three of the OLS specification (not reported). However, the effect disappears in the matching estimates. This is

approximately the same size as the loan amount increase, providing some support for the third and fourth channels, though this by no means excludes the first two.

We turn to default and repayment in Table 6 Panel 2. The OLS regressions (columns 1-3) yield no effect of private equity buyouts on CDRs. However, the effect is large and significant at the .05 level in both the matching estimators, which find 1.5 and 2.3 percentage point increases relative to a mean among private equity owned schools of 11 percent. The visual event study also suggests a positive effect, in Figure 7. As discussed in Section 3, CDRs are prone to manipulation. In the next section we will provide suggestive evidence that private equity backed schools are better able to manipulate two year cohort default rates, which may account for the imprecise estimates. Crossing cohort default rate thresholds can lead to the schools being sanctioned, and the loss of access to federal loans and grants.

Our preferred variable is the share of students in repayment, shown in columns 5-10. Relative to a mean among private equity owned schools of 35 percent, we find that private equity buyouts decrease the share of students in repayment by about 3 percentage points. Publicly traded schools are also strongly associated with lower repayment rates. The effect of private equity in the CEM model is higher, at 7 percentage points. Similarly, there seems to be a fall in wages, though this variable appears to have an upwards pre-trend.

Earnings

Our final outcome is student wages. We use a series of moments describing the distribution of log wages within a graduating cohort of a specific school. For example, we study how the 10th percentile log wage of a school's graduating cohort changes across cohorts observed before and after the private equity buyout. The results are shown in Table 7. Columns 1 and 3, which consider private equity and public ownership separately, find that both are associated with lower average wages. Private equity buyouts lead to 2.3-2.8 percent lower wages, relative to a mean among private equity owned firms of $34,789, in 2015 dollars. This is significant only at the .1 level. The specification with the $PE_{it}Public_{it}$ interaction finds a larger independent effect of private equity

(among schools that do not ultimately become publicly traded), of 4.3 percent, significant at the .05 level. Data limitations prevent us from using the matching estimators for the wage data.[15]

We plot how average earnings evolve around the buyout in Figure 8. As earnings exhibit strong time trends, both due to increasing wage earnings over the bulk of our sample period, and decreasing earnings for graduates following the Great Recession, we graph coefficients from a fixed effects regression. Figure 8 shows the coefficients β_j from the following specification:

$$lnE_{it} = \alpha_i + \alpha_t + \sum_{j=-4}^{6} \beta_j 1[Year = Year_{PE} + j] + \gamma X_{it} + \varepsilon_{it} \tag{2}$$

Here, $1[Year = j]$ is an indicator of a year pre or post the buyout year $Year_{PE}$, α_i and α_t are respectively school and year fixed effects, and X_{it} are demographic school controls. The outcome of interest is lnE_{it} log earnings six years after enrollment. We see no differential effects for schools prior to a private equity buyout. Afterwards, we see a deterioration in log earnings six years after enrollment.

In the remaining columns of Table 7, we examine the distribution of wages. Remarkably, the average effect is the result of a decline at the top of the distribution. We find smaller and mostly insignificant effects of private equity buyouts on the 10th, 25th, or 50th percentiles of wages. Instead, the positive effects are mostly coming from the 75th and 90th percentiles. This is a crucial result for understanding whether reduced student quality after private equity buyouts explains our results. As explained in detail in Section 5.2, if lower quality students (due to expansion) explained the negative impacts of buyouts on outcomes, we could not observe negative effects on wages *only* at the top of the distribution.

[15]This is because we only observe six cohorts (as described in Section 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.

Private Equity Firm Characteristics

We examined whether private equity firms with or without education specialties, or those that are especially high- or low-performing, drive our main results. For this exercise, we use the hand-collected data about private equity firms' returns and education-related investing experience, described in Section 3.2 above. Exhaustive analysis found no robust variation in the effects by these measures, though it should be noted that the sample size shrinks when we limit the exercise to firms matched to returns data.

5 Mechanisms

This section explores five channels through which the high-powered incentives of private equity ownership may lead to the negative student outcomes that we observe (primarily lower graduation rates, higher loan amounts, higher default rates, and lower earnings). The channels are not mutually exclusive, but we examine whether the evidence is most consistent with each channel. The first is the selection effect, in which private equity firms are simply extremely good at screening. The second and third channels are closely tied to expansion of enrollment. One is that the composition of the student body may have changed after the buyout. The other is an increase in misleading, or obfuscating, marketing and recruiting. The fourth channel is that cost reductions lower education quality. The final channel is a superior ability to capture government aid.

We provide new evidence, including causal identification approaches, to distinguish between these channels. We conclude that selection does not seem to be the primary driver, and that instead the evidence strongly favors a causal effect of private equity ownership. The data support the other four channels.

5.1 Selection: The effect of the 2007 Loan Limit Increase

Private equity firms may simply be expert screeners, and pick schools that would have undergone these changes anyway. Pre-trends are largely absent from our visual event studies, which is some

evidence against a pure selection mechanism. We do not rule out selection, but we require it to operate on a predictive margin; that is, private equity firms must be able to target schools on the cusp of better operational capability. We show this in a causal way using an unexpected regulatory change that created growth options for for-profit schools: In 2007-08, the government increased student loan borrowing limits. We examine whether schools already under private equity ownership were more responsive than their counterparts to this new opportunity. We use a difference in difference approach, and analyze the differential reaction of private equity held schools relative to non-private equity owned institutions. We find that private equity held schools raise tuition and borrowing at a faster rate following loan limit increases, consistent with these institutions being better at capturing government aid.

Approach

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.[16] The 2006 Higher Education Reauthorization Act (HERA) increased annual borrowing limits for subsidized loans, by amounts depending on class status, and the 2008 Ensuring Continued Access to Student Loans Act increased limits for unsubsidized loans by $2,000 for all students. In an analysis of the new limits, GAO [2014] was unable to discern an 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.

[16]Specifically, for the 2007-08 academic year, loan limits for first-and second-year undergraduate students as well as for graduate and professional students were increased by roughly $1,000, from $6,625 to $7,500 for independent students (over 24 years old, who comprise most for-profit students) in their first year of school, and from $7,500 to $8,500 for independent students in their second year. For the 2008-09 academic year, the limits further increased to $9,500 and $10,500, respectively. See Pub. L. No. 109-171, § 8005, 120 Stat. 4, 158 (2006). The amounts are smaller for subsidized Stafford loans, for which the federal government paid interest accrued while the student was in school (these loans were eliminated for graduate and vocational students in 2012). These loans are non-dischargable in bankruptcy, and 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.

Consistent with private equity-owned institutions being better able to capture federal subsidies, we find that response to the loan limit increase, private equity-owned schools increased average loan sizes more or faster than other for-profit schools, and that this effect is driven by tuition hikes. We compare private equity owned institutions to other for-profit schools.

$$L_{it} = \alpha_i + \alpha_t + \beta PE_i * Post2007 + \gamma X_{it} + \varepsilon_{it} \tag{3}$$

The term L_{it} denotes average borrowing or headline tuition in school i in year t. School fixed effects α_i absorb time invariant school specific factors, such as wealth in the area where a school is located and a school's alumni network. Time fixed effects α_t absorb national time varying factors that affect schools in the same fashion, such as the state of the national economy or changes in federal grant aid. We include school specific controls X_{it} for institution size. Standard errors are clustered at the institution system level to address potential serial correlation. The main coefficient of interest is β, which captures the increase in average borrowing at private equity-owned institutions relative to other institutions after the limit increase. If private equity-owned institutions are better at capturing aid, we would expect average loan amounts to rise at a faster rate relative to other institutions, and the coefficients β should be positive and significant. The year 2007 is excluded from the analysis, as the two reforms took place in 2007 and 2008 and thus it is somewhat ambiguous 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 trended similarly in terms of average student borrowing. Thus, any increase in borrowing at private equity-owned institutions following the limit increase is due to these institutions being better at capturing federally guaranteed loans. This assumption implies that, prior to the 2007 limit increase, we should see borrowing at private equity-owned and non-private equity owned institutions trending similarly. The top row of Figure 9 provides graphical evidence that this is indeed the case. The left hand panel compares private equity-owned schools to all for-profits. Prior to the 2007 limit increase, we see average borrowing levels track each other closely for private equity and non private equity

owned for-profits. Consistent with private equity-owned schools being better at capturing federal loans, we see slightly higher levels of borrowing at private equity-owned schools.

Following the increase in borrowing limits, we see the two series diverge further, with private equity-owned for-profits seeing a larger increase in average borrowing. A potential concern is that this effect could be driven by changes in the selection of schools around 2007, with private equity groups acquiring more for-profits with higher average borrowing after the limit increase. To address this concern, the right panel restricts the treatment group to institutions that were private equity owned prior to 2007. The results on the right hand panel are quite similar to the patterns in the left panel, suggesting that the observed effects are not driven by changes in the composition of private equity owned schools.

Results

Table 8 makes the graphical evidence presented in Figure 9 explicit. Table 8 presents estimates of equation 3. The results indicate that following the loan limit increases, average borrowing increased by an additional thousand dollars at private equity-owned institutions relative to other schools. The result is robust to different specifications. The first two columns presents a simple difference in difference, rather than including year and school fixed effects, we include a year trend and an indicator for whether a school is private equity-owned. Columns (1) through (3) include all schools, while columns (3) through (6) include only for-profit schools. The results are quite similar across the two different control groups are both significant at the .01 level. Consistent with tuition inflation and increases in borrowing, the year trend is positive and highly statistically significant. The coefficient on being private equity owned is also positive and highly significant in column (2), which is consistent with borrowers at private equity-owned schools borrowing more prior to the reform.

Columns (2) and (3) add in school and a year fixed effects, and controls respectively. The results remain similar, and differential effect private equity-ownership following the limit increase remains significant at the .05 level or higher. Columns (4) through (6) present identical specifications,

restricting the sample to schools that were owned by a private equity group prior to 2007. This avoids selection effects in which private equity groups may have targeted schools differently after the limit increase. The results are quite similar and statistically indistinguishable from the results in the other columns, confirming that private equity-owned schools saw faster growth in borrowing following the limit increase in 2007.

To further explore the timing of the effects, and to test the validity of the parallel trends assumption underlying the results, we run the following specification, interacting the private equity-ownership treatment with indicators for each year. This effectively allows us to observe the timing of the estimated effect in column (5) of Table 8.

$$L_{it} = \alpha_i + \alpha_t + \sum_{j=2001}^{2015} \beta_j PE_i * 1[Year = j] + \gamma X_{it} + \varepsilon_{it} \tag{4}$$

The treatment is restricted to schools that were acquired by a private equity group prior to 2007, and standard errors are clustered at the system level. The solid line shows point estimates of the coefficients β_j , while the dashed line shows a 95 percent confidence interval. The results are shown in left panel of Figure 10. Consistent with the raw averages seen in Figure 9, we do not observe any significant differences between the private equity-owned and other for-profit groups prior to 2007. The coefficients are very close to zero, and not statistically distinguishable from zero at conventional levels. Following the limit increase in 2007, we see borrowing increase relatively faster at private equity owned schools, and within three years from the reform this difference becomes significant at the .05 level. The timing of the effect is thus consistent with private equity owned for-profits capturing additional revenue through federal loans.

It is possible that this increase in borrowing is beneficial to students. Indeed, Goodman et al. [2017] find that many young borrowers are credit constrained, and use student loans as an additional source of liquidity. However, if schools are raising tuition to capture credit expansions, this is unlikely to benefit students. Figure 9 Panel B shows that this is the case. Private equity owned schools raise tuition at a faster rate relative to other institutions. Table 9 presents the results, replacing the outcome variable with tuition. We see sharp increases in tuition that completely offset

the increase in borrowing. Figure 10 Panel B also shows that there was no pre-trend; the timing of the limit increase coincides with the tuition hike.

The results are consistent with private equity owned schools responding to the credit expansion by hiking prices to capture government aid. Their superior ability to capture a new strategic opportunity is evidence that the private equity-owned firms are better managed than other for-profits. It seems farfetched to imagine that they were able to select not on managerial ability prior to the buyout, but on future improvements in management that are independent of private equity intervention. In this way, the analysis presents evidence that private equity ownership improves operations, and does so in a way that is rent-seeking rather than in the interests of students.

5.2 Composition

This section examines whether the negative effects of private equity buyouts on student performance may reflect the changing student body composition that accompanies enrollment expansion. The marginal student targeted by this expansion may be less well qualified, with poorer labor market potential. In this case, the negative results on student outcomes could be explained by student composition changes. That is, holding education quality constant, our worse outcomes may reflect additional students who are lower quality than the average student before the buyout. This hypothesis bears examination, as we found changes to student demographics after private equity buyouts in Section 4.2.3.

By construction, this argument means that the additional students are on the lower end of the expected wage spectrum. Without loss of generality, suppose they are all worse than the worst student before the buyout. Then, holding education quality constant, wages must decrease after the private equity buyout at least as much in the bottom percentiles of the distribution as the top. Note that this argument would not hold if, say the 25th percentile was zero before the buyout and remains zero after. However, at the 10th percentile, the lowest observed cohort wage across all our data is $1,093. In fact, we find that the 75th and 90th percentiles drive the negative average effect of private equity buyouts, with no effect at the median or below. This means that additional students

who are lower quality cannot explain the negative effect of private equity buyouts on wages, and thus are unlikely to fully explain the other negative effects.

A second, more direct test for the effect of composition is our regressions controlling for the socioeconomic makeup of the student body. In Section 4, the third specification for each outcome variable controlled for ethnicity and family income proxies. These regressions are problematic because they include outcome variables on the right hand side, but they typically yield as strong or stronger effects than the main specification. Thus demographics are not responsible for the main effects.

Therefore, it is unlikely that our results are fully explained by compositional changes. However, the substantial expansion and changes in student demographics that we observe raise the question of whether the marginal students would have been better off had they not attended a private equity-backed school. We cannot observe their counterfactual outcome. However, the literature discussed in Section 2.2, notably Liu and Belfield [2014], Darolia et al. [2015], Deming et al. [2016], Cellini and Turner [2016], and Armona et al. [2017], offers compelling evidence that students are better served by community colleges than for-profit colleges, with the exception of a few licensed vocational specialties, such as cosmetology. We cannot establish within our data whether the additional students would have been better off had they not attended the private equity-owned, expanded for-profit college. However, the existing literature, our findings suggestive of lower education inputs, and rent-seeking behavior in response to regulatory changes (discussed in Section 5.5), strongly suggests that the answer is no.

5.3 Predatory marketing

Many subsidized industries are also ones in which the quality of the product is opaque and difficult for the average consumer to evaluate. In stark contrast to, for example, restaurants, where quality is easily observable even to the least sophisticated customers, it is difficult to ascertain one's expected returns to education. For-profits specialize in marketing, and thus a third explanation for our results is that the operational improvements consist primarily of predatory recruiting and marketing

practices. The law enforcement actions, which primarily consist of misrepresentation, suggest that this is certainly happening.

We expect that obfuscation should be more difficult when there is less time between payment and consumption (job placement), and when the product is specific to a technical or business sector. For example, Cellini and Turner [2016] find that cosmetology certificates are the only field in which for-profit students experience better returns than students at community colleges. Since certificates are quicker and have clearer job outcomes, we expect that obfuscation will lead the negative effects of private equity buyouts to lie primarily in the two to four year degree programs, rather than in certificate programs. In Appendix Table A.8 we present two regressions for a number of key outcome variables. The first limits the sample to for-profits for which the highest degree offered is a certificate, and the second limits the sample to for-profits that offer two to four year degrees. This has the added benefit of demonstrating that our results are robust to limiting the total sample to for-profits.

We generally find that the effects of private equity buyouts are larger among schools offering two to four year degrees than certificate programs. The exception is CDRs, where private equity buyouts have a stronger negative effect among certificate programs. However, as mentioned above, this is a less reliable variable than repayment rates, where the negative effect is unambiguously driven by schools offering the longer programs whose quality is less observable (Appendix Table A.8 Panel 2 column 6). Together with the law enforcement action evidence, this suggests that when quality is more easily observable, the high-powered incentives to increase profits that private equity buyouts bring are less pernicious.

5.4 Reduced education quality

In many industries, cost reduction is one way that private equity ownership can lead to increased profits. This is not the case in our data, where costs increase after the private equity buyout (Figure 4). Unfortunately, we cannot affirmatively determine the reason for these increased costs due to data limitations. What data we have, however, indicates that sales and marketing expenditure

increase after private equity buyouts, while education inputs decrease. Figure 4 suggests that the instruction share of expenditures declines, though this is not significant in regression analysis (unreported). Table 3 and Table A.8 find decreases in the number of faculty per full-time equivalent student. While we do not have sales data for enough years to analyze in regressions, Figure 1 shows that on average, the sales staff at private equity-owned schools are a much higher share of total staff than any other institution type, well over double that at other for-profits.

Finally, the regressions controlling for student composition indicate that the negative effects of private equity buyouts on student outcomes (e.g. wages, graduation rates) cannot be fully explained by a change in student composition. This requires that the quality of education declines. We conclude that this channel is at play, but the relative weakness of these effects compared to the effects on enrollment and student outcomes results indicate that it is likely not the sole explanation.

5.5 Ability to capture government aid

The final mechanism is that private equity-owned schools achieved higher revenue in substantial part by an improved ability to capture government aid. We test this using two regulatory changes.

5.5.1 Two-year Cohort Default Rate

An important element of the federal government's regulatory policy vis-a-vis schools that rely on federal loans are limits on the extent to which students can default and the institution remain eligible for federal aid. Thus avoiding triggering these thresholds is key for firms operating in this sector. Specifically, the pre-2012 policy held that the share of students that default in the fiscal year after the fiscal year in which they graduated (the two year cohort default rate, or CDR) cannot exceed 25 percent for three years in a row, nor can it be higher than 40 percent in a single year. The two year cohort default rate is the fraction of individuals within a certain repayment cohort who default within two years of entering repayment. We find evidence that private equity owned institutions manage to avoid triggering the thresholds, which can result in loss of federal grants and loans.

Figure 11 shows the density of two-year cohort default rates by institution type. The solid line shows private equity-owned institutions, while the other lines respectively show for-profit and non-profit institutions. The solid vertical line shows the 25 percent two-year cohort default rate threshold. At for-profit institutions not owned by private equity-groups, and at other institutions, we see that cohort default rates largely evolve smoothly across the threshold. Consistent with avoidance regulation inducing thresholds, we see a sharp drop in the default density right before the threshold at private equity-owned institutions.

5.5.2 Gainful Employment Announcement

In this section, we present evidence that the market value of for-profit postsecondary schools is tightly connected to their ability to access federal aid regardless of student outcomes. We exploit four events comprising the introduction, watering down and eventual end of the Gainful Employment (GE) rule, which aimed to tie a school's access to federal grants and federally guaranteed loans to student labor market performance. Consistent with for-profit schools capturing government aid, 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 and ultimately vacated.

This analysis uses data on publicly traded firms. While this approach may seem somewhat disconnected from the paper's focus on private equity, in fact it serves to highlight the role of private equity in building the modern for-profit higher education sector. Currently, the largest purveyors of for-profit higher education are publicly traded, and all of the major public companies has at some point been private equity-owned. We document this in Table 10 shows. All received private equity investment prior to going public, except for Strayer University, which was taken private in a reverse LBO in 2001. The results in Section 4 revealed that the behavior of these formerly private equity owned, publicly traded schools is more similar to private equity owned, privately held schools than to other for-profits. Therefore, this section is both an extension of the private equity analysis, and also demonstrates the relationship between federal aid access and future cash

flows for all for-profits with higher powered incentives than either independent, privately held for profits or community colleges and other nonprofit institutions.

First announced on July 26, 2010, the GE rule would have required graduates to meet debt-to-earnings requirements in order for the college to remain eligible for federal aid.[17] The goal was to eliminate programs in which students took on debt that was unmanageable relative to their expected labor market outcomes. After the for profit industry voiced concerns, the Department of Education announced on April 20, 2011 that the enforcement of the GE rules would be delayed until 2014 if there were "good faith" efforts by institutions to comply. This is the second event we exploit in our analysis. The third event was a rule revision on June 2, 2011, which substantially weakened the original rule.[18] An association of for-profit schools subsequently sued the government. The fourth and final event we exploit is that on June 30, 2012, the U.S. District Court for the District of Columbia struck down the GE rules, finding that the 35 percent repayment standard was not based on any facts. This decision was later appealed, and the rule was ultimately adjusted further, with a new implementation date set to 2015.[19] In 2017, the rules were suspended altogether.[20]

Cumulative abnormal returns follow Campbell et al. [1997]and Acemoglu et al. [2016]. The abnormal return for stock i at date t is given by

$$AR_{it} = R_{it} - (\hat{\alpha}_i + \hat{\beta}_i R_{mt}) \tag{5}$$

where R_{it} is the return of stock i at date t, and R_{mt} is the market return. The terms $\hat{\alpha_i}$ and $\hat{\beta}_i$ are

[17]Specifically, to remain Title IV-eligible, all for-profit and certificate programs would have had to pass at least one of three metrics: 1) at least 35 percent of former students must be in active repayment, defined as reducing their loan annually by at least $1; 2) annual loan payments could not exceed 30 percent of a typical graduate's discretionary income; or 3) annual loan payments could not exceed 12 percent of a typical graduate's total earnings. See IFAP and US News for more information.

[18]Under the 2010 rules, if a school failed three tests, the school would immediately lose access to federal grants and loans. Under the new rules, if schools failed three tests three times in a four year span, access to federal grant and loans programs would be cut. The tests are that (1) at least 35 percent of students are paying down their loans, (2) graduates on average are spending less than 12 percent of their total income on loan payments and (3) graduates on average must be spending less than 30 percent of their discretionary income on loan payments. See the announcement for more information.

[19]See US News for more information.

[20]See the Washington Post for more information.

estimated from the following equation

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \tag{6}$$

Equation 6 is estimated for the 250 day trading period from 270 days prior to the event period.[21] The abnormal return in Equation 5 is calculated for each day of the event window, which encompasses the 20 trading days before to the 20 trading days after the event date. Firms are excluded if they are in the data for fewer than 150 days of the estimation window or fewer than 20 days of the event window.

Cumulative abnormal returns (CAR) are then calculated as

$$CAR[0, n] = \sum_{t=0}^{n} AR_{it}, \tag{7}$$

where n is the day following the start of the analysis period, 20 trading days prior to the event date. We compare fifteen firms that own for-profit institutions with GE data available between 2010 and 2015. Control firms for the event studies are publicly traded firms with the same first three-digit NAICS as those in the treatment sample. Thus, the control firms are those with NAICS codes with 611 (Educational Services) and 812 (Personal Services) as the first three digits, which includes 48 controls firms in total.

Figure 12 shows the CAR results. Each panel shows CAR values before and after a regulatory event. Time denotes days, and prices are measured at the close of each trading day. The top left graph shows the announcement of the GE rules on July 26, 2010.[22] There is a sharp drop in CAR for exposed firms. In contrast, we see no discernible pattern for education firms unaffected by the GE rules. The top right graph shows that the opposite relationship holds when the rules were delayed on April 20, 2011. The CAR jumps sharply for firms affected by the GE rules. We see no such effects for schools in the control group.

[21] This estimation period is chosen to prevent the estimation period from influencing market returns and the expected return calculation.

[22] See the announcement for more information.

The bottom right graph of Figure 12 shows the jump in CAR following the June 2, 2011 rule weakening. Again we see no response for the control group. Finally, the bottom right graph shows that CAR again jumped as it became clear that the court would strike the GE rules.

Table 11 presents results analogous to those in Figure 12. We use variants of the following specification

$$CAR_{it} = \alpha_i + \alpha_t + \delta FP_i * Post_t + \varepsilon_{it}, \tag{8}$$

where CAR_{it} are the cumulative abnormal returns for firm *i* on day *t*. We include firm effects α_i, which absorb time invariant firm specific factors. Trading day fixed effects α_t absorb market-wide factors. The coefficient of interest is δ, which gives us the differential effect of the treatment group, firms owning for-profit colleges, relative to the control group following the announcement.

The first three columns of Table 11 show results for the initial announcement of GE rules. The first column presents simple difference and difference estimates post and treatment dummies, the second column adds in date fixed effects, while the third column includes both sets of fixed effects. Consistent with the graphical evidence, we see a sharp drop in CAR, and the effect is statistically significant at the .05 level or higher in all specifications. Columns (4) through (6) repeat the analysis for the GE rules being held, while columns (7) through (9) repeats the analysis for the announcement of the new less restrictive GE rules, and columns (10) through (12) repeat the analysis for the court striking down the GE rules. The estimates regarding the GE rules being held and the announcement of the new GE rules are consistent with the graphical results in Figure 12, while the estimates in columns (10) through (12) are statistically insignificant at conventional levels.

In sum, this analysis provides additional evidence that a major aspect of for-profit market value is rent-seeking capture of government aid, which is unambiguously not in students' or taxpayers' interests. We focus on publicly traded for-profits, the largest of which were once private equity owned, and which likely have higher-powered incentives than independent, privately held for-profit schools. Our result does not in itself imply that private equity buyouts do not improve education

quality. However, in combination with the other evidence in Section 5 (loan limit increase and CDR bunching), it indicates that superior rent-seeking federal aid capture is an important channel through which high-powered incentives translate to higher profits.

6 Conclusion

Employing a novel, hand collected dataset on 88 private equity deals and 639 campus-level ownership changes, which we match to Department of Education data, we find that private equity investments lead to expanded enrollment and increased profits, but also to higher average debt per student, lower graduation rates, lower repayment rates, and lower earnings. We also use regulatory changes to show that private equity owned schools raise tuition following credit expansions at a faster rate relative to other schools, which leads to increased levels of debt.

Our findings suggest that in the education industry, high-powered incentives to maximize profit may operate counter to customers' (students') interest. Existing literature concludes that private equity buyouts add value to target firms. There is less work on the benefits of private equity buyouts to consumers. The benefits, or lack thereof, depend crucially on the alignment of incentives between firms and customers.

High levels of government intervention, complex or opaque product quality, and customer outcomes measurable only many years after payment characterize the education industry. All of these may affect how high-powered incentives to maximize profit operate translate to consumer interests. Future research is needed on whether some of these features are more important than others for incentive misalignment. In particular, an important avenue for future research is whether private equity buyouts have similar negative effects on consumer outcomes in other sectors with intense government subsidy, such as infrastructure and health care.

References

Alberto Abadie and Guido W Imbens. Bias-corrected matching estimators for average treatment effects. *Journal of Business & Economic Statistics*, 29(1):1–11, 2011.

Daron Acemoglu, Simon Johnson, Amir Kermani, James Kwak, and Todd Mitton. The value of connections in turbulent times: Evidence from the united states. *Journal of Financial Economics*, (121):368–391, 2016.

Peter Arcidiacono, Esteban Aucejo, Arnaud Maurel, and Tyler Ransom. College Attrition and the Dynamics of Information Revelation. 2016.

Luis Armona, Rajashri Chakrabarti, and Michael F Lovenheim. How Does For-profit College Attendance Affect Student Loans, Defaults and Earnings? 2017.

Elizabeth Popp Berman and Abby Stivers. Student loans as a pressure on US higher education. *The University under Pressure*, 46:129–160, 2016.

Shai Bernstein and Albert Sheen. The operational consequences of private equity buyouts: Evidence from the restaurant industry. *Review of Financial Studies*, page hhw037, 2016. ISSN 0893-9454.

Eric P Bettinger, Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu. 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, 2012.

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

Quentin Boucly, David Sraer, and David Thesmar. Growth {LBO}s. *Journal of Financial Economics*, 102(2):432–453, 2011.

John Y Campbell, Andrew Wen-Chuan Lo, and Archie Craig MacKinlay. *The Econometrics of Financial Markets*. Princeton University Press, 1997.

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

Kevin Carey. Revised Data Shows Community Colleges Have Been Underappreciated, oct 2017.

Stephanie Cellini, Rajeev Darolia, and Lesley Turner. Where Do Students Go When For-Profit Colleges Lose Federal Aid? *Working Paper*, 2017.

Stephanie Riegg Cellini. Financial aid and forâprofit colleges: Does aid encourage entry? *Journal of Policy Analysis and Management*, 29(3):526–552, 2010. ISSN 1520-6688.

Stephanie Riegg Cellini and Latika Chaudhary. The labor market returns to a for-profit college education. *Economics of Education Review*, 43:125–140, 2014.

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

Stephanie Riegg Cellini and Nicholas Turner. Gainfully Employed? Assessing the Employment and Earnings of For-Profit College Students Using Administrative Data. Technical report, National Bureau of Economic Research, 2016.

Judith A Chevalier and David S Scharfstein. Capital-Market Imperfections and Countercyclical Markups: Theory and Evidence. *The American Economic Review*, pages 703–725, 1996.

Anna S Chung. Choice of for-profit college. *Economics of Education Review*, 31(6):1084–1101, 2012.

Tressie McMillan Cottom. *Lower ed: The troubling rise of for-profit colleges in the new economy*. New Press, The, 2017.

Rajeev Darolia, Cory Koedel, Paco Martorell, Katie Wilson, and Francisco Perez-Arce. Do Employers Prefer Workers Who Attend For-Profit Colleges? Evidence from a Field Experiment. *Journal of Policy Analysis and Management*, 34(4):881–903, 2015.

Steven J Davis, John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda. Private equity, jobs, and productivity. *The American Economic Review*, 104(12):3956–3990, 2014.

David J Deming, Claudia Goldin, and Lawrence F Katz. The For-Profit Postsecondary School Sector: Nimble Critters or Agile Predators? *Journal of Economic Perspectives*, 26(1):139–64, dec 2012.

David J Deming, Noam Yuchtman, Amira Abulafi, Claudia Goldin, and Lawrence F Katz. The value of postsecondary credentials in the labor market: An experimental study. *The American Economic Review*, 106(3):778–806, 2016. ISSN 0002-8282.

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

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

Sarena Goodman, Adam Isen, and Constantine Yannelis. A Day Late and a Dollar Short: Limits, Liquidity and Household Formation for Student Borrowers. 2017.

Oliver Gottschalg, Ludovic Phalippou, and Others. The truth about private equity performance. *Harvard Business Review*, 85(12):17–20, 2007.

Guilbert C Hentschke. Innovations in business models and organizational cultures: The for-profit sector. *USC Rossier School of Education (June)*, 2010.

Stefano M Iacus, Gary King, and Giuseppe Porro. Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, 20(1):1–24, 2012.

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

Ozan Jaquette and Edna E Parra. Using IPEDS for panel analyses: Core concepts, data challenges, and empirical applications. In *Higher education: Handbook of theory and research*, pages 467–533. Springer, New York, 2014. ISBN 9401780048.

Michael C Jensen. Eclipse of the public corporation. *Harvard Business Review*, 1989.

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

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

Steven N Kaplan and Antoinette Schoar. Private equity performance: Returns, persistence, and capital flows. *The Journal of Finance*, 60(4):1791–1823, 2005. ISSN 1540-6261.

Steven N Kaplan and Per Strömberg. Leveraged buyouts and private equity. *The Journal of economic perspectives*, 23(1):121–146, 2009. ISSN 0895-3309.

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

Kevin Lang and Russell Weinstein. The wage effects of not-for-profit and for-profit certifications: Better data, somewhat different results. *Labour Economics*, 24:230–243, 2013.

Josh Lerner, Morten Sorensen, and Per Strömberg. Private equity and long-run investment: The case of innovation. *The Journal of Finance*, 66(2):445–477, 2011.

Yuen Ting Liu and Clive Belfield. 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*, 2014.

Alexander Ljungqvist, Lars Persson, and Joachim Tag. Private Equity's Unintended Dark Side: On the Economic Consequences of Excessive Delistings. 2016.

Adam Looney and Constantine Yannelis. 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*, pages 1–89, 2015.

David O Lucca, Taylor Nadauld, and Karen Chen. Credit supply and the rise in college tuition: Evidence from the expansion in federal student aid programs. 2016.

Andrew Metrick and Ayako Yasuda. The economics of private equity funds. *The Review of Financial Studies*, 23(6):2303–2341, 2010.

Andrew Metrick and Ayako Yasuda. Venture capital and other private equity: a survey. *European Financial Management*, 17(4):619–654, 2011.

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

Frank Schilbach, Heather Schofield, and Sendhil Mullainathan. The psychological lives of the poor. *The American Economic Review*, 106(5):435–440, 2016.

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

Robert Shireman. Learn Now, Pay Later: A History of Income-Contingent Student Loans in the United States. *The ANNALS of the American Academy of Political and Social Science*, 671(1): 184–201, 2017. ISSN 0002-7162.

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

R Wilson. Two banking industry representatives play key lobbying role on student loans. *Chronicle of Higher Education*, page 26, 1987.

Matthew Wiswall and Basit Zafar. Determinants of College Major Choice: Identification using an Information Experiment. *Review of Economic Studies*, 82(2):791–824, 2014.

Table 1: Descriptive Statistics by Institution Type

Panel 1: Firm-year variables

	Nonprofit, State	**Community**	**For profit, not PE**	**PE owned**
N (firm/institution-year obs)	47,827	23,929	47,827	570
	Mean (Std Dev)	**Mean (Std Dev)**	**Mean (Std Dev)**	**Mean (Std Dev)**
Profits (mill 2015$)			2	4
			(20)	(92)
Total revenue (mill 2015$)	128	44	9	169
	(288)	(78)	(66)	(306)
Total expenditure (mill 2015$)	94	39	7	122
	(197)	(64)	(48)	(218)
Net tuition revenue (mill 2015$)	34	60	11	117
	(60)	(9)	(37)	(117)
Net tuition revenue per FTE student (2015$)	11,131	3,607	12,044	12,988
	(7,545)	(3,821)	(7,476)	(7,258)

Note: This table compares institution types at the firm (SystemID) level. ‡Full-time equivalent (applies to all below). *Grad rate at 150% of normal time for all levels 1995 to 2010. †Exiting cohort defalault rate 2 years after cohort exit

Panel 2: School-year variables

	Nonprofit, State	**Community**	**For profit, not PE**	**PE owned**
N (school-year obs)	55,095	29,678	34,342	8,611
	Mean (Std Dev)	**Mean (Std Dev)**	**Mean (Std Dev)**	**Mean (Std Dev)**
Publicly traded			0.05	0.44
			(0.22)	(0.50)
Selective admissions	0.49	0.14	0.16	0.11
	(0.50)	(0.35)	(0.36)	(0.31)
Highest degree offered	1.3	2.25	2.57	2.02
	(0.64)	(0.60)	(0.66)	(0.79)
Students‡	2,792	2,678	298	778
	(4,954)	(3,629)	(1,018)	(1,724)
Undergraduates	2,919	4,165	292	790
	(4,810)	(5,220)	(914)	(1,839)
1st law enforcement action	0.0000	0.0000	0.0001	0.0026
	(0.0059)	(0.0044)	(0.0119)	(0.0507)
Faculty per 100 students±	7.10	4.60	4.33	3.30
	(5.84)	(4.39)	(4.20)	(2.46)
Share students black	0.12	0.13	0.20	0.27
	(0.20)	(0.16)	(0.24)	(0.23)
Share students white	0.68	0.69	0.53	0.41
	(0.29)	(0.25)	(0.32)	(0.26)
Pell grants per student (2015 $)	1,071	1,394	3,501	3,875
	(1,720)	(1,208)	(2,741)	(2,596)
Graduation rate	0.52	0.28	0.56	0.47
	(0.20)	(0.19)	(0.25)	(0.20)
Loan per borrower (2015 $)	3,858	3,952	6,288	8,008
	(2,388)	(2,017)	(2,977)	(2,731)
Cohort default rate (2 year)†	0.05	0.10	0.12	0.11
	(0.06)	(0.08)	(0.11)	(0.06)
Repayment rate (3 year)††	0.66	0.49	0.43	0.35
	(0.17)	(0.12)	(0.17)	(0.15)
Ave earnings after school (2015 $)	49,067	36,914	31,787	34,789
	(15,121)	(7,394)	(11,283)	(10,459)

Note: This table compares institution types at the school (UnitID) level. ‡Full-time equivalent (applies to all below). *Grad rate at 150pct normal time for programs of 2 years or less duration. †Exiting cohort default rate 2 years after cohort exit. ±Full time faculty. ††Share of students in repayment after three years (have paid back at least $1 in principal).

Table 2: Private Equity Ownership and Financial Outcomes

Panel 1

Dependent variable*	Profits (mill 2015$)					Total revenue (mill 2015$)				
				NNM±	CEM±				NNM±	CEM±
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PE owned	1.1***	1.2***	1.1***	2.5***	2*	.95***	.97***	.9***	2***	.91**
	(.23)	(.24)	(.24)	(.37)	(1)	(.15)	(.14)	(.15)	(.23)	(.45)
Public	.78***	1.7***	1.1***			1***	1.4***	1***		
	(.38)	(.31)	(.38)			(.28)	(.29)	(.27)		
PE owned·Public		-.86					-.53			
		(.63)					(.5)			
Composition controls‡	N	N	Y	-	-	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-	Y	Y	Y	-	-
Selective admissions f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
School f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
N	80245	80245	80245	9855	1176	80245	80245	80245	11566	1241
R^2	.84	.84	.84	-	.0032	.97	.97	.97	-	.0033

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on firm (highest level of school or chain) financials. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. *All in logged millions of 2015 dollars. Observations are at the firm (SystemID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Panel 2

Dependent variable*	Net tuition revenue (mill 2015$)					Total expenditure (mill 2015$)				
				NNM±	CEM±				NNM±	CEM±
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PE owned	.78***	.79***	.72***	1.8***	.78	.9***	.92***	.86***	2***	1.1***
	(.15)	(.14)	(.15)	(.33)	(.52)	(.16)	(.15)	(.16)	(.24)	(.43)
Public	.71***	.89***	.7***			1***	1.3***	.99***		
	(.22)	(.28)	(.21)			(.27)	(.31)	(.27)		
PE owned·Public		-.26					-.44			
		(.42)					(.5)			
Composition controls‡	N	N	Y	-	-	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-	Y	Y	Y	-	-
School f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
N	80245	80245	80245	4048	547	80245	80245	80245	10815	1241
R^2	.95	.95	.96		.004	.97	.97	.97		.0055

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on firm (highest level of school or chain) financials. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. *All in logged millions of nominal dollars. Observations are at the firm (SystemID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with ∗, ∗∗,∗∗∗ , denote p < .1, p < .05, p < .01, respectively.

Table 3: Private Equity Ownership and School Operations

Panel 1

Dependent variable*	Number of FTE students					Number of Undergraduates				
				NNM±	CEM±				NNM±	CEM±
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PE owned	.29***	.33***	.3***	.69***	.75***	.27***	.32***	.29***	.66***	.64***
	(.062)	(.053)	(.061)	(.13)	(.054)	(.062)	(.054)	(.063)	(.12)	(.054)
Public	.14**	.33***	.16**			.18***	.36***	.2***		
	(.056)	(.076)	(.057)			(.058)	(.076)	(.059)		
PE owned·Public		-.28***					-.27***			
		(.086)					(.09)			
Public	.14**	.33***	.16**			.18***	.36***	.2***		
	(.056)	(.076)	(.057)			(.058)	(.076)	(.059)		
Composition controls‡	N	N	Y	-	-	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-	Y	Y	Y	-	-
Selective admissions f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
School f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
N	127186	127186	127186	24091	17865	127186	127186	127186	24083	16839
R^2	.97	.97	.97	-	.011	.96	.96	.97	-	.0085

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. *All logged. Observations are at the school (UnitID)-year level. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Standard errors are clustered by institution/firm (a firm may have multiple schools, or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with ∗, ∗∗,∗∗∗ , denote p < .1, p < .05, p < .01, respectively.

Panel 2

Dependent variable*	Faculty per 100 students					1st law enforcement action			
				NNM±	CEM±				NNM±
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PE owned	-.35	-.42**	-.28	.35	-.042	.0026***	.0026***	.0025***	.00018
	(.21)	(.2)	(.2)	(.55)	(.55)	(.00058)	(.00056)	(.00057)	(.0033)
Public	-.44***	-.72***	-.42***			.00094	.00091	.00085	
	(.11)	(.25)	(.1)			(.0014)	(.00072)	(.0014)	
PE owned·Public		.41					.000046		
		(.29)					(.0014)		
Composition controls‡	N	N	Y	-	-	N	N	Y	-
Highest degree offered f.e.†	Y	Y	Y	-	-	Y	Y	Y	-
Selective admissions f.e.	Y	Y	Y	-	-	Y	Y	Y	-
School f.e.	Y	Y	Y	-	-	Y	Y	Y	-
Year f.e.	Y	Y	Y	-	-	Y	Y	Y	-
N	58754	58754	58754	5110	2211	127186	127186	127186	24091
R^2	.82	.82	.82	-	2.6e-06	.14	.14	.14	-

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. *All logged. Observations are at the school (UnitID)-year level. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. For law enforcement actions, the algorithm failed to find any matched strata. Standard errors are clustered by institution/firm (a firm may have multiple schools, or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with ∗, ∗∗,∗∗∗ , denote p < .1, p < .05, p < .01, respectively.

Table 4: Private Equity Ownership and Student Demographics

Dependent variable*	Share students black				Share students white				Pell grants per student (2015$)			
			NNM±	CEM±			NNM±	CEM±			NNM±	CEM±
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
PE owned	.027***	.028***	.0026	.041***	-.04***	-.052***	-.07**	-.034***	-98	40	240	634***
	(.0064)	(.0067)	(.027)	(.0088)	(.0079)	(.0064)	(.034)	(.012)	(282)	(305)	(204)	(184)
Public	.02***	.026**			-.055***	-.1***			286	516		
	(.0063)	(.01)			(.012)	(.023)			(215)	(311)		
PE owned·Public		-.0079				.071***				-341		
		(.011)				(.026)				(432)		
Degree f.e.†	Y	Y	-	-	Y	Y	-	-	Y	Y		
Admission f.e.	Y	Y	-	-	Y	Y	-	-	Y	Y		
Year f.e.	Y	Y	-	-	Y	Y	-	-	Y	Y		
N	127186	127186	24101	22107	127186	127186	24101	22107	127186	127186	26007	11773
R^2	.93	.93	-	.00099	.91	.92	-	.00034	.69	.69	-	.001

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on student demographic outcomes. *All logged. Observations are at the school (UnitID)-year level. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Standard errors are clustered by institution/firm (a firm may have multiple schools, or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 5: Private Equity Ownership and Graduation Rates

Dependent variable: Share students graduating in 150% normal time					
				NNM	CEM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)
PE owned	-.052***	-.063***	-.051***	-.053**	-.074***
	(.014)	(.014)	(.013)	(.022)	(.021)
Public	-.023	-.063***	-.02		
	(.027)	(.016)	(.026)		
PE owned·Public		.059*			
		(.03)			
Composition controls‡	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-
Selective admissions f.e.	Y	Y	Y		
School f.e.	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-
N	58137	58137	58137	9427	7040
R^2	.8	.8	.8	-	.0018

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on student demographic outcomes. Observations are at the school (UnitID)-year level. $^{\pm}$The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Standard errors are clustered by institution/firm (a firm may have multiple schools, or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 6: Private Equity Ownership and Student Borrowing

Panel 1

Dependent variable	Average loan per borrower (2015$)				
				NNM$^{\pm}$	CEM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)
PE owned	805***	961***	796***	502*	639***
	(260)	(295)	()	(268)	(151)
Public	-214	657	-76		
	(734)	(777)	(622)		
PE owned·Public		-1168			
		(971)			
Composition controls‡	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-
Selective admissions f.e.	Y	Y	Y		
School f.e.	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-
N	77799	77799	77799	21111	9935
R^2	.75	.75	.75	-	.0018

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on on student borrowing, default, and repayment. $^{\pm}$The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Observations are at the school (UnitID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Panel 2

Dependent variable	Cohort default rate (2 year)					Repayment rate (3 year)				
				NNM$^{\pm}$	CEM$^{\pm}$				NNM$^{\pm}$	CEM$^{\pm}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PE owned	.00044	.0042	-.00093	.015**	.023**	-.03**	-.027*	-.025**	-.036**	-.07**
	(.0048)	(.005)	(.0047)	(.0076)	(.0093)	(.01)	(.011)	(.009)	(.016)	(.031)
Public	.0058	.02***	.0043			-.036**	-.026*	-.031**		
	(.0071)	(.0064)	(.0068)			(.011)	(.011)	(.01)		
PE owned·Public		-.021**					-.013			
		(.0092)					(.0092)			
Composition controls‡	N	N	Y	-	-	N	N	Y	-	-
Highest degree offered f.e.†	Y	Y	Y	-	-	Y	Y	Y	-	-
Selective admissions f.e.	Y	Y	Y			Y	Y	Y		
School f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
Year f.e.	Y	Y	Y	-	-	Y	Y	Y	-	-
N	83002	83002	83002	9635	5855	20357	20357	20357	4523	1100
R^2	.61	.61	.61	-	.001	.96	.96	.96	-	.0048

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on student borrowing, default, and repayment.$^{\pm}$The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Observations are at the school (UnitID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 7: Private Equity Ownership and Student Wages Six Years After Graduation

Panel 1

Dependent variable*	Mean			10th pctile			25th pctile		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PE owned	-.028*	-.043**	-.023*	-.0078	-.035	.0026	-.019	-.039**	-.014
	(.011)	(.010)	(.01)	(.025)	(.021)	(.023)	(.013)	(.013)	(.013)
Public	-.055**	-.091**	-.045*	-.11**	-.18**	-.094**	-.065*	-.11*	-.054
	(.019)	(.021)	(.02)	(.032)	(.053)	(.034)	(.029)	(.041)	(.028)
PE owned·Public		.068***			.13*			.091*	
		(.013)			(.051)			(.031)	
Composition controls‡	N	N	Y	N	N	Y	N	N	Y
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y	Y	Y	Y
Selective admissions f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
School f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	17238	17238	17238	17238	17238	17238	17238	17238	17238
R^2	.97	.97	.97	.91	.91	.91	.95	.95	.95

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on student wages. *Log wages six years after graduation, measured within a school's graduating cohort, and in 2015$. For example, the mean (columns 1-2) is the mean within a school (UnitID) graduation class. We do not use matching estimators because wages are only observed for 6 cohorts, making it impossible to create a tractable sample matched the year prior to the buyout. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Panel 2

Dependent variable*	Median (50th pctile)			75th pctile			90th pctile		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PE owned	-.022	-.037**	-.017	-.028**	-.042**	-.024*	-.032*	-.045**	-.028*
	(.010)	(.012)	(.01)	(.010)	(.01)	(.009)	(.012)	(.011)	(.012)
Public	-.055*	-.092**	-.045	-.052**	-.086***	-.045*	-.05**	-.083***	-.043*
	(.022)	(.024)	(.023)	(.017)	(.019)	(.018)	(.018)	(.016)	(.017)
PE owned·Public		.07***			.064***			.062***	
		(.013)			(.011)			(.013)	
Composition controls‡	N	N	Y	N	N	Y	N	N	Y
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y	Y	Y	Y
Selective admissions f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
School f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	17238	17238	17238	17238	17238	17238	17238	17238	17238
R^2	.96	.96	.96	.96	.96	.96	.95	.95	.95

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on student wages. *Wages are six years after graduation, measured within a school's graduating cohort, and in 2015$. For example, the mean (columns 1-2) is the mean within a school (UnitID) graduation class. We do not use matching estimators because wages are only observed for 6 cohorts, making it impossible to create a tractable sample matched the year prior to the buyout. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with ∗, ∗∗,∗∗∗ , denote p < .1, p < .05, p < .01, respectively.

Table 8: Loan Limit Increase: Borrowing

Dependent Variable: Average loan per borrower ($)						
	(1)	(2)	(3)	(4)	(5)	(6)
PE X Post 2007	746.9***	998.0***	1000.7***	661.1***	991.1***	998.7***
	(215.3)	(230.1)	(230.1)	(249.6)	(256.9)	(261.4)
PE	908.0***			408.9*		
	(216.4)			(212.2)		
Post	2468.2***			2515.4***		
	(56.56)			(143.2)		
Controls	No	No	Yes	No	No	Yes
Sample	All	All	All	For-Profits	For-Profits	For-Profits
School Fixed Effects	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Observations	63,768	63,768	63,768	26,666	26,666	26,666

Note: *Standard errors are clustered at the system level. Coefficients marked with ∗, ∗∗,∗∗∗ , denote p < .1, p < .05, p < .01, respectively.

Table 9: Loan Limit Increase: Tuition

Dependent Variable: Average tuition ($)						
	(1)	(2)	(3)	(4)	(5)	(6)
PE X Post 2007	1803.8***	1255.8**	1359.4**	1325.4**	1266.3	1313.8*
	(497.9)	(565.8)	(584.9)	(668.0)	(771.3)	(779.4)
PE	4423.1***			1872.1***		
	(510.6)			(704.5)		
Post 2007	2555.4***			4464.9***		
	(133.2)			(217.8)		
Controls	No	No	Yes	No	No	Yes
Sample	All	All	All	For-Profits	For-Profits	For-Profits
School Fixed Effects	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes	No	Yes	Yes
Observations	58,294	58,294	58,294	13,073	13,073	13,073

Note: *Standard errors are clustered at the system level. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table 10: Major Publicly Traded Higher Education Institutions

	First private equity investment/buyout	**IPO date**	**Private equity reverse LBO date (public to private)**	**Second IPO date**	**Share of for-profit enrollment in 2010**
EDMC	1986	1996	2006	2009	2.7%
Devry	1987	1991			2.8%
Corinthian	1995†	1999			2.1%
Capella	1995	2006			1.6%
Strayer		1996	2001		2.2%
Apollo (U. of Phoenix)		2000	2017		20.2%
Grand Canyon	2004	2008			1.4%
Laureate	2007	2017			1.8%

Note: This table lists the largest for profit higher education institutions ever publicly traded. †PE-financed acquisition of 15 campuses.

Table 11: Gainful Employment Event Studies

Panel 1

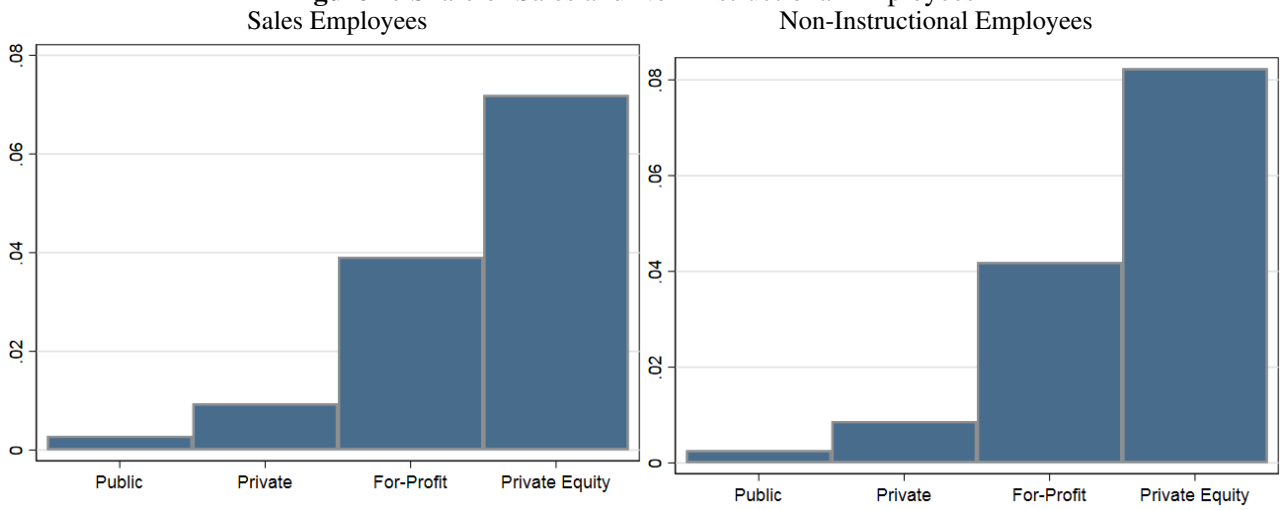
Event:	GE Rules Announced			GE Rules Held		
Dependent Variable: Cumulative Abnormal Returns						
	(1)	(2)	(3)	(4)	(5)	(6)
PE X Post 2007	-0.186***	-0.186***	-0.186***	0.0340*	0.0340*	0.0340*
	(0.0340)	(0.0344)	(0.0348)	(0.0189)	(0.0191)	(0.0193)
PE	-0.0321**	-0.0321**		-0.00226	-0.00226	
	(0.0146)	(0.0148)		(0.0126)	(0.0127)	
Post	0.00455			-0.0126		
	(0.0181)			(0.0142)		
Firm Fixed Effects	No	No	Yes	No	No	Yes
Date Fixed Effects	No	Yes	Yes	No	Yes	Yes
Observations	1845	1845	1845	2050	2050	2050

Panel 2

Event:	Revised GE Rules			Court Strikes GE Rules		
Dependent Variable: Cumulative Abnormal Returns						
	(1)	(2)	(3)	(4)	(5)	(6)
PE X Post 2007	0.135***	0.135***	0.135***	-0.00734	-0.00658	-0.00729
	(0.0245)	(0.0248)	(0.0251)	(0.0412)	(0.0417)	(0.0421)
PE	0.0264	0.0264		0.00825	0.00825	
	(0.0198)	(0.0200)		(0.0155)	(0.0156)	
Post	-0.0192			-0.0334		
	(0.0134)			(0.0227)		
Firm Fixed Effects	No	No	Yes	No	No	Yes
Date Fixed Effects	No	Yes	Yes	No	Yes	Yes
Observations	2050	2050	2050	1957	1957	1957

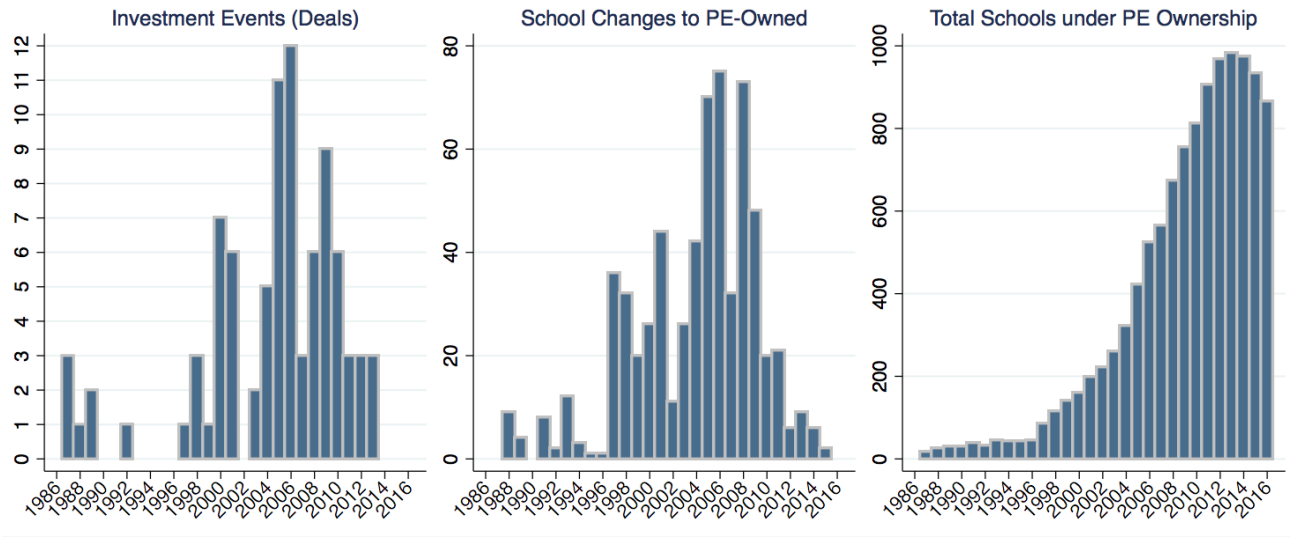
Note: *Average Cumulative Abnormal Returns for the stocks are calculated around 60-day event windows, $CAR[0,n] = \sum_{t=0}^{n} AR_{it}$. Standard errors are clustered at the firm level. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Figure 1: Share of Sales and Non-Instructional Employees



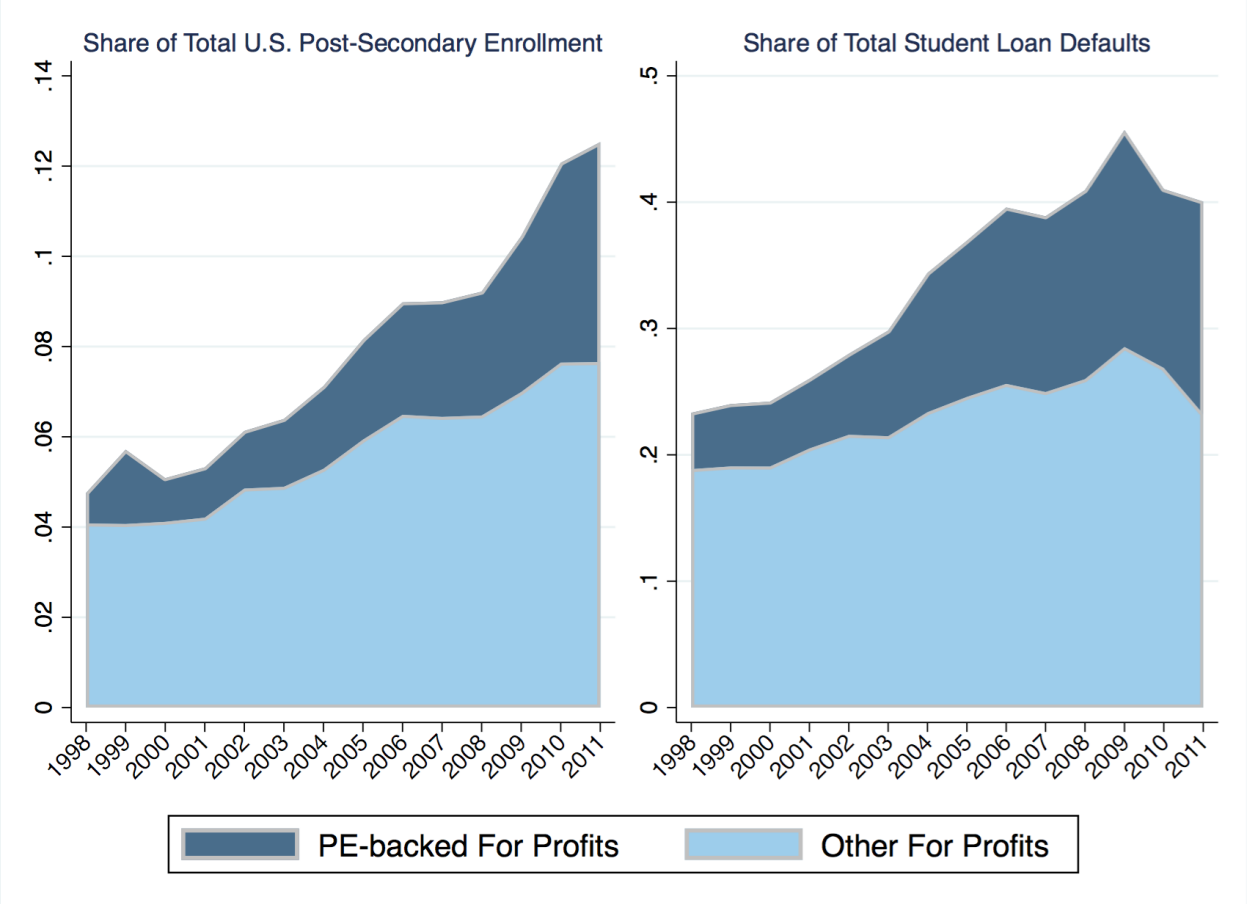
Note: The figure above shows the share of sales and non-instructional staff by institution type. Data on sales and non-instructional staff comes from IPEDS.

Figure 2: Private Equity Deals and School Ownership



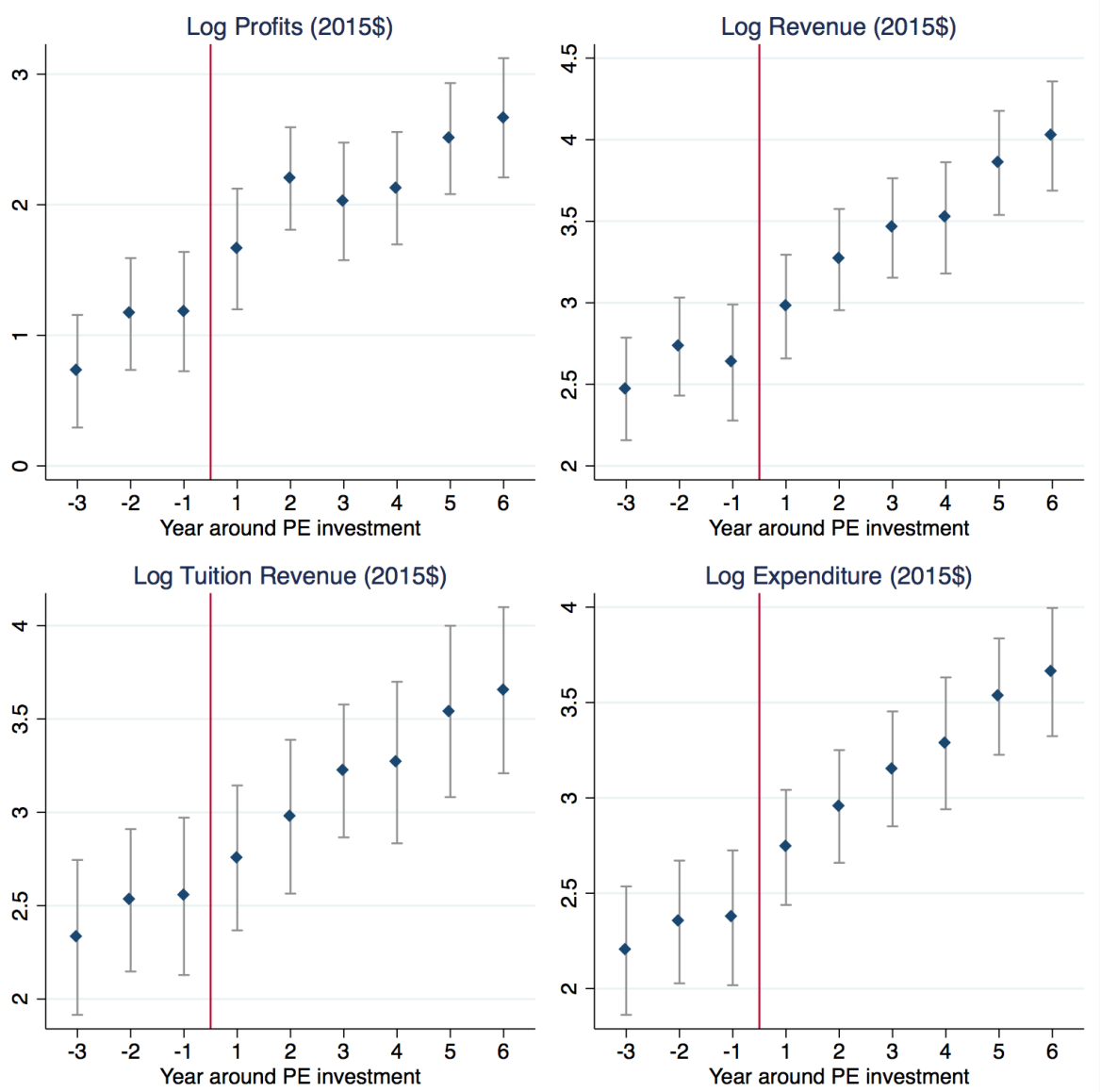
Note: The left-hand figure shows the 88 PE deals (mostly buyouts) in our data; these are PE firm investments in schools or chains of schools. The middle figure shows school (UnitID)-level ownership changes to PE. The right-hand figure shows the total number of schools under PE ownership. Data collected by the authors.

Figure 3: For Profit Schools Share of Loan Defaults and Enrollment



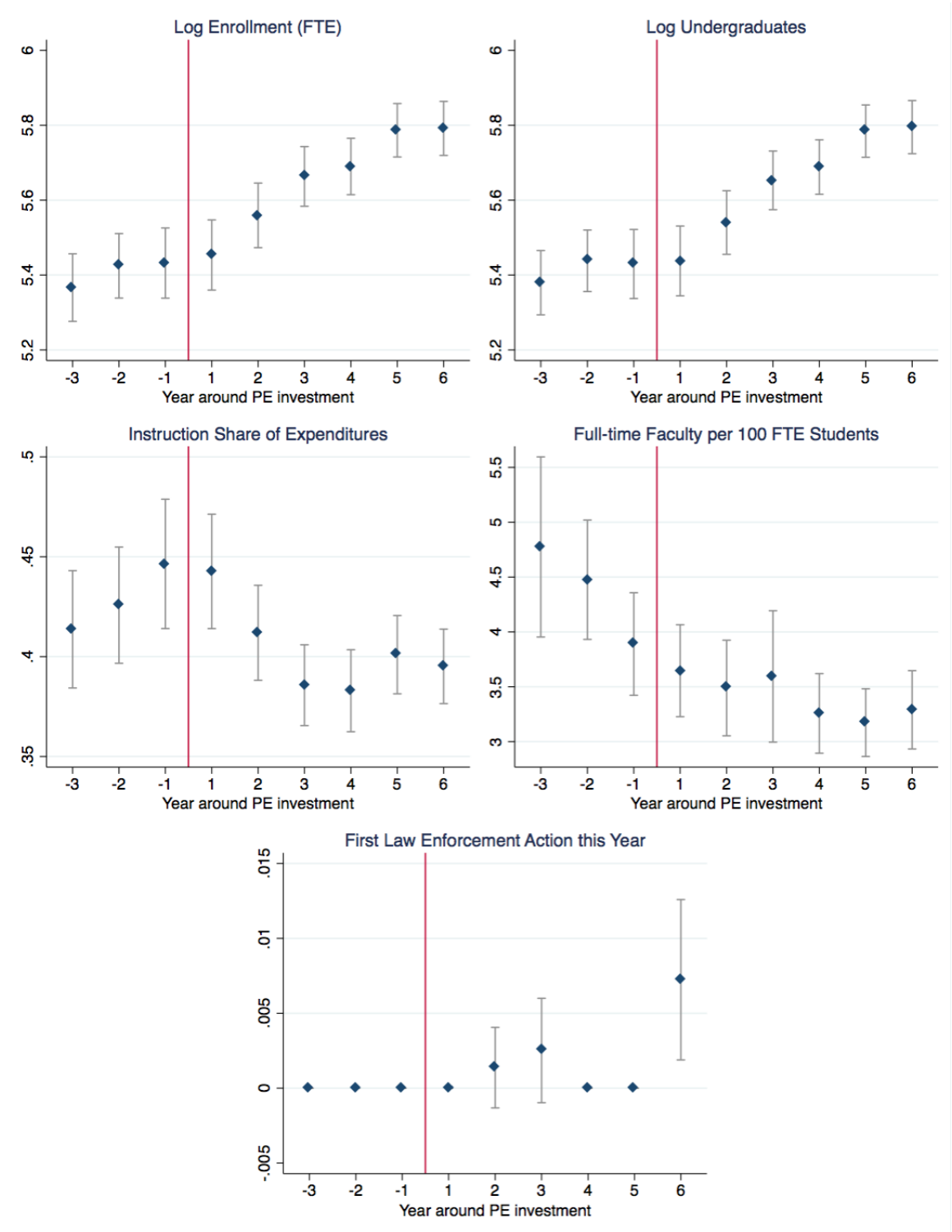
Note: The figure on the left shows the share of total US Post-Secondary enrollment in the for profit sector, broken down by private equity backed institutions and non-private equity backed institutions. The figure on the right shows the share of total student loan defaults within two years of entering repayment, broken down by private equity backed institutions and non-private equity backed institutions.

Figure 4: Financials around Private Equity Ownership Change



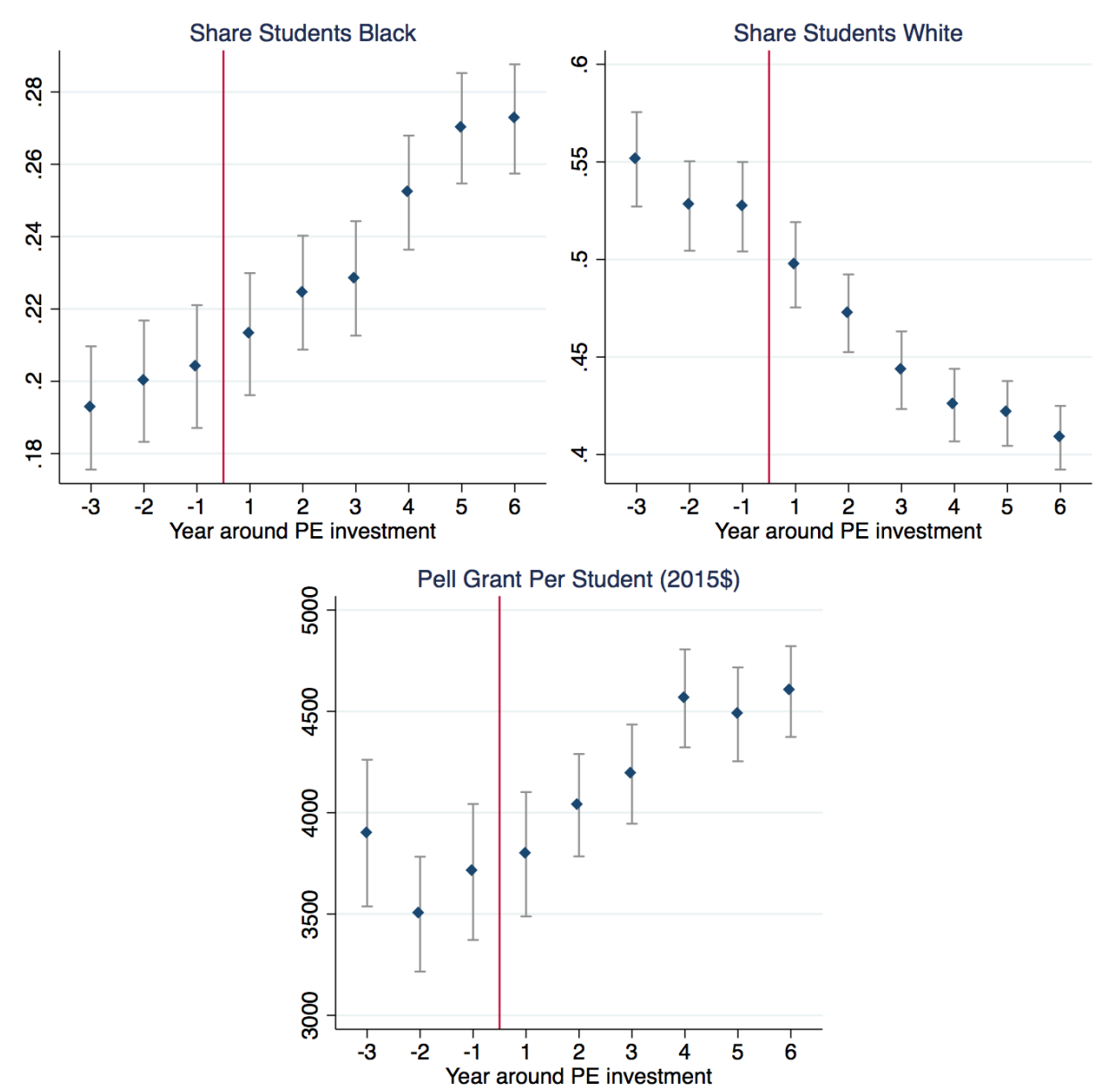
Note: The figures above show, within the sample of school systems bought by PE, the means of financial variables in the years around the ownership change. The level of observation is the ultimate parent company, or SystemID (our shorthand). This is not the PE deal level. A PE deal may involve multiple SystemIDs, and PE-owned SystemIDs may acquire other SystemIDs. 95% confidence intervals shown.

Figure 5: Operations around Private Equity Ownership Change



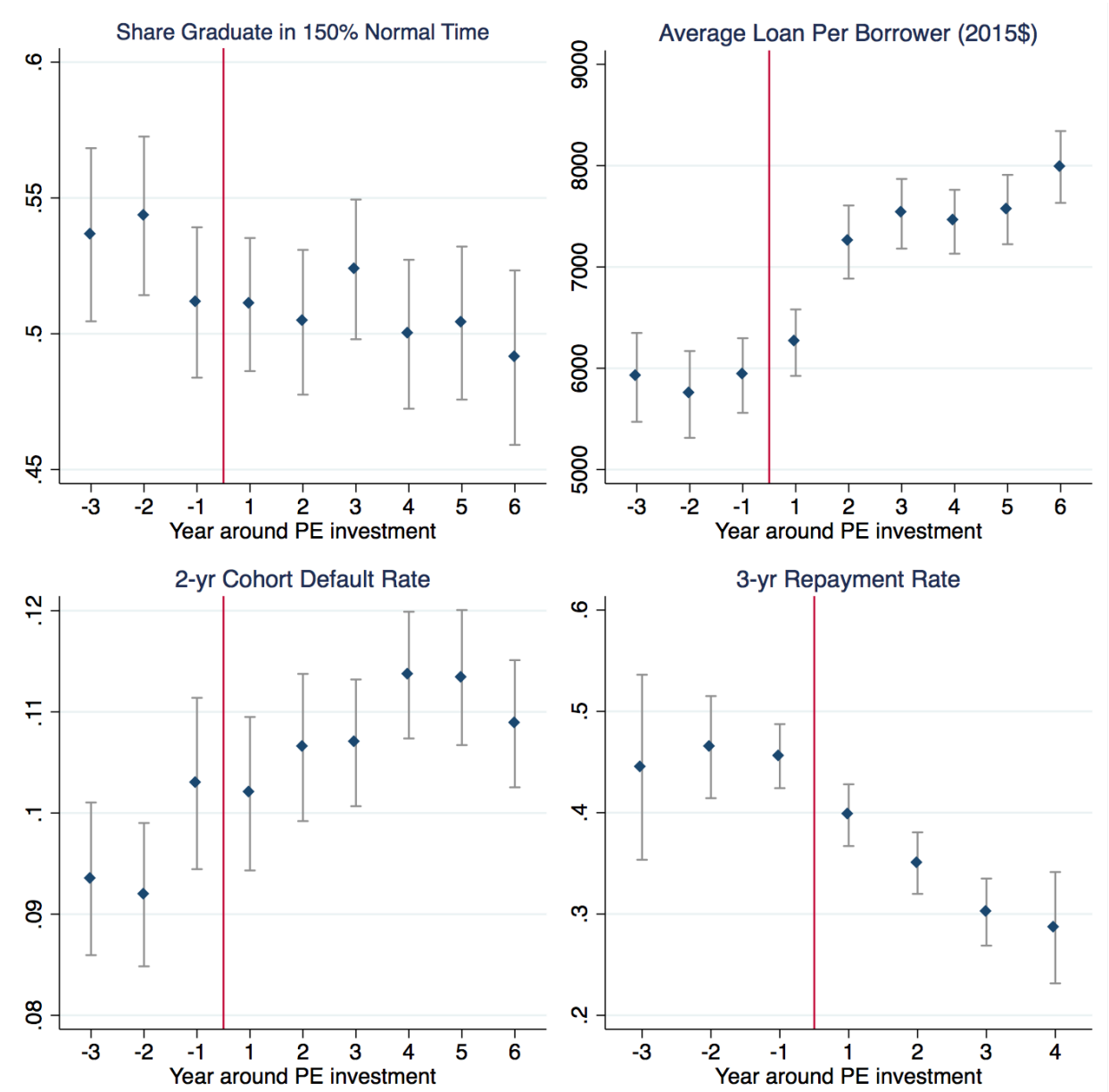
Note: The figures above show, for the sample of schools bought by PE, the means of operations-related variables in the years around the ownership change. This data is at the school, or UnitID level, except for the share of expenditure data, which is at the firm, or SystemID level. 95% confidence intervals shown.

Figure 6: Demographics around Private Equity Ownership Change



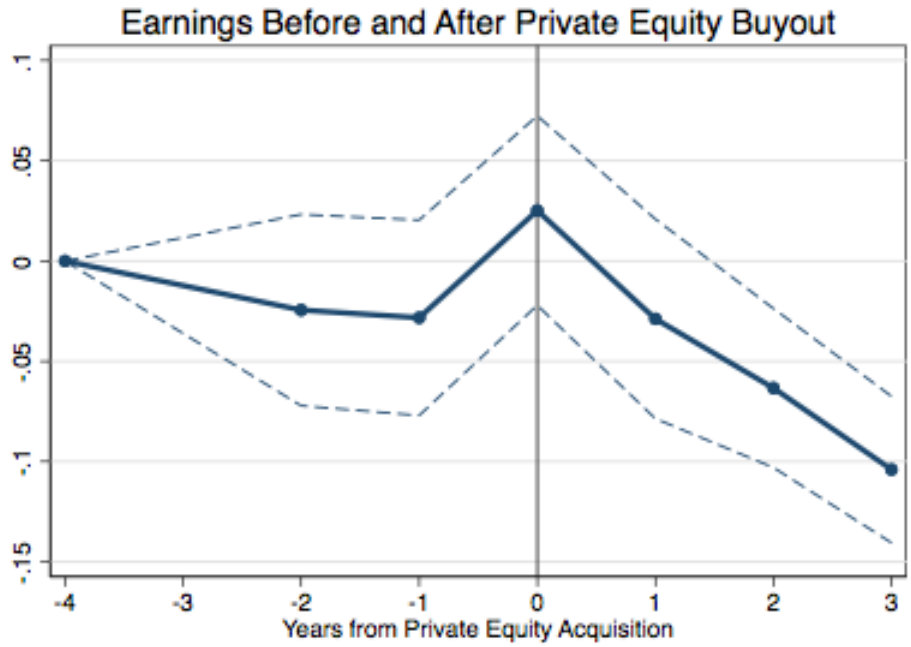
Note: The figures above show, for the sample of schools bought by PE, the means of demographic variables in the years around the ownership change. This data is at the school, or UnitID level. Federal grants are only available to low-income students, so increased rates of federal grants indicate a poorer population. 95% confidence intervals shown.

Figure 7: Student Outcomes around Private Equity Ownership Change



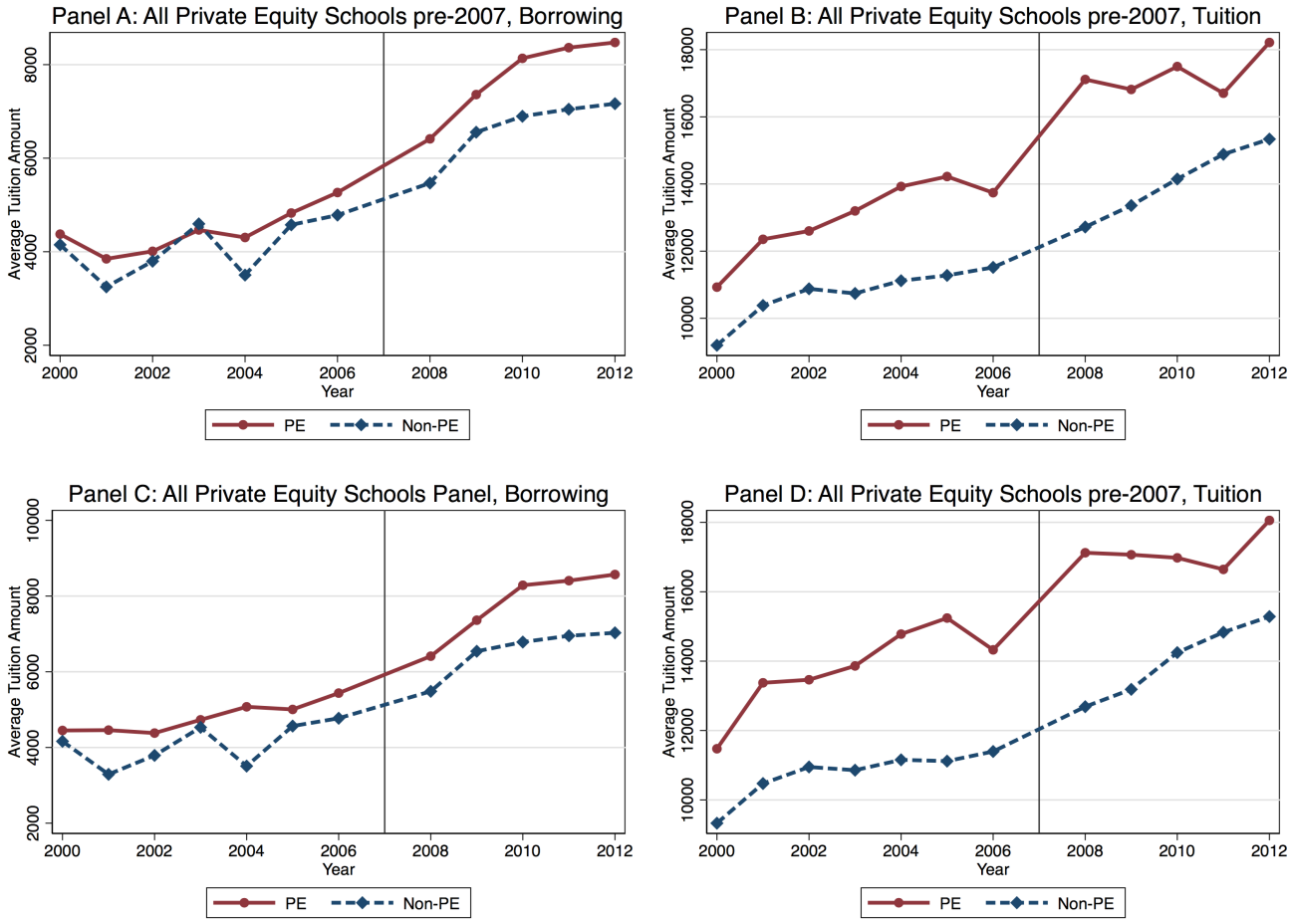
Note: The figures above show, for the sample of schools bought by private equity, the means of student outcome variables in the years around the ownership change. This data is at the school, or UnitID level. 95% confidence intervals shown.

Figure 8: Earnings around Private Equity Ownership Change



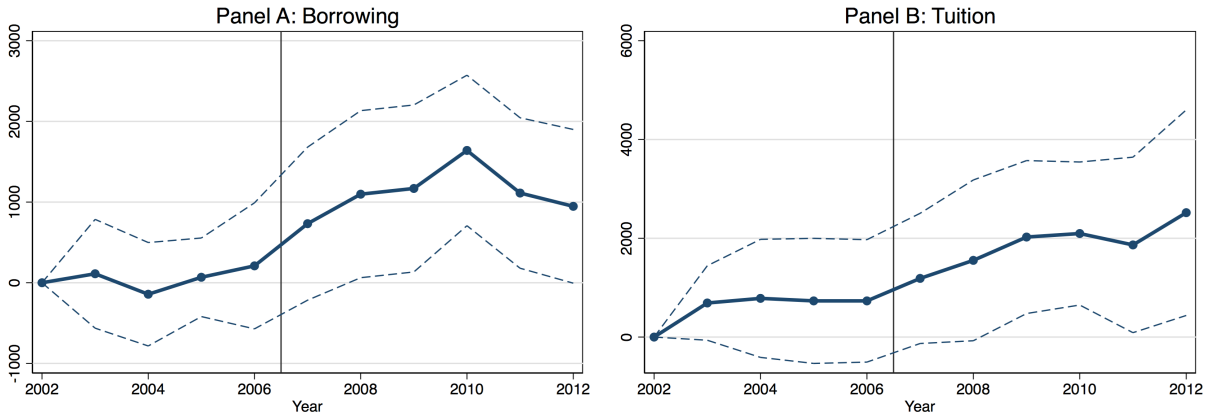
Note: The figures above show, for the sample of schools bought by private equity, the coefficient on an indicator of whether a time period if before or after a private equity ownership change. The dahsed lines denote a 95% confidence interval. This data is at the school, or UnitID level.

Figure 9: Borrowing and Tuition at Private Equity Institutions



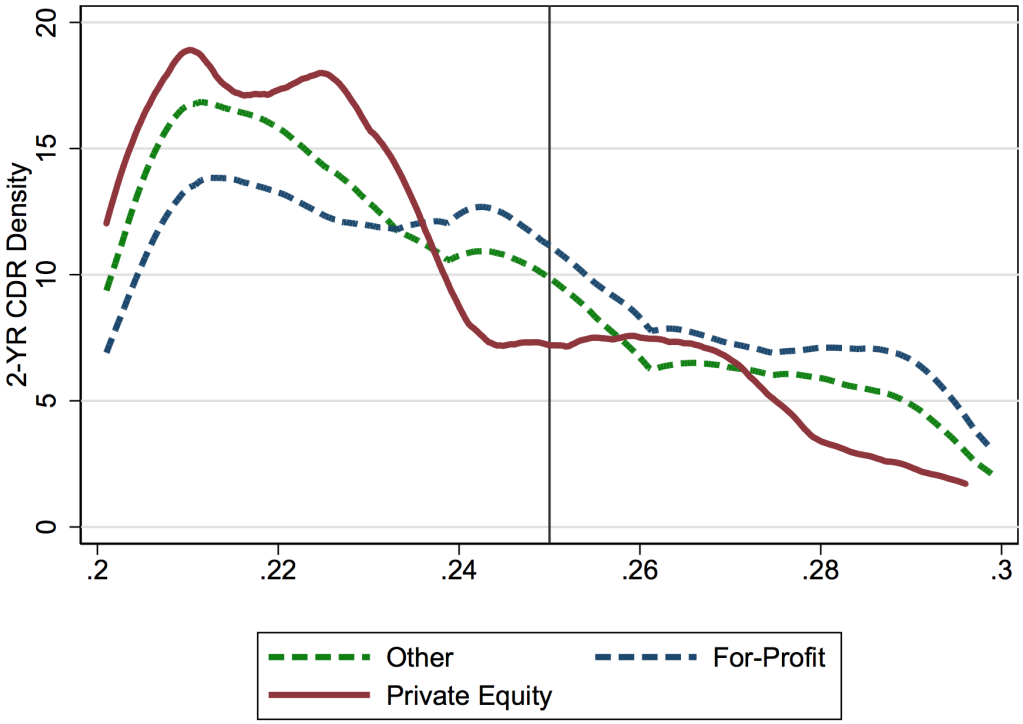
Note: The figure above shows average borrowing and Tuition in the for-profit sector, broken down by private equity owned schools and non-private equity owned schools. The panel on the right restricts private equity schools to those that were private equity owned pre-2007. The vertical line shows 2007, when student borrowing limits were increased.

Figure 10: Difference in Difference Coefficients Over Time



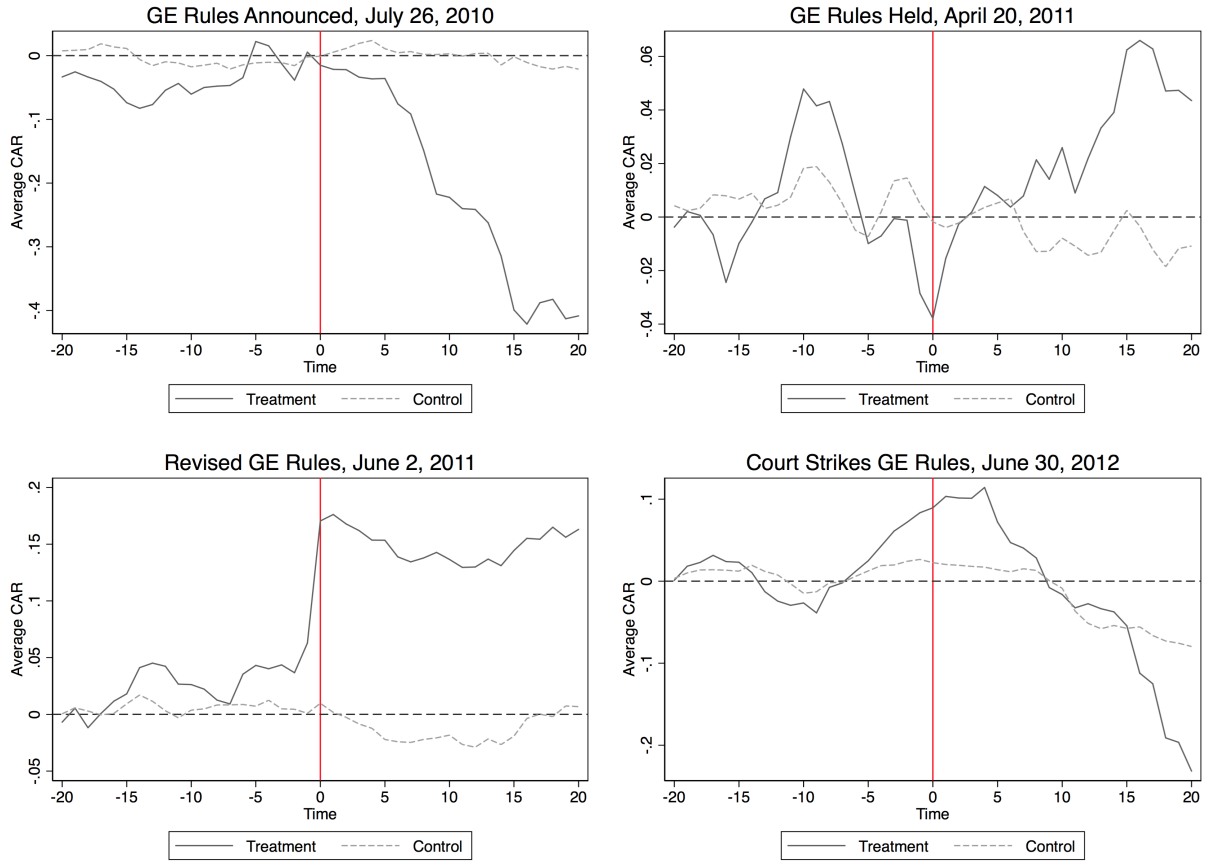
Note: The figure above shows coefficients β_j from the following specification $L_{it} = \alpha_i + \alpha_t + \sum_{j=2001}^{2015} \beta_j PE_i * 1[Year = j] + \gamma X_{it} + \varepsilon_{it}$. The dashed lines show 95% confidence intervals. Results are enrollment weighted. The vertical line shows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

Figure 11: Density of Cohort Default Rates by Institution Type



Note: This figure shows the density of two year cohort default rates, broken down by institution type.

Figure 12: Gainful Employment Rules and Cumulative Abnormal Returns



Note: The figure above shows cumulative abnormal returns for treatment and control schools. Average Cumulative Abnormal Returns for the stocks are calculated around 60-day event windows.

Appendix to "When Investor Incentives and Consumer Interests Diverge: Private Equity in Higher Education"

(for online publication)

Table A.1: Variable Descriptions

Variable name	Unit of Analysis	Years covered	Source	Description
		Panel 1: Operations		
Highest degree offered	UnitID	1987-2015	IPEDS	Indicator for whether the highest degree offered is a 4-year degree or higher, a 2-year degree, or a less-than-2-year certificate or degree.
Selective admissions	UnitID	1987-2015	IPEDS	An indicator for whether the school has any selective admissions requirements.
Number of students	UnitID	1987-2015	IPEDS	The number of fall semester fulltime equivalent students.*
Number of undergraduates	UnitID	1987-2015	IPEDS	The total number of fall semester undergraduate students, both full time and part time
Faculty per 100 students	UnitID	1987-2015	IPEDS	The number of fulltime faculty per 100 students published in the Delta Cost Project
1st law enforcement action	UnitID	1987-2015	Authors	Indicator for the school experiencing its first law enforcement action in year
		Panel 2: Demographics		
Share students black	UnitID	1987-2015	IPEDS	Share of fall semester undergraduates who are black.
Share students white	UnitID	1987-2015	IPEDS	Share of fall semester undergraduate who are white.
Total Pell grant revenue per student (mill 2015$)	UnitID	2000-2015	IPEDS	Total revenue from Pell grants awarded to fultime first-year students per fulltime first-year student
		Panel 3: Outcomes		
Graduation rate, all levels	UnitID	1995-2010	IPEDS	The graduation rate after 150 percent of normal time to degree.±
Average loan per borrower (2015$)	UnitID	2000-2015	IPEDS	Dollars borrowed per borrower among fulltime, first-year undergraduate student.
Cohort default rate (2 year)	OPEID	1990-2011	NSLDS	The default rate of the exiting cohort of borrowers 2 years after the cohort leaves school by either graduating or dropping out.
Loan repayment rate (3 year)	OPEID	2007-2011	NSLDS	The share of borrowers who have not defaulted and have repaid at least $1 dollar of principal on their loans 3 years after exiting school either by graduating or dropping out.
Wages 6 years after graduation	OPEID	1998-2007	College Score Card	Average income of exiting student cohort 6 years after the cohort leaves school by either graduating or dropping out.

Panel 4: Financials				
Profits	SystemID	1987-2015	IPEDS	Operating profits calculated as total revenue minus total education and operating costs[†]
Total revenue (mill 2015$)	SystemID	1987-2015	IPEDS	Total revenue
Total expenditure (mill 2015$)	SystemID	1987-2015	IPEDS	Total education and operating costs
Net tuition revenue (mill 2015$)	UnitID	1987-2015	IPEDS	Total revenue from tuition, including tuition paid for by federal and state grant aid programs.
Panel 5: Ownership and identifiers				
PE		1987-2015	Authors	Indicator for whether a parent company of a college or system was under private equity ownership at the beginning of the academic year.
Public		1987-2015	Authors	Indicator for whether a parent company of a college or system was publicly traded at the beginning of the academic year.[**]
UnitID		1987-2015	IPEDS	Unique identification number assigned to postsecondary institutions surveyed in IPEDS.
SystemID		1987-2015	Authors	A unique identifier created by the authors for the parent system of postsecondary institutions including parent companies of for-profit college chains.
OPEID		1990-2015	NSLDS	Reporting unit in the National Student Loan Data System [††]
Year		1987-2015	IPEDS	Year in which the spring term ends. For example, the 2001/2002 academic year is referred to as 2002.

Note: [†]Delta Cost Project variables: total03_revenue minus eandg01. [*]Each part time student is included in this count as a fraction of a full time based on IPEDS specificied formulas. [±]For 4-year, 2-year, and less-than-2-year degrees and certificates. We include this by year of the cohort's first enrollment. [**]This is not mutually exclusive from private equity ownership such as in cases where private equity owners take a company public or acquire substantial shares in a publicly traded company without taking it private. [††]OPEIDs commonly encompass more than one college owned by a for-profit parent company.

Table A.2: Private Equity Deal Data

Panel 1: Private equity deal data (PE firm deal level)

	N	Mean	Std. Dev.	Min	Median	Max
Total deals (first PE buyout or investment in school or chain)	88					
Bought controlling stake	88	0.78	0.41	0.00	1.00	1.00
Deal value (2016$)	35	38.9	64.4	0.38	10.6	311
Years to liquidity event, if exited	43	6.83	4.42	0.1	6.00	20.01

Panel 2: Private equity deal and exit types

Deal type		**Exit Type**	
Growth/Buyout	34	IPO	7
LBO	28	Sale to other PE firm(s)	22
VC	13	Sale to public company	8
Mezzanine	1	Sale to private company	3
Other/Unknown	10	School closed (failed)	10
		Still in portfolio (as of 10/2017)	27
		Exit status unknown	10

Panel 3: Top acquirers

Top PE firms (by deal frequency)		**School-Level Acquisitions**	
	N		N
Quad Partners	6	Total acquisitions/investments	205
TA Associates	4	Top PE-owned acquirers (by frequency)	
Significant Federation	5	Corinthian Colleges	36
Summit Partners	2	Education Affiliates	20
TL Ventures	2	Delta Career Education Systems	13
Primus	2	Lincoln Educational Services Corporation	11
Leeds Equity Advisors	2	National Business College	8
Liberty Partners	2	Forefront Education	8

Panel 4: Private equity firm data

	N	Mean	Std. Dev.	Min	Median	Max
Total firms (firms identified as participating in PE event)	118					
Firm age at investment	60	14.4	10.8	0	11.5	43
Firm has other education investment experience*	118	0.35	0.48	0	0	1
Number other education deals*	118	2.1	3.76	0	0	13
Median net multiple of firm's funds†	62	1.59	0.99	0.51	1.52	7.47
Median net multiple of firm's funds divided by fund type benchmark†	60	0.96	0.46	0.3	0.92	2.93
Median net IRR of firm's funds†	59	14.9	22.0	-27.2	14	167
Median net IRR of firm's funds less fund type benchmark†	59	1.53	22.0	-34.7	0.2	154.8
Modal quartile of firm's fund performance†	60	2.55	1.16	1	2	4

Note: *Source for education experience is Mitch Leventhal. †Source for return info is Preqin, so only firms matched to Preqin have returns data. The benchmarks are calculated by Preqin using their whole database, and are by fund type (e.g. VC, buyout).. Panel 5 shows the top private equity-backed acquirers of other schools. There are 205 instances of ownership change to private equity backing. The top acquirers, or private equity-owned school systems that bought new schools within the scope of the data, are summarized.

Table A.3: Law Enforcement Actions

Total law enforcement actions linked to IPEDS data		125	
Allegation		**Prosecuting Agency**	
Violated rules about recruiting/marketing*	44	State AG	56
Student loan fraud	35	DOJ	24
False Claims	31	DOE	23
Misrepresented job placement statistics	28	FBI	5
Misrepresented credentials/accreditation	23	FTC	4
Embezzlement	7	SEC	4
Fraudulent High School Diplomas	5	CFPB	3
Illegal Funds	4	Other	6
Real estate fraud	1		
	PE-owned	**Not PE-owned**	
Total school-year observations	13,137	309,242	
Number of instances in which school experienced its first law enforcement action	34	24	

Note: *For example, there are regulations limiting incentive compensation to sales force.

Table A.4: Nearest neighbor matching covariate balance

Panel 1: T-tests of covariates in year prior to PE event (all for-profits)

	Not PE backed in subsequent year			PE backed in subsequent year				
	N	**Mean**	**S.d.**	**N**	**Mean**	**S.d.**	**Diff**	**2-tailed p-value**
Students	41036	4.60	1.33	71	7.14	1.40	-2.54	0.00
Pell grants per student (2015$)	22186	3810	2805	67	2785	2106	1025	0.00
Share white	38692	0.55	0.33	68	0.49	0.23	0.06	0.15
Highest degree offered	93673	2.64	0.60	76	1.75	0.75	0.89	0.00
Public	94320	0.00	0.05	76	0.01	0.11	-0.01	0.04

Panel 2: T-tests of covariates in year prior to PE event after NN matching

	Not PE backed in subsequent year			PE backed in subsequent year				
	N	**Mean**	**S.d.**	**N**	**Mean**	**S.d.**	**Diff**	**2-tailed p-value**
Students	11712	4.91	1.34	53	7.37	1.28	-2.46	0.00
Pell grants per student (2015$)	11712	2639	1905	53	2136	1583	503	0.06
Share white	11712	0.53	0.33	53	0.48	0.22	0.05	0.23
Highest degree offered	11712	2.45	0.65	53	1.74	0.74	0.71	0.00
Public	11712	0.01	0.09	53	0.00	0.00	0.01	0.52

Note: This table shows t-tests before and after nearest-neighbor matching. Schools are also matched exactly on year (year prior to the private equity buyout), so this reduces the algorithm's ability to match well on the other variables.

Table A.5: Coarse exact matching reporting

Financials: Table 2		**Operations: Table 3 Panel 1**		**Operations, Table 3 Panel 2**	
Initial imbalance*	1.00	Initial imbalance*	0.99	Initial imbalance*	0.99
Imbalance after match	0.43	Imbalance after match	0.26	Imbalance after match	0.15
Matched PE:	10	Matched PE:	977	Matched PE:	784
Matched non private equity for-profits:	2500	Matched non PE for-profits:	38869	Matched non PE for-profits:	34558
Demographics, Table 4 columns 4, 8		**Demographics, Table 4 column 12, Table 5, Table 6 Panel 1**		**Borrowing, Table 6 Panel 2 column 5**	
Initial imbalance*	0.99	Initial imbalance*	0.99	Initial imbalance*	0.99
Imbalance after match	0.36	Imbalance after match	0.27	Imbalance after match	0.61
Matched PE:	1203	Matched PE:	926	Matched PE:	170
Matched non PE for-profits:	40760	Matched non PE for-profits:	35112	Matched non PE for-profits:	7126
Borrowing, Table 6 Panel 2 column 10					
Initial imbalance*	0.99				
Imbalance after match	0.53				
Matched PE:	187				
Matched non PE for-profits:	7476				

Note: This table reports the matching statistics from the coarse exact matching exercises. *Multivariate L1 distance. The estimator was developed by Iacus, King, and Porro [2012] and implemented in Stata with the cem package. After matching exactly on year (the year prior to the private equity event), we match on: number of students, the percent of students on federal grants, the share black, the share white, the highest degree type offered, whether the school is primarily online, and whether the school is selective. There are multiple matching exercises in this table because we do not match on the dependent variable, and because the datasets are different at the SystemID level (financials), OPEID level (CDR), and UnitID level (all others).

Table A.6: Effect omitting largest deals and their subsequent acquisitions

Panel 1

Dependent variable	Log profits (mill 2015$)			Log number students			Share students white	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PE owned	1.1***	1.2***	1***	.35***	.36***	.36***	-.034***	-.044***
	(.24)	(.24)	(.24)	(.059)	(.053)	(.062)	(.0093)	(.0078)
Public	1.1**	1.7***	1.1**	.25***	.32***	.26***	-.075***	-.11***
	(.42)	(.31)	(.41)	(.061)	(.083)	(.062)	(.015)	(.02)
PE owned·Public		-.98			-.17			.09***
		(.71)			(.13)			(.025)
Composition controls‡	N	N	Y	N	N	Y	N	N
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y	Y	Y
School f.e.	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y
N	80192	80192	80192	123571	123571	123571	123571	123571
R^2	.84	.84	.84	.97	.97	.97	.92	.92

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on on student borrowing, default, and repayment. ±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Observations are at the school (UnitID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Panel 2

Dependent variable	Share students in repayment 3 years after graduation			Log average wage (2015$)		
	(1)	(2)	(3)	(4)	(5)	(6)
PE owned	-.031*	-.03*	-.027*	-.032*	-.05***	-.027*
	(.011)	(.011)	(.011)	(.012)	(.01)	(.01)
Public	-.035**	-.026	-.031**	-.061**	-.086**	-.048*
	(.013)	(.014)	(.01)	(.017)	(.02)	(.02)
PE owned·Public		-.018			.083**	
		(.028)			(.018)	
Composition controls‡	N	N	Y	N	N	Y
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y
School f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
N	19783	19783	19783	16936	16936	16936
R^2	.96	.96	.96	.97	.97	.97

Note: This table shows regression estimates (OLS) and matching estimates of the effect of private equity ownership on student borrowing, default, and repayment.±The two matching estimators (nearest neighbor and coarse exact) are first matched on the year before the treated school's buyout, among for-profit schools, and then matched on characteristics as described in Section 4. In both matching estimators, the dependent variables are measured 2 years after the treated school's buyout. NNM yields an average treatment effect, and does not produce an R^2 or equivalent statistic. Observations are at the school (UnitID)-year level. Standard errors are two-way clustered by year and institution/firm (a firm may have multiple "schools", or campuses). ‡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. †Less than 2-year (certificate), 2-year, or 4-year. Coefficients marked with $*$, $**$,$***$, denote p < .1, p < .05, p < .01, respectively.

Table A.7: State & Year Fixed Effects (Sample: all for-profits)

Panel 1

Dependent variable:	Log Students	Log Undergraduates	Faculty per 100 students	Share students black	Share students white	Percent students on federal grants	Log total Pell grant revenue
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PE-backed	.61***	.57***	-.53***	.048***	-.064***	5.8***	.69***
	(.11)	(.1)	(.18)	(.014)	(.02)	(2)	(.11)
Number students				2.5e-06*	-9.7e-06***	-.00092***	.0003***
				(1.5e-06)	(3.7e-06)	(.00026)	(.000041)
Public	.51***	.5***	-1.5***	.018*	-.088***	-1.3	.49***
	(.14)	(.13)	(.19)	(.011)	(.021)	(2.1)	(.15)
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y	Y
State f.e.	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y
N	74994	73754	14862	72024	72024	45600	42945
R^2	.39	.39	.15	.3	.38	.5	.45

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. Observations are at the school-year level. Standard errors are clustered by institution/firm (a firm may have multiple "schools", or campuses). †Less than 2-year (certificate), 2-year, or 4-year. *** indicates p-value<.01.

Panel 2

Dependent variable:	Graduation rate, 2 yr*	Cohort default rate (2 year)†	Loan amount per borrower	Loan amount per first-time freshman	Federal grants per first-time fresh-man	First law enf event this year
	(1)	(2)	(3)	(4)	(5)	(6)
PE-backed	-.04**	.011*	822***	1042***	438	.0014
	(.02)	(.0063)	(244)	(256)	(383)	(.001)
Number students	-.000012***	1.8e-06**	-.021	-.088***	-.009	1.2e-06***
	(2.9e-06)	(9.1e-07)	(.028)	(.029)	(.033)	(3.5e-07)
Public	-.1***	.023***	804**	152	666	-.0015**
	(.028)	(.0069)	(354)	(218)	(504)	(.00071)
Highest degree offered f.e.†	Y	Y	Y	Y	Y	Y
State f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
N	16095	33457	41398	40122	42120	74994
R^2	.27	.23	.41	.4	.19	.0068

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. Observations are at the school-year level. Standard errors are clustered by institution/firm (a firm may have multiple "schools", or campuses). †Less than 2-year (certificate), 2-year, or 4-year. *** indicates p-value<.01.

Panel 3

Dependent variable:	Profits (mill nom $)	Total revenue (mill nom $)	Public grant revenue (mill nom $)	Net tuition revenue (mill nom $)	Total expenditure (mill nom $)
	(1)	(2)	(3)	(4)	(5)
PE-backed	.98***	.98***	1***	.68***	1***
	(.14)	(.12)	(.14)	(.14)	(.12)
Number students	.00027***	.00026***	.00023***	.00022***	.00025***
	(.000017)	(.000018)	(.000021)	(.000016)	(.000018)
Public	-1.2***	-1.1**	-1.4**	-.97***	-.97**
	(.4)	(.43)	(.55)	(.36)	(.43)
Highest degree offered f.e.†	Y	Y	Y	Y	Y
State f.e.	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y
N	21746	26547	24656	11146	24475
R^2	.35	.54	.41	.54	.54

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. Observations are at the school-year level. Standard errors are clustered by institution/firm (a firm may have multiple "schools", or campuses). †Less than 2-year (certificate), 2-year, or 4-year. *** indicates p-value<.01.

Table A.8: Effect by degree offered (test for obfuscation)

Panel 1

Dependent variable	Share students black		Share students white		Pell grants per student		Loan amount per borrower	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PE-backed	.023***	.022**	-.021*	-.045***	-85	150	511**	770***
	(.0072)	(.0083)	(.012)	(.016)	(186)	(311)	(203)	(156)
Public	.0031	.017*	-.063***	-.051**	56	235	-313	378
	(.0068)	(.0085)	(.014)	(.019)	(198)	(256)	(630)	(392)
School f.e.	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y
Sample:								
Highest degree < 2-year/certificate)	Y		Y		Y		Y	
Offers 2 or 4 year degree		Y		Y		Y		Y
N	.85	.87	.82	.85	.58	.57	.68	.77
R^2	0.839	0.848	0.808	0.827	0.542	0.512	0.641	0.734

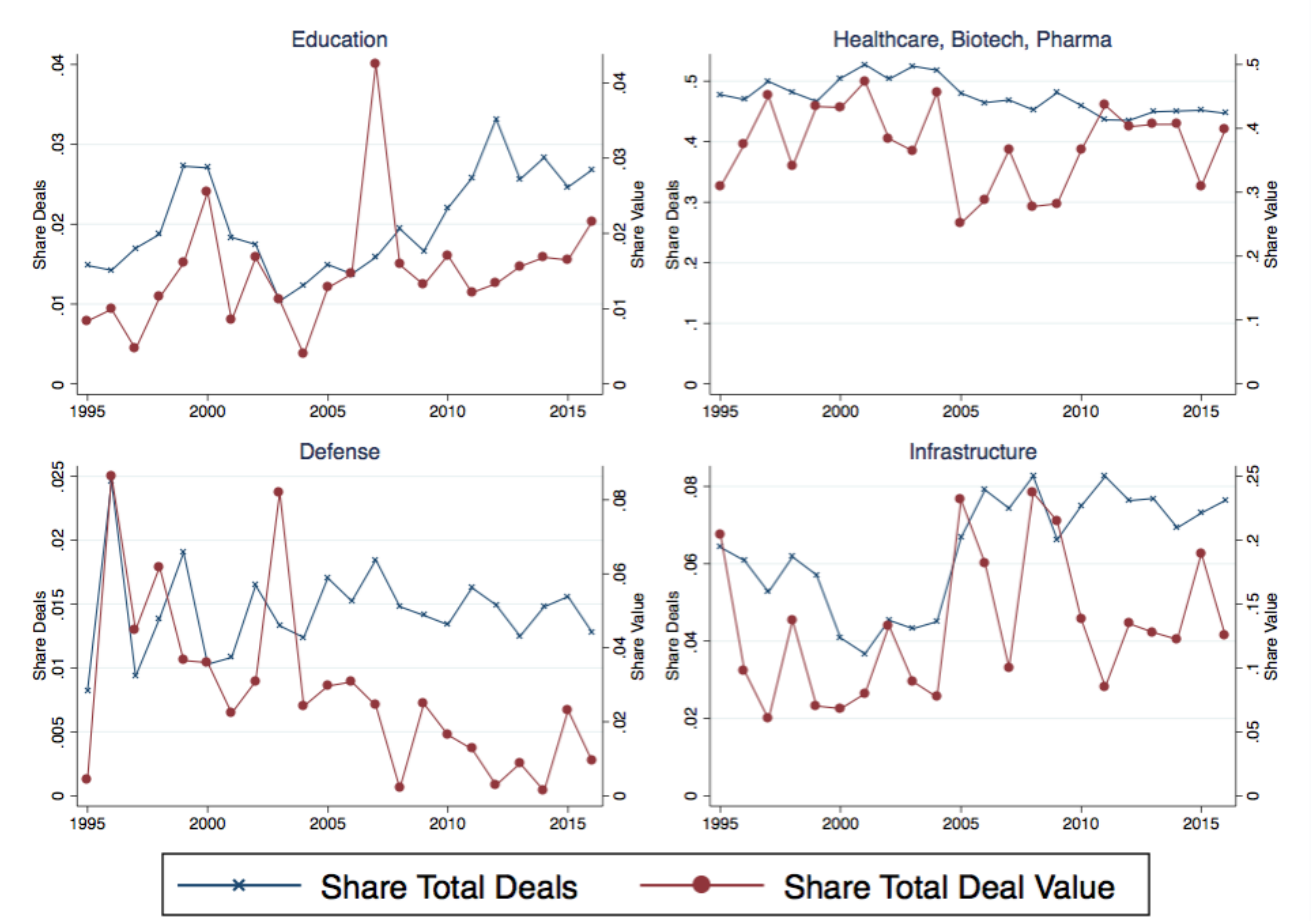
Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. Observations are at the school-year level. Standard errors are clustered by institution/firm (a firm may have multiple "schools", or campuses). ‡Number of FTE students included as control. †Less than 2-year (certificate), 2-year, or 4-year. *** indicates p-value<.01.

Panel 2

Dependent variable	CDR (2 year)		Share in repayment (3 yr)		Average log wages	
	(1)	(2)	(3)	(4)	(5)	(6)
PE-backed	-.011**	.0094	-.0035	-.031**	-.0028	-.031
	(.0046)	(.0069)	(.0098)	(.01)	(.013)	(.019)
Public	.012**	.0033	-.031*	-.015	-.029	-.03*
	(.0057)	(.0062)	(.012)	(.0083)	(.015)	(.013)
School f.e.	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
Sample:						
Highest degree < 2-year/certificate)	Y		Y		Y	
Offers 2 or 4 year degree		Y		Y		Y
N	.55	.62	.95	.92	.93	.9
R^2	0.513	0.582	0.929	0.896	0.906	0.877

Note: This table shows regression estimates (OLS) of the effect of private equity ownership on school outcomes. Observations are at the school-year level. Standard errors are clustered by institution/firm (a firm may have multiple "schools", or campuses). ‡Number of FTE students included as control. †Less than 2-year (certificate), 2-year, or 4-year. *** indicates p-value<.01.

Figure A.1: Share of private equity investment in government subsidy-intensive sectors as share of overall private equity investment, 1995-2016



Note: All global private equity transactions included. Total value was $716 billion in 2016, up from just $19 billion in 1995. Source: CIQ.