How Does For-profit College Attendance Affect Student Loans, Defaults and Earnings?*

Luis Armona
Stanford University

Rajashri Chakrabarti
Federal Reserve Bank of New York

Michael F. Lovenheim
Cornell University, NBER and CESifo

November 2017

Abstract

For-profit providers are becoming an increasingly important fixture of US higher education markets. Students who attend for-profit institutions take on more educational debt, have worse labor market outcomes, and are more likely to default than students attending similarly-selective public schools. Because for-profits tend to serve students from more disadvantaged backgrounds, it is important to isolate the causal effect of for-profit enrollment on educational and labor market outcomes. We approach this problem using a novel instrument combined with more comprehensive data on student outcomes than has been employed in prior research. Our instrument leverages the interaction between increases in the demand for college when labor demand declines and the local supply of for-profit schools. We compare enrollment and postsecondary outcome changes across areas that experience similar labor demand shocks but that have different latent supply of for-profit institutions. The first-stage estimates show that students are much more likely to enroll in a for-profit institution for a given labor demand change when there is a higher supply of such schools in the base period. Second-stage estimates vary somewhat across the two-year and four-year sectors. Among four-year students, for-profit enrollment leads to more loans, higher loan amounts, an increased likelihood of borrowing, an increased risk of default and worse labor market outcomes. Two-year for-profit students also take out more loans, have higher default rates and lower earnings. But, they are more likely to graduate and to earn over $25,000 per year (the median earnings of high school graduates). Finally, we show that negative local labor demand shocks induce for-profit entry and that this effect is larger in areas that have a higher latent supply of for-profit institutions. Our results point to low returns to for-profit enrollment that have important implications for public investments in higher education as well as how students make postsecondary choices.

KEYWORDS: Postsecondary Education, For-profits Schools, Student Loans, Default, Returns to Education

*We thank Julie Cullen, Enrico Moretti, Matthew Notowidigdo and seminar participants at the American Economic Association conference, Federal Reserve Bank of Cleveland, Louisiana State University, University of Illinois-Chicago, University of Utah, University of Connecticut, CESifo Conference on the Economics of Education, and the APPAM Annual Meeting for valuable comments. John Grigsby, Michelle Jiang and Michael Stewart provided excellent research assistance. The views expressed in this paper are those of the authors and do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System. All results, conclusions and errors are our own.

1 Introduction

One of the most dominant trends in the US postsecondary market over the past several decades has been the rise in for-profit institutions and enrollments. As of 2000, 450,000 students were enrolled in the for-profit postsecondary sector, which represented 2.9% of all higher education enrollment. By 2014, almost 1.6 million students, or 7.7% of all enrollments, were in a for-profit institution. The rise of for-profit enrollment over this period has been driven in part by a 69% increase in the number of for-profit schools, from 789 to 1334.[1] Over 296,000 AA or BA degrees were conferred by for-profit colleges and universities in 2014, which is 10.3% of all such degrees awarded in that year. With the precipitous rise in the for-profit postsecondary sector has come a growing concern that students are not well-served by attending these schools. They often charge higher tuition than their public counterparts of similar quality, and students have lower completion rates and longer time-to-degree than other similarly-selective institutions. Students attending these schools also tend to have much lower earnings post-attendance at a for-profit college relative to their non-profit and public counterparts. This pattern is associated with high student loan default rates of for-profit students: 39% percent of those who defaulted on their federal loans in the 2012 repayment cohort had attended for-profit colleges, while only 11.5% of students were enrolled at for-profit schools in the 2010-11 academic year (Chakrabarti, Lovenheim and Morris 2016a). Concerns about predatory behavior among for-profit postsecondary institutions led to heightened student loan regulations during the Obama administration, but the Trump administration has rolled back many of these rules (Stratford 2017). These policy changes underscore the importance of more fully understanding how for-profit enrollment affects student outcomes.

For-profit schools are much more likely to cater to students from lower-income backgrounds, first generation college attendees and under-represented minority students than are public and not-for-profit institutions (Chakrabarti, Lovenheim and Morris 2016b; Deming, Goldin and Katz 2012). Because they serve a larger proportion of students from disadvantaged backgrounds, the worse postsecondary and labor market outcomes associated with for-profit students are not necessarily indicative of a causal effect of these institutions. Identifying such causal

[1] These tabulations come from the 2016 *Digest of Education Statistics*.

effects is of high importance for two reasons. First, the federal government spends a large amount on financial aid for students attending for-profit colleges. About 20% of Pell grants and Stafford loans go to for-profit undergraduate students, totalling over $15 billion (Trends in Student Aid 2015).[2] Second, the return to enrolling in a particular postsecondary institution is a core component of the human capital investment decision (Becker 1962). With substantial information asymmetries in the higher education market, especially for students from low-income and minority backgrounds (Hoxby and Avery 2013; Hoxby and Turner 2013; Arcidiacono and Lovenheim 2016b), there is scope for students to enroll in colleges that ultimately do not benefit them because they do not know it is a bad investment at the time of enrollment. For-profits colleges typically do not practice selective admissions, and there usually are local, lower-priced nonselective public colleges or universities that students could attend instead (Deming, Goldin and Katz 2013). Whether students and taxpayers would be better served by shifting attendance from for-profit to these other similarly-selective local options currently is poorly understood.

In this paper, we estimate the effect of attending a for-profit college relative to a local public college or university on educational, financial aid, student debt and labor market outcomes using a novel identification strategy based on local labor demand shocks combined with the supply of postsecondary schools in a local area. Employing administrative institution-level data on enrollment, graduation, financial aid, student debt, default, and subsequent employment and earnings, we leverage the fact that higher education enrollment tends to increase when there is an adverse local labor demand shock (Betts and McFarland 1995; Christian 2007; Clark 2011; Hershbein 2012; Aguiar, Hurst and Karabarbounis 2013; Long 2014). This effect comes from increased enrollment among recent high school graduates who now face a lower opportunity cost of attending college as well as recently unemployed workers who are seeking to increase their skill levels.

We construct 3-year rolling labor demand shocks from 2000-2014 at the Core Based Statistical Area (CBSA) level using the shift-share labor demand instrument first introduced by Bartik (1991). The approach entails interacting a baseline employment share across industries with national trends in industry employment shares. We use industries defined by 2-digit NAICS

[2]This understates federal expenditures on for-profit students because it ignores veteran benefits provided by the post-9/11 GI Bill (Barr 2015).

codes as well as national trends outside of each CBSA's state to measure local labor demand conditions. Our main innovation comes from interacting this labor demand measure with for-profit penetration in the base year (2000)[3] in the CBSA. We thus use the fact that enrollment in for-profits should be higher in areas that have a higher baseline supply of such schools for a given labor demand shock. Critically, we are not identified off of baseline higher education supply conditions or off of differences across cities in the size of a labor demand shock. Rather, the instrument we use exploits *the interaction of these two forces*: we compare changes in outcomes among two otherwise similar CBSAs that experience similar labor demand shocks at the same time but that have different baseline postsecondary supply conditions. The main identification assumption we invoke is that there are no other contemporaneous shocks or trends that are correlated with the timing, magnitude, and sign of the labor demand shocks as well as with the initial supply of for-profit schools. That we have positive and negative demand shocks is a particular strength of our approach because it insulates us from biases associated with any uni-directional trends in outcomes that are correlated with for-profit supply. Furthermore, we demonstrate that for-profit supply and labor demand changes are only weakly correlated with each other and are uncorrelated with pre-2000 trends in our outcomes of interest, which makes it unlikely unobserved factors could bias our estimates.

We measure for-profit supply in a geography and sector (two- or four-year) by the percentage of for-profits schools in that geography and sector in 2000.[4] We find that the interaction of the percentage of a certain type (two- or four-year) of for-profit schools at the CBSA level in the 1999-2000 academic year interacted with the predicted labor demand measure is a strong instrument for for-profit enrollment in that sector. For a given negative labor demand shock, enrollment at 2-year for-profit schools increases by 105 students when the for-profit share in 2000 is 1 percentage point higher. Among four-year schools, the effect is 230 students. Both estimates are significantly different from zero at the 1% level. These estimates are of interest in their own right, as they demonstrate the importance of local postsecondary supply conditions in determining enrollment patterns following a recession and they suggest that for-profit schools played a significant role in many areas in worker retraining during the great recession. To our

[3]In this paper, we refer to academic years by the calendar year of the spring semester. Thus, 2000 refers to the 1999-2000 academic year.

[4]The two-year sector as we have defined it included less-than-two-year schools as well. For parsimony, we will call this group "two-year schools" throughout the paper.

knowledge, these patterns have not been shown previously, and they further underscore the policy importance of understanding how for-profit enrollment affects student outcomes.

In the second stage, our results point consistently to worse outcomes among for-profit students. In the two-year sector, estimates indicate that for-profit students take out more loans, originate higher loan amounts, and are more likely to default. While two-year for-profit students are more likely than their public counterparts to obtain a degree, they are less likely to be employed six years after leaving the institution and have lower earnings. However, they are more more likely to earn over $25,000 per year – the median earnings for a high school graduate – suggesting that these students may be better off than not having enrolled in college. Though many of these estimates are not precisely estimated, they suggest that for-profit two-year schools lead students to take on more debt without enhanced labor market outcomes relative to community college enrollment.

Among four-year students, there is a consistent negative effect of attending a for-profit. We find that for-profit students have a much higher propensity to take federal loans (both subsidized and unsubsidized). For-profit enrollment increases the likelihood that students take out subsidized federal loans by between 48 and 69 percent and unsubsidized loans by between 42 and 61 percent. Conditional on taking out any loans, for-profit enrollment increases the loan origination amount by $3,356 and increases the likelihood of default by 11 percent. A main reason why for-profit students are more likely to default is that they have worse labor market outcomes: they are less likely to be employed and have lower earnings than their public university counterparts. While the earnings estimates are somewhat imprecise, they (along with the estimates for employment) indicate that for-profit students have lower returns to their postsecondary investments than public four-year students.

Our core contribution to the literature is to provide estimates of the effect of for-profit attendance that more plausibly account for selection bias than prior work. We also examine a larger range of important outcomes than previous research, including graduation, employment, earnings, financial aid takeup, student loan originations and student loan defaults. Furthermore, we examine both four-year and two-year enrollment and outcomes. Finally, we are to our knowledge the first to examine how for-profit entry and exit responds to local labor demand

changes. Little currently is known about decisions of for-profit colleges to enter or exit a given market, and our estimates provide important insight into the role played by labor demand conditions. That we find for-profits are more likely to enter when labor demand is low (and thus demand for postsecondary enrollment is high) suggests these institutions are more demand responsive in their entry decisions than are traditional postsecondary schools.

This analysis relates most closely to the body of research that examines the return to for-profit enrollment. In general, this work is hampered by the difficulty in overcoming biases stemming from the selection of students into different school types. Deming, Goldin and Katz (2012) attempt to overcome this problem using selection-on-observables methods with the Beginning Postsecondary Students (BPS) dataset. They compare for-profit students to observationally-similar non-for-profit students and find large negative effects of the for-profits on employment outcomes. Using similar data and methods, Lang and Weinstein (2012, 2013) find similar if somewhat muted effects when comparing associates degree and certificate recipients at for profit versus other postsecondary sectors.

Another set of studies employs individual fixed effects to try and identify the returns to for-profit enrollment. This approach is likely to control more thoroughly for selection-on-unobservables than simply controlling for observed differences across students, but this method can be biased by selection into for-profit schools based on unobserved shocks or trends. In addition, the use of student fixed effects requires one to focus on older students who have sufficient pre-enrollment earnings. Cellini and Chaudhary (2008) are the first to use this method to examine the returns to for-profit enrollment. They estimate how earnings of those enrolling in for-profit two-year degree programs change relative to the change in earnings among high school graduates who do not enroll. The comparison group in this study therefore is non-attendees rather than those who attend another local public college as in our application. While both comparisons are of interest, understanding how the decision to enroll in a for-profit versus a non-for-profit affects outcomes is a core policy parameter that this analysis cannot inform. Cellini and Chaudhary find a post-enrollment increase in earnings of about 10% among for-profit attendees. Using a similar individual fixed effects strategy with US tax data, Turner (2011) and Cellini and Turner (2016) find that students enrolling in for-profits experience much

less growth in earnings than those enrolling in public institutions, and the latter analysis show that for-profit students experience a decline in earnings relative to their pre-enrollment levels.

The papers that most credibly overcome the selection biases associated with identifying the causal effect of for-profit enrollment are two recent randomized audit studies (Darolia et al. 2015; Deming et al. 2016). These studies send resumes to potential employers and randomly include a for-profit or a non-for-profit school as the education credential. Deming et al. (2016) find that listing a business or health degree from an online college (almost all of which are for-profit) significantly reduces the likelihood of callback relative to a nonselective public school. Darolia et al. (2016) find no adverse consequences of listing a for-profit degree. However, they focus only on sub-baccalaureate training in contrast to Deming et al. (2016). The differences in findings across these studies suggests it is important to consider four-year and two-year degrees separately, which we do in this analysis.

Taken together, the prior research on the return to for-profit enrollment is consistent with a negative effect relative to enrolling in another type of non-selective institution. However, the evidence based on secondary data relies on strong identifying assumptions. While the audit studies provide important insight into employer perceptions, one cannot necessarily translate these findings to labor market outcomes (Heckman 1998). Furthermore, it is important to examine a broader set of outcomes to try to understand the myriad ways in which the decision to enroll in a for-profit college affects educational and labor market outcomes. We therefore make several contributions to the literature. First, we examine a wide array of observed outcomes that have not been included in prior studies, including educational attainment, financial aid, probability of loan take-ups and loan volumes, and loan defaults. Second, we argue our empirical strategy more credibly overcomes concerns related to selection bias than has been possible in the papers discussed above that use secondary data. Third, the variation we use is of independent interest given the high policy concern surrounding worker retraining. We provide new evidence on how the local supply of postsecondary institutions affects enrollment decisions in an economic downturn, which in turn impacts returns to postsecondary investments. Our results thus inform worker training policies, and they have implications for the dynamics of how different areas recover from recessions. While our estimates are local to students who enroll in college because

of local labor demand variation, this is clearly a group of immense importance. This is the first analysis to examine the returns to for-profit enrollment decisions among such students.[5] Finally, to our knowledge, we are the first to study for-profit entry and exit in response to local labor demand shocks.[6] We find evidence that for-profit colleges respond to such shocks and are more likely to enter when labor demand is low, potentially to leverage the increased pool of students seeking enrollment.

2 Data

The data we use in this analysis come from five sources: Quarterly Census of Employment and Wages (QCEW), Integrated Post-secondary Education Data System (IPEDS), National Student Loan Data System (NSLDS), College Scorecard data (CSD), and the US Census. We discuss each of these datasets in turn below.

2.1 Measuring Labor Demand Shocks

We measure labor demand shocks using the shift-share approach pioneered by Bartik (1991). This entails combining baseline industry employment shares in a local area with changes in national employment shares in each industry outside of the local area. We use QCEW data from 1997-2014 that contain administrative employment information taken from establishments that report to state unemployment insurance systems. The QCEW covers about 97% of civilian workers across the US. Our main sample uses 2-digit North American Industry Classification System (NAICS) industry codes at the CBSA level to measure employment shares. We use 2-digit NAICS codes because the less-aggregated 3- and 4-digit codes have a high prevalence of missing values driven by nondisclosure and introduce much more error into the measure. Our labor demand measure is not terribly sensitive to using 3-digit or 4-digit NAICS codes, however.

[5] Jacobson, LaLonde and Sullivan (2005) show that community college enrollment increases future earnings among displaced workers. Jepsen, Troske and Coomes (2014) also find large but heterogeneous effects of community college degrees and certificates on earnings. They focus on somewhat older students who have pre-collegiate earnings, many of whom are likely to be displaced workers. Neither of these studies examines the differential returns to for-profit versus public community colleges, however, which is the main parameter of interest in this analysis.

[6] Gilpin, Saunders and Stoddard (2015) show that for-profit enrollment and completions among two-year students are responsive to local labor market conditions, but they do not examine institutional entry, per se, nor do they examine the four-year sector, which we show to be empirically important. Our results align qualitatively with theirs in showing that the for-profit sector is much more elastic than the public sector with respect to student demand.

Using the QCEW data, we construct predicted rolling 3-year labor demand ($\hat{\eta}$) shocks[7] for CBSA (c) in year (t) and state (s):[8]

$$\hat{\eta}_{ct} = \sum_{k=1}^{K} \gamma_{kc,t-3} \eta_{k\not{c}t}, \tag{1}$$

where γ is the employment share of industry k in baseline year $t-3$ and CBSA (c) and η is the percentage change in employment share of industry k between $t-3$ and t outside of CBSA c's state. Thus, we use national employment share changes outside of the CBSA's state in order to guard against the possibility that national changes are influenced by endogenous labor demand shifts within the CBSA. Note that rather than fix the employment share in a common year, the employment share changes across baseline years. This creates a stronger measure of actual labor demand changes over our long panel but is potentially concerning if changes in the base employment share are endogenous. We underscore that our focus is on the interaction of labor demand changes and a baseline higher education supply measure that does not change over time. As long as any endogeneity in the baseline share variation within a CBSA over time is uncorrelated with initial for-profit supply, our estimates will not be biased. A strength of our model is that we control for $\hat{\eta}_{ct}$ directly, which accounts for unobserved heterogeneity correlated with changes in baseline shares. Furthermore, in results available upon request, we find that our estimates are robust to using CBSA employment shares in 2000 for each outcome year.

Figure 1 shows the geographic variation in labor demand changes across CBSAs from 1997-2000. Just in this one year, there is a large amount of variation: some areas experience modest positive labor demand increases while others experience sizable contractions. There also is little spacial correlation among these changes that is evident from Figure 1. Within relatively concentrated geographic areas, one can find cities that are experiencing reductions and increases in labor demand. The sign and magnitude of these changes varies considerably within CBSA over time as well, which is shown in Figure 2. The figure shows the size of labor demand changes by quartile. While 50% of the distribution experiences small positive and negative shocks, the prevalence of quite large labor demand changes varies considerably. The vertical

[7] We have estimated our models using 1-year rolling labor demand shocks and find very similar results. These estimates are available from the authors upon request.

[8] Our notation follows Autor and Duggan (2003), who construct similar 3-year rolling labor demand averages at the state rather than at the CBSA level.

distance in each year shows the interquartile spread of 3-year demand shocks ending in that year. The recessions of the early 2000s and 2008 are clearly evident, but in each year there are areas that experienced large predicted increases and decreases in labor demand. There also are many areas in each year that experience positive and negative shocks as well as areas that experience little change in labor demand. Our identification strategy rests heavily on this large amount of variation in sign and magnitude of labor demand changes.

2.2 Measuring For-profit Supply, Student Outcomes and Demographic Characteristics

Our measure of for-profit supply is the proportion of postsecondary institutions in 2000 in each CBSA that are for-profit.[9] This is measured using 2000 IPEDS data and census crosswalks that help place public and for-profit institutions in CBSAs, and we construct a separate measure for two-year and four-year institutions. Throughout, we exclude all not-for-profit institutions. We do this for two reasons. First, the composition of the for-profit market is much more similar to the public market than to the not-for-profit market. Eighty-two percent of for-profit institutions are less than four-year, while 68% of public institutions are less than four-year. However, only 17% of private, not-for-profit colleges and universities are at the two- or less-than-two year level. Consequently, publics and for-profits are more similar in terms of degrees offered than are for-profit and not-for-profit colleges: 67% of degrees conferred by private, not-for-profit schools are graduate degrees and 28% are BAs. In the public sector, these percentages are 38 and 32, respectively, while in the for-profit sector they are 12 and 5%, respectively. These tabulations suggest that the private, for-profit sector is very different from the not-for-profit sector in ways that makes it unlikely students will be deciding between attending a for-profit versus a not-for profit institution. Rather, the decision set of most students considering a for-profit probably consists of public and for-profit institutions. Thus, we focus on these two sectors throughout this analysis.[10]

Figures 3 and 4 show the proportion of two-year and four-year institutions that are for-profit by CBSA in 2000. There is much heterogeneity across space. While there are many more

[9] A strong argument can be made for using enrollment percentages as our measure of for-profit supply instead. The goal of this measure is to capture the ability of the for-profit sector to absorb increases in student demand. Empirically, we find that the proportion of for-profit institutions captures this elasticity better than the proportion of enrolled students.

[10] In results not reported, we show our estimates are robust to including not-for-profit institutions.

CBSAs without a for-profit four-year school than without a 2-year school, for-profit schools are not clustered in any particular part of the country, and their prevalence varies even across nearby CBSAs. This suggests that there is much idiosyncratic variation in for-profit penetration that we can use for identification. Examining the distribution of for-profit supply shows there is heaping at 100% in the two-year sector. Sixty-five CBSAs have only for-profit two-year institutions; we exclude these CBSAs from the analysis because students in these areas do not have a public option and these CBSAs are unlikely to be similar to areas with both public and for-profit schools.

Enrollment data come from IPEDS. We focus on total 12-month enrollment, but we show below that our results and conclusions are robust to using 12-month undergraduate enrollment and various fall enrollment measures such as total enrollment, undergraduate enrollment, full-time equivalent enrollment, and full-time enrollment.[11] For graduation, we exploit data on degree recipients within 150% time (3 years for 2-year graduates and 6 years for 4-year graduates) at the institution-entering cohort level. That is, we measure the number of graduates in each year from cohorts 3 or 6 years ago. To construct this measure, we multiply the 150% graduation rate by the enrollment size of the cohort 3 or 6 years ago. The result is a cohort-based number of graduates.[12]

Data on financial aid at the institution-year level comes from the National Student Loan Data System (NSLDS). The NSLDS data are comprised of administrative information on all Title IV federal student loan originations. For each institution and year, we observe the total number of loans originated as well as four loan types: direct unsubsidized loans, direct subsidized loans, subsidized loans under the Federal Family Education Loan (FFEL) Program, and unsubsidized FFEL loans.[13] In addition to the number of originations, the data contain the value of all loans originated both overall and by type. These data are aggregated to the institution-year level, so while we have detailed financial aid outcomes for each college and university, we cannot link them to specific students. We supplement these data with information on three-year cohort

[11] Twelve month enrollment measures include all students enrolled during the 12-month reporting period (July 1 - June 30) for credit. Fall enrollment measures only include students enrolled for credit as of the institution's fall reporting date (or October 15) of the corresponding academic year. Since our outcome measures relate to students enrolled during the 12-month period (not just fall), the 12-month enrollment measures are the most relevant.

[12] We focus on 150% completion rate because 100% and 200% completion rates are only available for a small subset of our time period, while 150% graduation rate spans the entirety of our time period.

[13] Direct and FFEL loans are very similar. The main differences involve interest rates for PLUS loans as well as loan forgiveness options for public service.

default rates by institution and year from the US Department of Education. Cohorts in the default data are assigned based on when students first enter repayment, which typically occurs upon graduation or dropout.[14] The three-year cohort default rate is defined as the proportion of students entering repayment in a fiscal year who default by the end of the second subsequent fiscal year. For each institution, we observe the number of former students in default and the number of borrowers in repayment as well as the official cohort default rate.

The final dataset we use to measure student outcomes is the College Scorecard, which contains earnings and employment post-enrollment for all students who received Title IV federal aid while in college. These data come from US tax records and record earnings for each *incoming* cohort of students between 6 and 10 years after first enrolling. We observe the number of students from each cohort who are employed and not employed (including unemployed). Similar to graduation counts, we construct a cohort-specific measure of the number of former students employed by multiplying the cohort-specific employment rate by the enrollment size of the cohort. The CS data also contain mean earnings by institution, excluding those who are not employed and those who are enrolled in any postsecondary institution, including graduate school. We measure total earnings of each cohort (y) from each institution (j) by calculating the following: $\bar{W}_{yj} * \%emp_{yj} * E_{yj}$, where $\bar{W}$ is mean earnings, $\%emp$ is the cohort-institution employment rate, and E is enrollment of the cohort at the given institution. Finally, we calculate the number of students earning over \$25,000 per year in 2014 dollars as the fraction earning over \$25,000 multiplied by the enrollment level of the cohort.

Because our education data come at the institution level and because the labor demand shocks occur at the CBSA level, throughout the analysis we aggregate data to the CBSA-type-year level, separately for four-year and two-year institutions. Institution types are either for-profit or public. Thus, separately for two- and four-year institutions, for each CBSA and year there are two observations: one observation that aggregates all for-profit institutions and one that aggregates all public institutions.

To match labor demand shocks to enrollment, we assume that enrollment responds to an observed labor demand change. We therefore match 3-year rolling labor demand shocks to next

[14] If a student attends graduate school or enters a profession that qualifies for delayed loan repayment, she will not enter repayment upon leaving college.

year's enrollment (i.e., a one-year lead of enrollment).[15] For example, we link the labor demand shock from 2001-2004 (where 2001 is the base year, 2004 the end year) to enrollment in the 2005-06 academic year.[16] Most of the outcomes we examine are linked directly to incoming cohorts. Loan default rates are more complicated to merge with our analysis dataset because they are observed with a lead. Our data on default corresponds to cohorts in the "repayment" year, which corresponds to the year in which a student exits postsecondary enrollment because of graduation or dropout without a delayed payment exemption (such as public service or graduate school enrollment). To link default data to our enrollment data, we need to make assumptions about the length of time students spend in college. We assume students spend 100% of the statutory degree time in college, which means four-year students would be enrolled for 4 years and two-year students would be enrolled for 2-years. Our results are robust to assuming 150% time as well. The measurement error induced by our timing assumption is potentially problematic, but we believe it is most likely to bias our estimates towards zero to the extent we mismeasure the relationship between labor demand-induced entry and subsequent default outcomes. To create a serious bias in our results, it also would have to be the case that such measurement error is differential across the higher education sectors we consider. While possible, we know of no reason to believe this to be the case.

The final dataset we employ is from the US Census and contains year-by-CBSA demographic information. We obtain county-level estimates from the Census and use a county-CBSA crosswalk (also from the US Census) to aggregate up to the CBSA level. We construct measures of percent female, the racial composition of the CBSA (percent white, black, Hispanic, American Indian, Asian, and two or more races), the age distribution of the CBSA (percent 0-19, 20-29, 30-39, and 40 or over), the poverty rate, and total population.

Table 1 contains descriptive statistics of our main analysis variables, overall and by institutional level and control.[17] In both the four-year and two-year sectors, for-profit schools are much smaller than their public counterparts. Consistent with Figure 2, the mean labor demand change is about -1%, but there is much heterogeneity as illustrated by the relatively large stan-

[15] Online Appendix Table A-19 shows year ranges for all outcome variables we use in this study.

[16] Our QCEW data pertain to calendar years, while our education data pertain to academic years. So we assume that the shock in a calendar year (for example a shock that has 2004 as the end year) affects enrollment in the academic year starting the next fall (that is, the 2005-06 academic year).

[17] Online Appendix Table A-1 contains descriptive statistics separately for the periods 2000-2006 and 2008-2014 and Table A-18 shows tabulations for CBSA-level observables.

dard deviation. As expected, for-profit students are much more likely to take out loans and to default once one scales the number of borrowers and defaulters by enrollment. Earnings among two-year attendees are much lower than among four-year attendees, but the difference between for-profit and public attendees is larger in the two-year than in the four-year sector.

3 Empirical Approach

The core identification concern with estimating the returns to investing in different postsecondary options is omitted variable bias: students likely sort into different colleges and universities based on skills and preferences that are difficult to observe and that correlate with labor market outcomes. This problem has been the core focus of the returns to college quality literature (Brewer, Eide and Ehrenberg 1999; Black and Smith 2004, 2006; Dale and Krueger 2002; Hoekstra 2009; Andrews, Li and Lovenheim 2016). The majority of this research has examined highly- or moderately-selective schools where selection on unobserved dimensions of skill is likely to be a first-order concern. Selective postsecondary markets are highly geographically integrated (Hoxby 2009), which means that the decision to enroll in a given institution is not strongly linked to where students live. In contrast, most enrollment in non-selective postsecondary institutions is local: students not enrolling in selective postsecondary colleges and universities usually attend the local community college, for-profit school or non-selective four-year institution. This feature of the US higher education system has two implications. First, it creates market power among institutions in local markets in which the supply of colleges is low. Second, it creates considerable variation across local markets in the types of postsecondary institutions to which students have access. If a student is in an area with many local options that include public and for-profit schools at the two-year and four-year levels, they can exercise much more control over their postsecondary investment than a student who lives in an area without a four-year university or without for-profit institutions.

As a consequence of the uneven distribution of postsecondary institution types and quantities across CBSAs, higher education demand shocks should sort students into different types of schools depending on where they live. This is the underpinning of our empirical strategy. Specifically, we exploit the fact that people are more likely to enroll in college when labor

demand is low because of the reduced opportunity cost of enrollment in terms of foregone earnings. On the other hand, when labor demand is high and the opportunity cost of enrolling in college is high, people are more likely to join the labor force.[18] The thought experiment underlying our approach is to consider two cities that experience identical labor demand shocks (say, negative) in a given year but that have a different pre-existing supply of for-profit colleges. The negative labor demand shock increases the demand for college enrollment by the same amount in both cities. Enrollment in for-profit schools should increase more in the city with a higher supply of for-profit institutions when this common demand shock occurs, however. As long as students are responsive to local higher education supply conditions when making enrollment decisions, the interaction of labor demand shocks with pre-existing supply of for-profit institutions can be used as an instrument for for-profit enrollment.

Specifically, we estimate models of the following form using data aggregated to the CBSA (c), year (t) and for-profit/public (j) level:

$$\begin{aligned} E_{jcst} &= \alpha_0 + \alpha_1\hat{\eta}_{c,t-1} + \alpha_2(\hat{\eta}_{c,t-1} * Supply_c) + \alpha_3 FP_{jct} + \alpha_4(\hat{\eta}_{c,t-1} * FP_{jct}) + \alpha_5(Supply_c * FP_{jct}) \\ &\quad + \alpha_6(\hat{\eta}_{c,t-1} * Supply_c * FP_{jct}) + \alpha_7 L_{c,t-4} + \eta X_{ct} + \delta_c + \psi_{st} + \upsilon_{sj} + \zeta_{tj} + \mu_{jcst} \qquad (2) \end{aligned}$$

$$\begin{aligned} Y_{jcst} &= \beta_0 + \beta_1\hat{\eta}_{c,t-1} + \beta_2 FP_{jct} + \beta_3(\hat{\eta}_{c,t-1} * FP_{jct}) + \beta_4(Supply_c * FP_{jct}) \qquad (3) \\ &\quad + \beta_5\hat{E}_{jcst} + \beta_6\hat{E}_{jcst} * FP_{jct} + \beta_7 L_{c,t-4} + \eta X_{ct} + \phi_c + \theta_{st} + \omega_{sj} + \tau_{tj} + \epsilon_{jcst}, \end{aligned}$$

where equation (2) shows the first-stage and equation (3) shows the second stage. The variable E is total 12-month enrollment in institution type j in CBSA c in state s and year t, while Y is our outcomes of interest at the same level. The variable $\hat{\eta}$ is calculated using the formula shown in equation (1) and measures the labor demand change between time period $t-4$ and $t-1$ that we then link to the cohort entering college in time t. *Supply* is the proportion of postsecondary institutions in 2000 that are for-profit among all for-profit and public institutions and FP is an indicator variable equal to 1 if the observation is for the for-profit sector. The instruments in the first stage are the interaction between $\hat{\eta}$ and *Supply* as well as the interaction between $\hat{\eta}$,

[18] It is not obvious that enrollment will increase when labor demand declines because low labor demand can cause credit constraints to bind. Consistent with the prior literature (e.g., Christian (2007) and Hershbein (2012)), we show that any credit constraint effects are swamped by opportunity cost effects. As a result, enrollment in local postsecondary institutions increases when labor demand declines.

Supply and FP. The former shows how public enrollment increases for a given labor demand change as a function of the for-profit supply in 2000, and the latter shows a similar effect for the for-profits. The two endogenous independent variables in equation (3), $\hat{E}_{jcst}$ and $\hat{E}_{jcst} * FP_{jct}$, are instrumented with these variables.[19] The main coefficient of interest in equation (2) is α_6 because it shows how a given labor demand shock and pre-existing for-profit supply translate into for-profit enrollment. We focus on this parameter in the results below.

Equations (2) and (3) also include controls for CBSA fixed effects (δ,ϕ), state-by-year fixed effects (θ,ψ), state-by-for-profit fixed effects (υ,ω), year-by-for-profit fixed effects (ζ,τ), and lower order interactions among $\hat{\eta}$, *Supply* and FP. The state-by-year fixed effects account for any state-level economic shocks or policies that could be correlated with postsecondary choices or outcomes (such as higher education funding). State-by-for-profit and year-by-for-profit fixed effects control for the changing nature of for-profit institutions over time and across states. It could be the case that certain states have more or less productive for-profit institutions or that unobserved dimensions of for-profit productivity are changing over time, both of which will be accounted for by these fixed effects. We include as well a set of controls (X) that account for changes in the composition of CBSAs over time: percent female, the racial composition of the CBSA (percent black, Hispanic, American Indian, Asian, and two or more races), the age distribution of the CBSA (percent 20-29, 30-39, and 40 or over), the poverty rate, and total population. Finally, we control for base period ($t-4$) total employment (L) that helps account for any changes in the size of the workforce that could be correlated with labor demand changes.

We estimate equations (2) and (3) separately for four-year institutions and two- and less-than-two-year institutions;[20] *Supply* is calculated separately for each institution level. Because there is likely to be serial correlation in the errors within CBSAs over time, we cluster all standard errors at the CBSA level.[21]

The coefficients of interest in the second stage are β_5 and β_6. Importantly, all lower-level

[19] There are two first-stage equations, with E_{jcst} and $E_{jcst} * FP_{jct}$ as the dependent variables. We focus on the first-stage estimates with $Ejcst$ as the outcome variable below because it is more straightforward to interpret. Estimates using $E_{jcst} * FP_{jct}$ are available upon request from the authors.

[20] About 5% of institutions switch from 2-year to 4-year over our sample period. Online Appendix Table A-13 shows results fixing the institutional categorization in the base year. Results are very similar to our preferred results shown below that allow schools to switch 2-year/4-year status.

[21] The four-year estimates include selective and nonselective schools. This is potentially problematic if selective institutions are not substitutes for for-profit schools or if selective universities have geographically dispersed enrollment that comes from outside the local area. In Appendix Tables A-3 through A-6, we show estimates that exclude institutions that have an admissions selectivity rating of Very Competitive or higher in the 2001 Barrons rankings. The first-stage and second-stage estimates are very similar in magnitude and statistical significance to baseline.

interactions between $\hat{\eta}$, $Supply$ and FP are included in the second stage, except for $\hat{\eta} * Supply$, which is another excluded instrument. These controls allow us to account for independent effects of labor demand changes (β_1), fixed differences between for-profit and public institutions (β_2), differential effects of labor demand shocks on for-profit versus public institutions (β_3), and differential changes in the for-profit sector as a function of 2000 supply (β_4). Furthermore, controlling for $\hat{\eta}$ accounts for changes in the industrial share over time within CBSA.[22] Conditional on these controls, the main identifying assumption underlying this model is that there are no differential trends or shocks that are correlated with the timing, magnitude and sign of the labor demand shocks and that differentially impact for-profit schools in places where the pre-existing supply of for-profit schools is higher. That is, the main threat to identification comes from secular shocks that mimic the timing, sign and magnitude of labor demand changes and that also are correlated with $Supply$.

A particular strength of our approach is that our estimates are much less sensitive to bias from secular trends than is the case in a typical panel data analysis because our labor demand measure varies in sign within CBSA. Fifty-nine percent of CBSAs in our data experience both a positive and negative 3-year labor demand change between 2000 and 2014. Among the areas that have a negative labor demand shock in 2000, 46% have a positive shock at some point in the future. Among CBSAs with a positive shock in 2000, 13% experience a negative shock over the next 14 years. Thus, secular trends in outcomes in one direction should not impact our results unless the secular trends switch signs with the labor demand changes. The use of rolling labor demand shocks, however, introduces serial correlation in the $\hat{\eta}$ measures across years. In the short run, the serial correlation could be related to secular trends in outcomes within a CBSA. To account for this concern, we show below that our results are robust to controlling for several leads of predicted labor demand shocks.

The validity of our approach is further bolstered by the weak correlation between the predicted labor demand changes and 2000 for-profit supply: the correlation is only 0.17 in our sample. Figures 5 and 6 provide graphical evidence of this result. Figure 5 shows the correlation between 2000 for-profit share and CBSA-year labor demand changes that are residual to

[22] Because we use rolling labor demand changes, the baseline industrial share changes in each year. Without controlling for these shares, some of the identifying variation comes from changes in the industrial composition within CBSAs over time. This variation is accounted for by controlling for $\hat{\eta}$. We also have estimated models in which we fix the baseline share at 2000 levels. These estimates are very similar to baseline and are available upon request.

the CBSA-specific mean. Both in the four-year and two-year sectors, there is no evidence that labor demand changes are related to the relative size of the for-profit market in CBSAs.[23] Figure 6 presents trends of CBSA-demeaned labor demand shocks by for-profit supply. The top panel shows trends among four-year institutions and the bottom panel shows trends among two-year colleges. We present trends in predicted labor demand among those CBSAs with no for-profits and then with above and below median for-profit supply among areas with any for-profits. It is clear that the three groups exhibit near identical trends in labor demand changes over time. In Online Appendix Figure A-1, we show that the trends aggregated across school types also are similar. Taken together, these figures strongly suggest that labor demand changes within CBSAs are at most weakly correlated with 2000 for-profit supply heterogeneity. It therefore is very unlikely there are unobserved shocks that are correlated with both variables, which supports our identification strategy.

Further evidence that our estimates are not biased by secular trends comes from comparisons of pre-2000 trends in outcomes across the distribution of for-profit supply and labor demand changes. Figure 7 presents trends in enrollment and outcome variables from 1996-1998 for CBSAs that are above and below the median interaction of 2000 supply and $\hat{\eta}$. The panels of the figures thus test whether the value of the instrument in 2000 is correlated with pre-2000 outcome trends. The patterns are virtually identical across CBSAs with different instrument values in 2000, suggesting we are not picking up differential secular trends in our estimates. Online Appendix Figures A-2 and A-3 show similar trends by for-profit supply. There is no evidence that outcomes in areas with different for-profit penetration are changing differently prior to 2000. Importantly, pre-2000 trends in predicted labor demand changes are also similar across the for-profit supply distribution. Additionally, Figures A-4 and A-5 show that pre-2000 outcome trends are similar across CBSAs with above and below median labor demand shocks in 2000 as well as for those with positive versus negative labor demand shocks in 2000. Finally, Appendix Figure A-6 presents pre-trend estimates by whether CBSAs had a positive or negative value of the interaction between *Supply* and $\hat{\eta}$ in 2000. These results again show no evidence of differential trends. The lack of pre-2000 trends in outcomes as a function of for-profit supply

[23] As discussed in the previous section, we exclude CBSAs that only have for-profit two-year schools. We leave them in Figure 5 in order to show the full for-profit supply distribution, however.

and labor demand changes provides much support for our identification strategy, as any secular variation in outcomes needs to be correlated with both $\hat{\eta}$ and for-profit supply to cause a bias in our estimation of β_5 and β_6.

Because we aggregate data to the CBSA-institution type-year level for this analysis, another identification threat comes from potential changes in the composition of students at different school types induced by the identifying variation. For example, if labor demand declines cause more disadvantaged students to enroll in for-profit institutions when there is higher for-profit supply, our estimates may be picking up this compositional shift rather than the effect of for-profit enrollment on student outcomes. In this case, our estimates will reflect how for-profit outcomes change in areas with higher for-profit supply when there is a local demand increase driven by a labor demand decline, but they will not tell us whether these differential outcomes reflect productivity differences across the for-profit and public sectors.

In order to determine the extent to which student composition changes endogenously in a manner that may affect our results, we estimate a version of equation (2) in which the dependent variables are aggregate characteristics of students. The resulting estimates, presented in Table 2, show how much for-profit student characteristics relative to public student characteristics change across the distribution of for-profit supply when there is a labor demand shock. We examine a wide range of outcomes: student racial/ethnic backgrounds, age, financial aid receipt, the number of colleges to which students sent a FAFSA, parental education, parental income, the academic achievement of the student body, and the composition and types of degrees awarded.[24] Only one of the 46 estimates in the table is statistically significantly different from zero at the 1% level and two more are significant at the 10% level. These proportions of significant coefficients are what one would expect from random error if all effects were zero. Furthermore, all estimates are small in magnitude, suggesting that our instrument does not generate a relative shift in the composition of students across institution types. Our results are consistent with students sorting into different institutions in the same way where for-profit supply is high versus low, given a labor demand change. That is, students do not sort differently into for-profit institutions when there is a negative labor demand shock in areas that have a higher pre-existing supply of for-profits. The results in Table 2 from the extensive set of observed characteristics in our data

[24]Descriptive statistics of these variables are shown in Online Appendix Table A-2.

make it unlikely that our results are being driven by compositional shifts of students.

The null effects for changes in the types of degrees and majors in Table 2 are particularly important for interpreting our results below. For-profit and public postsecondary institutions can offer different degrees and can have different academic foci. This is not a problem for identification, insofar as part of the causal effect of attending a given school is to obtain training in an area in which the school specializes. That we do not see changes in broad categories of majors or degree types, however, suggests our results are not being affected by changes in the types of academic credentials students earn. Rather, we argue they reflect differences in the return to enrolling in a for-profit versus a public institution, holding fixed the type academic credential being earned.

4 Results

4.1 First Stage Estimates

First-stage estimates for two-year and four-year institutions are shown in Table 3. In the interest of space, we show only the coefficients on the instruments: α_6, which corresponds to the variable $\hat{\eta} * Supply * FP$ and α_2, which corresponds to the variable $\hat{\eta} * Supply$. Odd columns contain results that include year fixed effects, and even columns include state-by-year fixed effects.[25] Panels A and B show results for two-year and less-than-two-year institutions and are distinguished by the inclusion of state-by-for-profit and year-by-for-profit fixed effects in Panel B. Focusing on the estimate on $\hat{\eta} * Supply * FP$ in the 2000-2014 period, which is the main coefficient of interest, a 1 percentage point decline in labor demand increases enrollment in for-profits by 104.5 students when the proportion of enrollment in for-profits is 1 percentage point higher in 2000. The results change little when state-year, state-for-profit and year-for-profit fixed effects are added to the regression. Estimates in all columns are significant at the 1% level, and the P-value of the test that the coefficients on the instruments are zero are shown at the bottom of each panel. In all columns and panels, the null is rejected at very high levels of significance, indicating that our instruments are sufficiently strong. These results are interesting in their own right because they suggest both that postsecondary enrollment is strongly related

[25] Recall that all estimates include CBSA fixed effects, which subsume state fixed effects in the odd columns.

to local labor demand changes and that the types of institutions in which students enroll because of reduced labor demand is influenced by the latent supply of schools.[26] When there are more for-profit institutions, students are more likely to sort into those schools when a recession hits the local area. The estimates in Table 3 therefore have important implications for where students are receiving their training (or re-training) during recessions. And, to the extent that there are causal effects of school choice on student outcomes, it suggests that latent supply of postsecondary institutions can affect the dynamics of how local areas recover from recessions.

In the subsequent columns of Table 3, we examine estimates from 2000-2006 and from 2008-2014. The first time period corresponds to relatively strong labor demand, while the later period contains the great recession. If we are simply picking up secular trends, we might expect the estimates in these two periods to differ in magnitude and sign. While the effect is larger in the 2000-2006 time period, enrollment responsiveness to labor demand shocks as a function of for-profit supply is economically and statistically significant during and after the great recession.

Panels C and D of Table 3 present similar estimates for the four-year sector. Although the levels differ from the two-year estimates because four-year institutions are larger, the patterns are almost identical. For a 1 percentage point labor demand reduction, enrollment increases by 230 students in the for-profit sector for every 1 percentage point increase in baseline for-profit supply. These estimates change little with the inclusion of state-by-year, state-by-for-profit and year-by-for-profit fixed effects, and the instrument are sufficiently powered in both time periods. Unlike the two-year sector, the effect is larger in the 2008-2014 period than in the 2000-2006 period. This difference may be driven in part by the relatively large growth in four-year for-profits during the later period. The estimates indicate that students are highly responsive to labor demand changes and to higher education supply in their postsecondary enrollment choices in both periods.

Also of interest in Table 2 is the coefficient on $\hat{\eta} * Supply$, corresponding to α_2 in equation (2). These estimates show that public enrollment declines for a given labor demand shock when the for-profit share is higher, although the effect is much weaker in the 4-year sector.

[26] These results are consistent with prior research showing that students are very willing to substitute across the public and for-profit sectors. Goodman and Henriques (2016) demonstrate that reductions in state appropriations induce students to substitute from the public to the for-profit sector. Cellini, Darolia and Turner (2016) find that when for-profits lost the ability to disperse Title IV financial aid, students substituted to the public sector such that overall enrollment rates did not decline. The results in Table 3 also indicate that there is a good deal of substitution across the public and for-profit sectors and that the degree of substitution is related to the higher education supply conditions.

This occurs because students are more likely to sort into for-profit schools due to the labor demand decline. Across columns in Table 3, all estimates are positive and are statistically significant at the 1% level in the two-year sector. We consistently find that $|\alpha_2| < |\alpha_6|$, which means that overall postsecondary enrollment grows when there is a negative demand shock in areas that have higher baseline for-profit supply. This result is consistent with for-profits reducing capacity constraints in local postsecondary markets. That higher for-profit supply leads to higher overall enrollment when there is a negative labor demand shock underscores concerns that the composition of students also is changing. However, in Table 2 we show this is not the case: students sort in the same manner into for-profit and public institutions due to demand shocks across the distribution of 2000 for-profit supply.

One objection to the four-year estimates in Table 3 is the inclusion of selective colleges and universities. These institutions do not take most of their enrollments from local areas, and they are unlikely to be in serious competition from the for-profit sector. In Appendix Table A-3, we show first-stage estimates that exclude any colleges and universities that have a rating of "Very Competitive" or higher in the 2001 Barrons rankings.[27] For two-year schools, the estimates are almost identical to those in Table 3 as expected.[28] The four-year university estimates are similar in magnitude to the results in Table 3, but they are less precisely estimated. Thus, the selective schools increase statistical power, but they do not drive our enrollment results.

Our baseline estimates in Table 3 use total 12-month enrollment. There are several enrollment measures we could use instead, including 12 month undergraduate enrollment and various fall enrollment measures such as undergraduate enrollment, full-time enrollment and first-time enrollment. Appendix Table A-7 presents results corresponding to Table 3, where we use 12-month undergraduate enrollment as the enrollment measure instead of total 12-month enrollment. As the results in Table A-7 show, the results are qualitatively similar to those in Table 3. As may be expected, the estimates of α_6 in Table A-7 are economically smaller in the four-year sector (as they only include undergraduate enrollment rather than total enrollment) and are virtually identical in the two-year sector. These are always statistically significant at

[27] This restricts our sample to schools with an admission rate of at least 75%. Deming, Lovenheim and Patterson (2016) argue that these are the institutions most likely to be in competition with the for-profit sector, and the students from these schools come predominantly from local areas.

[28] They are not completely identical because a small number of two-year institutions switch to four-year institutions and begin practicing selective admissions during our sample period.

1% level and are also always highly powered. Figure 8 shows first-stage estimates of α_6 from equation (2) using different enrollment measures. Although the estimates differ somewhat in magnitude due to different baseline levels, all estimates are economically meaningful in magnitude and are statistically significant at the 1% level both for the two-year and the four-year sector. Figure 8 demonstrates that our results are not sensitive to the specific way in which we measure student enrollment.[29]

4.2 Second Stage Estimates

Tables 4-6 present second-stage estimates of β_5 and β_6 from equation (3) for our outcomes of interest. We present results separately including year (columns 1-2 and 5-6) and state-by-year (columns 3-4 and 7-8) fixed effects. Even columns include state-by-for-profit and year-by-for-profit fixed effects. Estimates are shown separately for two-year and four-year schools. In general, we find weaker evidence of the effects of two-year for-profit enrollment on educational and labor market outcomes. With some exceptions, which we note below, the estimates are not statistically different from zero. We therefore focus on the four-year results.

Table 4 presents second stage estimates where the outcome variable is the number of loan recipients by student loan type. The first four columns show results for two-year schools and the second four columns show four-year results. The main coefficient of focus is the effect of for-profit enrollment. This table examines whether for-profit students are more likely to take out the various type of student loans. The extent to which students are taking out subsidized versus unsubsidized loans is important in understanding the social cost of different enrollment choices. Four-year for-profit enrollment leads to a markedly higher likelihood of originating each type of student loan. Panel A shows that a marginal for-profit enrollee is 61-69 percent more likely to take out a direct subsidized loan than her public counterpart. Panels B, C, and D, respectively, show that for-profit enrollment increases the propensity of originating a direct unsubsidized loan, a FFEL subsidized loan and FFEL unsubsidized loan by 61 percent, 48 percent and 43 percent respectively. Examining estimates across columns shows the estimates are robust to the addition of various fixed effects we include in the regressions. Overall, Table 4 shows that for-profit students rely much more heavily on federal loans (including subsidized

[29]Estimates of equation (2) using these various enrollment measures are available from the authors upon request.

loans): a marginal for-profit enrollee has more than a 95% probability of originating a direct subsidized loan, whereas her public counterpart has only a 33-35% probability of originating such a loan.

Table 5 presents results for student loans and defaults. Panel A includes estimates for total number of loans originated. Among four-year students, there is a consistent positive effect across columns that is significant at the 1% level: an additional for-profit enrollee originates 1.1 more loans than her counterpart in a public college or university. Estimates among two-year students are similar in magnitude though less precise. Relative to the number of loans taken out by public two-year students, these increases are substantial. Panel B shows estimates for total loan origination amounts, which allows us to examine whether students are taking out larger loans when they attend for-profits. Across both two-year and four-year institutions, there is clear evidence that this is the case: a marginal two-year for-profit student originates $6,428 more in student loans than her public sector counterpart. The last two columns of Panel B show that a marginal for-profit enrollee in the four-year sector originates between $3,356 and $3,847 more in student loans than her public counterpart. Thus, not only are for-profit students taking on more loans, but they are taking on far more student debt than comparable public college and university students among the loans they originate. As a result, total student debt among these students is much higher.

Does this added debt burden increase the likelihood of default? Panel C shows effects on student loan defaults assuming 100% time in college. Among four-year students, the results indicate that for-profit enrollment increases the likelihood of default by about 11 percentage points. These estimates are statistically different from zero at the 1 percent level and indicate that the increased loan burden associated with for-profit universities leads to a substantially higher risk of default. In the two-year sector, the estimates are even larger, at 21-34 percent. The effects are significant at the 10% level when only using year fixed effects but become very imprecise when state-year fixed effects are included in the model. However, these estimates are suggestive of much higher default among two-year for-profit students.

One reason default rates are higher among for-profit students may be that they are less likely to graduate and have worse labor market outcomes. Table 6 reports results for these outcomes;

for brevity we only show estimates that include state-by-year fixed effects. In Panel A, we show that four-year for-profit students are somewhat less likely to graduate than their public counterparts, although the estimates are not statistically different from zero at conventional levels. Among two-year students, there is evidence that for-profit schools are more adept at graduating students: an additional enrollment is associated with 0.28 additional students graduating. While not statistically significant, these results suggest that enrolling in a two-year for-profit institution increases the likelihood of degree receipt substantially.

Looking past postsecondary outcomes, the remaining panels of Table 6 examine the labor market return to for-profit enrollment relative to public enrollment. Recall from Section 2.2 that the labor market outcomes are only for those who received federal financial aid. In Table 2, we do not find a consistent relationship between the instrument and the percentage receiving federal aid, which suggests there is no differential selection into the labor market sample that would bias our estimates. Nonetheless, it is important to highlight that the labor market results hold only for federal aid recipients.

Panel B of Table 6 shows that four-year for-profit enrollment reduces the likelihood of employment by 9-11 percent while two-year for-profit enrollment reduces the likelihood of employment by 36 percent. However, the two-year estimates are not statistically significant. Panel C shows that earnings are lower for four-year for-profit students as well, but the estimates are quite imprecise. A marginal four-year for-profit student earns about $6,000 less than her public counterpart six years after enrolling. In the two-year sector, the earnings penalty for for-profit enrollment is about $9,000, which is substantial. In both sectors, earnings reductions are large relative to the *Enroll* estimates, which can be interpreted as the mean earnings among public postsecondary students. Estimates in Panel D show less consistent results across sectors for the likelihood of earning more than $25,000 per year. The threshold of $25,000 is of interest as it corresponds approximately to the median wage of workers ages 25 to 34 with only a high-school degree. Examining this outcome helps gauge whether and by how much a student is better off from having decided to enroll in a for-profit (or public) college rather than not invest in a postsecondary degree. The likelihood a four-year public college student earns over $25,000 is 71 percent. Four-year for-profit students are about 8 percentage points (or 11%) less likely

than their public counterparts to earn over this amount. But, the estimates are not statistically significantly different from zero at conventional levels. Among two-year students, the results indicate an increased likelihood of earning over $25,000, but they also are imprecise.

Taken together, the results in Table 4-6 indicate that four-year for-profit enrollment leads to higher likelihood of borrowing, larger debt originations, higher default, and worse labor market outcomes. The results among two-year students are less clear due to the imprecision of the estimates, but we do find evidence that these students are more likely to graduate, take on more debt, and are more likely to default. Despite their higher debt and the higher likelihood of degree receipt, our estimates suggest these students have worse labor market outcomes than community college students. The evidence thus indicates the return to these degrees is rather low. In the four-year sector, our results strongly suggest the return to for-profit enrollment relative to public four-year enrollment is negative. Students could earn more, take on less debt, and be more likely to graduate if they attended a public four-year institution.

How do the IV estimates relate to those from OLS models that control for the same fixed effects and observed characteristics? Online Appendix Tables A-15 through A-17 present such OLS results for our outcomes of interest. Broadly, the estimates are similar to those in Tables 4-6, but there are some notable differences. For direct and FFEL loans, the OLS estimates are larger in the two-year sector while they are smaller in the four-year sector. The default estimates are much smaller in the OLS than in the IV models in both sectors, while the labor market estimates are attenuated in the OLS relative to the IV models in the four-year sector. Especially in the four-year sector, failing to account for selection would lead one to under-state (in absolute value) most effects, which is consistent with negative selection into for-profits.

4.3 Robustness Checks

Our baseline estimates rely on numerous data and modeling assumptions that are detailed in the previous sections. We now present a series of robustness checks that probe the sensitivity of our findings to many of these assumptions. First, we examine whether for-profit institutions take advantage of the rise in demand from a negative labor market shock by raising tuition

relative to their public counterparts.[30] Table 7 presents IV estimates with tuition as the dependent variable. In Panel A, we show estimates for total tuition paid by in-state students. These estimates can be interpreted as the effect of for-profit enrollment on tuition payments per student. Among four-year students, tuition payments increase by about \$3,000-\$4,000, which roughly corresponds to the effect of for-profit enrollment on debt originations (Table 5). The tuition effect is larger for two-year students but it is less precise and also corresponds in magnitude with the debt origination estimates. Thus, for-profit enrollment leads to higher tuition levels, which then induces students to take on more debt. While for-profit students pay more in tuition, they are not incurring higher out of pocket tuition expenses because of increased loans. As a specification check, in Panel B, we see no evidence that attending a for-profit college is associated with higher average tuition. That is, for-profit institutions do not raise prices differentially due to the variation induced by our instrument.

A core identification concern in our analysis relates to serial correlation in the labor demand shock. We control for $\hat{\eta}$ directly in our regressions, but if any serial correlation in predicted labor demand is correlated with for-profit supply heterogeneity, then it could cause a bias in our estimates if it also is correlated with secular trends in our outcomes of interest. We perform robustness checks in which we control for leads of labor demand shocks to provide evidence that this source of bias is not present in our setting. In Table 8, we show results for a set of our outcomes in which we control for $\hat{\eta}_t$ and $\hat{\eta}_{t+1}$, each of these variables interacted with FP, $Supply$, and $FP * Supply$, and all of the other variables included in equations (2) and (3). Results are very similar to baseline. In Online Appendix Table A-9, we show estimates that only include $\hat{\eta}_t$ and the associated interactions, and again the estimates are very close to those that do not account for leads. These estimates indicate that serial correlation in the instrument is not biasing our results.

In the results shown thus far, four-year institutions are defined as any institution that grants at least one Bachelor's degree. Many universities have a mix of two- and four-year degree programs, and so in Table 9 we explore the sensitivity of our results to different categorizations of four-year universities. The first four columns show estimates in which we classify four-year

[30] A related concern is that our instrument is correlated with changes in state appropriations, which form a large component of total resources for most public universities. In results available upon request, we controlled for state appropriations per student and the results and conclusions are unchanged.

schools as those with at least 25% of their degrees as Bachelors in 2000. In columns (5)-(8), we exclude from the four-year category the bottom 25 percent of schools according to percent Bachelors degrees awarded in 2000. The estimates are very similar to those shown previously, with two minor differences. First, the loan origination estimates are somewhat smaller and are not statistically significant in columns (1)-(4). But, they are qualitatively similar and those in columns (5)-(8) match those in Table 5 quite closely in terms of magnitude and statistical significance. Second, the employment estimates are larger in absolute value at between 14-16%.

Rather than splitting the sample by the percentage of Bachelors degrees awarded, we can instead control for interactions between baseline degree composition and year fixed effects. Online Appendix Tables A-10 through A-12 include controls for percent certificates awarded in 2000 interacted with year indicators for the two-year regressions and percent Bachelor's degrees awarded in 2000 interacted with year indicators in the four-year sample. The results accord very closely with those shown in Tables 4-6 in sign, magnitude and statistical significance.

Our identification strategy relies on local postsecondary enrollment, which means online options can generate bias if the online enrollment is coming from other areas with different postsecondary supply and labor market conditions. We exclude online colleges and universities from our analysis in Online Appendix Table A-14. Because online institutions are still not a large part of the postsecondary market, this restriction does not have a large effect on the sample size, and the point estimates are changed little from baseline. The one exception is that the employment estimates in the four-year sector are larger in absolute value, but they also are imprecise such that the 95% confidence interval includes the preferred estimate. As previously discussed, we also can exclude selective institutions that have an admissions selectivity rating of Very Competitive or higher in the 2001 Barrons rankings. The first-stage and second-stage estimates shown in Appendix Table A-3 through A-6 are very similar to those shown above. Finally, we estimate models using 12-month undergraduate enrollment rather than 12-month total enrollment as the endogenous independent variable. Results for selected outcomes are shown in Online Appendix Table A-8, and they are are almost identical to those in Tables 4-6. Taken together, these results all indicate that our main estimates are robust to the specific modeling and data assumptions we make.

4.4 For-Profit Entry

Our estimates are identified off of students who are induced to attend different schools when there is a labor demand shock because of the latent distribution of for-profit supply in a CBSA. One of the mechanisms underlying our results could be entry or exit in response to changes in demand. To the extent new entrants are less productive, this could drive some of the worse outcomes we document among for-profit students. In Table 10, we examine how for-profit and public entry respond to labor demand changes. Panel A shows there is at most a very small relationship between predicted labor demand shocks and postsecondary supply. Among two-year schools, labor demand shocks do not predict any change in entry once state-year fixed effects are included in the model. Among four-year institutions, a 1 percentage point decrease in labor demand leads to a 0.01 percentage point increase in the likelihood a for-profit school enters, and this estimate is significant at the 10% level. There is no evidence that public four-year supply responds to labor demand shocks. These estimates indicate that in the four-year sector, for-profits are more responsive to demand conditions than are public institutions. Panels B and C shows that there is little heterogeneity across the 2000-2006 and 2008-2014 periods, except among public 2-year institutions. Public two-year entry responds positively to labor demand declines prior to the great recession but this relationship switches sign in the later period. These estimates are consistent with increases in less-than-two-year schools during the boom period of the early 2000s and with the decline in community colleges during the great recession (Chakrabarti, Lovenheim and Morris 2016b).

The results in Table 11 examine entry behavior in response to labor demand shocks differentially by pre-existing for-profit supply. It could be the case that areas with higher supply are more amenable to for-profit schools, in which case entry would be even higher in areas with a higher supply in 2000 when there is a labor demand decrease. Table 11 shows this to be the case: the effect of a negative labor demand shock on entry is larger in areas that had a larger share of for-profit institutions in 2000. In both the four-year and two-year sectors, these effects are most prominent in the 2008-2014 periods. Thus, especially during and after the great recession, areas that had a larger pre-existing supply of for-profit colleges experienced more growth in for-profit supply when a negative labor demand shock occurred. Interestingly,

we observe the same change in the cyclicality of public two-year entry documented in Table 10, but the magnitudes are quite small.

The results in Tables 10 and 11 are important because little currently is known about the for-profit entry decision. Our estimates indicate that for-profit institutions are responsive to labor-market driven demand, although the estimates are sufficiently small that we can only explain a small portion of entry behavior. This entry and exit behavior is unlikely drive our results for two reasons. First, endogenous entry behavior is quite modest: even if new entrants are less productive they are not prevalent enough to seriously impact our estimates. Second, the entry effect is mostly concentrated in the 2008-2014 period. For most long-run outcomes, the timing is such that the cohorts examined were in college prior to 2006. Hence, the post-2008 entry effects occur too late to affect these outcomes.

5 Conclusion

For-profit providers are becoming an increasingly important fixture of the higher education landscape. These institutions are controversial because they are more expensive to attend, students who attend them take on more debt and default at higher rates, and post-enrollment earnings of for-profit attendees is lower. Because students from disadvantaged backgrounds are more likely to enroll in for-profit postsecondary schools, one cannot interpret the negative correlation between for-profit attendance and student outcomes as evidence of a negative causal effect relative to attending a public college or university. For-profit institutions may be more responsive to student demand and to the needs of the local labor market, so it is plausible that returns to investing in this type of school are positive as well. These arguments underscore the importance of identifying the return to for-profit enrollment.

The research to date on the returns to for-profit postsecondary attendance looks at a limited set of outcomes and/or faces serious challenges overcoming the selection of students into different higher education sectors. We contribute to the literature by providing a more comprehensive analysis of the return to for-profit enrollment using several administrative datasources that have not previously been brought to bear on this question. Our approach to overcoming the selection problem is to exploit predicted labor demand changes using the shift-share instrument first

introduced by Bartik (1991) combined with the pre-existing supply of for-profit institutions in a CBSA. The idea underlying our approach is that labor demand decreases increase the demand for postsecondary enrollment, and most students induced to attend when labor demand is low will go to local schools. We compare enrollment and student outcomes across CBSAs that experience the same labor demand shock in a given year but that have different for-profit supply in our base year (2000). Our estimates strongly support the conclusion that students are more likely to sort into for-profit schools due to a labor demand shock when there are more such schools in the area in 2000. These results are interesting in their own right because they show that in many areas the for-profit sector is an important component of worker retraining during recessions.

Using predicted labor demand shocks interacted with 2000 for-profit supply as an instrument for for-profit enrollment, we estimate the causal effect of for-profit enrollment relative to enrolling in a public college or university on a large set of educational and labor market outcomes: propensity to borrow, number of loans originated, loan amounts, loan default, graduation, employment and earnings. We do the analysis separately for four-year and two-year institutions as well. The four-year estimates point consistently to negative effects of enrolling in a for-profit college on student outcomes. Relative to their public counterparts, enrolling in a for-profit institution increases the likelihood of borrowing by between 42 and 69 percent, increases the size of loans originated by over $3,300, and leads to an 11 percentage point increase in the likelihood of loan default. There is some evidence for-profit students are less likely to graduate and that they have substantially worse labor market outcomes.

Estimates among two-year and less-than-two-year students are less precisely estimated but are consistent with worse outcomes among for-profit students. These students take out more loans, originate over $6,000 more in loans, and are much more likely to default. However, the estimates indicate that these students originate much higher loan amounts and are more likely to default. They also experience lower likelihoods of employment and lower earnings on average, but they are more likely to earn over $25,000. Interestingly, for-profit students appear more likely to graduate than community college students, which suggests that the return to obtaining sub-baccalaureate for-profit degrees is particularly low. Our estimates identify a (local) average

effect of for-profit enrollment, but we are unable to determine what properties of for-profit colleges are associated with more versus less productivity. We examine this important question in ongoing follow-on work.

Finally, we present the first evidence in the literature on how local labor demand shocks affect the entry and exit decisions of for-profit schools. Our results indicate that for-profit schools are more likely to enter when there is a local labor demand shock, and that this effect is larger in areas in which the pre-existing supply of for-profits is higher. These findings suggest that for-profit institutions are more responsive to student demand changes than are public schools. The factors that induce for-profit entry are likely to be much more expansive than labor market outcomes, and this is a question of high importance in its own right that we plan to explore in the future.

Overall, our results indicate that, on average, for-profit enrollment leads to worse outcomes for students than enrolling in a public college or university. This is an important set of findings for several reasons. First, a substantial amount of public funds go to for-profit institutions through the financial aid system. Our estimates indicate the return to such expenditures may be quite low, particularly among four-year students. Second, the results suggest that students who attend local postsecondary institutions when there is a negative labor demand shock may be making mistakes: they would be better off attending the local public college or university instead. This highlights the potentially important role for providing such students with more information to help support them in making more informed choices. Exactly how to provide such information to students and what effects it will have on enrollment decisions is an important set of questions for future research.

References

[1] Aguiar, Mark, Erik Hurst and Loukas Karabarbounis. 2013. "Time Use During the Great Recession." *American Economic Review* 103(5): 1664-1696.

[2] Andrews, Rodney J., Jing Li and Michael F. Lovenheim. 2016. "Quantile Treatment Effects of College Quality on Earnings." *Journal of Human Resources* 51(1): 200-238.

[3] Arcidiacono, Peter and Michael F. Lovenheim. 2016. "Affirmative Action and the Quality-Fit Tradeoff." *Journal of Economic Literature* 54(1): 3-51.

[4] Autor, David H. and Mark G. Duggan. 2003. "The Rise in the Disability Rolls and the Decline in Unemployment." *Quarterly Journal of Economics* 118(1): 157-205.

[5] Bartik, Timothy J. 1991. "Who Benefits from State and Local Development Policies?" Upjohn Institute for Employment Research: Kalamazoo, MI.

[6] Barr, Andrew, 2015. "From the Battlefield to the Schoolyard: The Short- Term Impact of the Post-9/11 GI Bill." *Journal of Human Resources* 50(3): 580–613.

[7] Becker, Gary S. 1962. "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy* 70(5), Part 2: 9–49.

[8] Betts, Julian R. and Laurel L. McFarland. 1995. "Safe Port in a Storm: The Impact of Labor Market Conditions on Community College Enrollments." *Journal of Human Resources* 30(4): 741–765.

[9] Black, Dan A. and Jeffrey A. Smith. 2004. "How Robust is the Evidence on the Effects of College Quality? Evidence from Matching." *Journal of Econometrics* 121(1-2): 99–124.

[10] Black, Dan A. and Jeffrey A. Smith. 2006. "Estimating the Returns to College Quality with Multiple Proxies for Quality." *Journal of Labor Economics* 24(3): 701–728.

[11] Brewer, Dominik, Eric Eide and Ronald Ehrenberg, 1999. "Does it Pay to Attend an Elite Private College? Cross-Cohort Evidence on the Effects of College Type on Earnings." *Journal of Human Resources* 34(1): 104-123.

[12] Chakrabarti, Rajashri, Michael Lovenheim and Kevin Morris, 2016a. "Who falters at student loan payback time?" *Federal Reserve Bank of New York Liberty Street Economics* (blog), September 9.

[13] Chakrabarti, Rajashri, Michael Lovenheim and Kevin Morris, 2016b. "Who Contributed to the Enrollment Bulge? The Changing Face of the Higher Education Market" *Federal Reserve Bank of New York Liberty Street Economics* (blog), September 7.

[14] Christian, Michael S. 2007. "Liquidity Constraints and the Cyclicality of College Enrollment in the United States." *Oxford Economic Papers* 59(1): 141-169.

[15] Cellini, Stephanie Riegg and Latika Chaudhary. 2014. "The Labor Market Returns to a For-profit College Education." *Economics of Education Review* 43: 125-140.

[16] Cellini, Stephanie Riegg and Claudia Goldin. 2014. "Does Federal Student Aid Raise Tuition? New Evidence on For-Profit Colleges." *American Economic Journal: Economic Policy* 6(4): 174-206.

[17] Cellini, Stephanie Riegg and Nicholas Turner. 2016. "Gainfully Employed? Assessing the Employment and Earnings of For-Profit College Students Using Administrative Data." Mimeo.

[18] Cellini, Stephanie Riegg, Rajeev Darolia and Lesley J. Turner. 2016. "Where Do Students Go when For-Profit Colleges Lose Federal Aid?" Mimeo.

[19] Clark, Damon. 2011. "Do Recessions Keep Students in School? The Impact of Youth Unemployment on Enrolment in Post-compulsory Education in England." *Economica* 78: 523-545.

[20] Dale, Stacey Berg and Alan B. Krueger. 2002. "Estimating the Payoff to Attending A More Selective College: An Application of Selection on Observables and Unobservables." *Quarterly Journal of Economics* 117(4): 1491-1527.

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

[22] Deming, David J., Claudia Goldin and Lawrence F. Katz. 2012. "The For-Profit Postsecondary School Sector: Nimble Critters or Agile Predators?" *Journal of Economic Perspectives* 26(1): 139-163.

[23] Deming, David J., Claudia Goldin and Lawrence F. Katz. 2013. "For-profit Colleges." *The Future of Children* 23(1): 137-163.

[24] Deming, David J., Michael Lovenheim and Richard Patterson. 2016. "The Competitive Effects of Online Education." Mimeo.

[25] Deming, David J., Noam Yuchtman, Amira Abulafi, Claudia Goldin and Lawrence F. Katz. 2016. "The Value of Postsecondary Credentials in the Labor Market: An Experimental Study." *American Economic Review* 106(3): 778-806.

[26] Gilpin, Gregory A., Joseph Saunders and Christiana Stoddard. 2015. "Why Has For-profit Colleges Share of Higher Education Expanded so Rapidly? Estimating the Responsiveness to Labor Market Changes." *Economics of Education Review* 45: 53-63.

[27] Goodman, Sarena and Alice Henriques. 2016. "Attendance Spillovers between Public and For-Profit Colleges: Evidence from Statewide Variation in Appropriations for Higher Education." Mimeo.

[28] Hershbein, Bradley J. 2012. "Graduating High School in a Recession: Work, Education, and Home Production." *B.E. Journal of Economic Analysis & Policy* 12(1): 1-30.

[29] Hoekstra, Mark. 2009. "The Effect of Attending the State Flagship University on Earnings: A Discontinuity-Based Approach." *Review of Economics and Statistics* 91(4): 717–724.

[30] Hoxby, Caroline M. 2009. "The Changing Selectivity of American Colleges." *Journal of Economic Perspectives* 23(4): 95-118.

[31] Hoxby, Caroline and Christopher Avery. 2013. "The Missing "One-Offs": The Hidden Supply of High-Achieving, Low-Income Students." *Brookings Papers on Economic Activity* Spring: 1-50.

[32] Hoxby, Caroline and Sarah Turner. 2013. "Expanding College Opportunities for High-achieving, Low Income Students." Stanford Institute for Economic Policy Research Discussion Paper 12-014.

[33] Jacobson, Louis, Robert LaLonde, and Daniel G. Sullivan. 2005. "Estimating the Returns to Community College Schooling for Displaced Workers." *Journal of Econometrics* 125(1-2): 271-304.

[34] Jepsen, Christopher, Kenneth Troske and Paul Coomes. 2014. "The Labor-Market Returns to Community College Degrees, Diplomas, and Certificates." *Journal of Labor Economics* 32(1): 95-121.

[35] Lang, Kevin and Russell Weinstein. 2012. "Evaluating Student Outcomes at For-Profit Colleges." NBER Working Paper No. 18201.

[36] Lang, Kevin and Russell Weinstein. 2013. "The Wage Effects of Not-for-profit and For-profit Certifications: Better Data, Somewhat Different Results." *Labour Economics* 24: 230-243.

[37] Lang, Kevin and Russell Weinstein. 2013. "The Wage Effects of Not-for-profit and For-profit Certifications: Better Data, Somewhat Different Results." *Labour Economics* 24: 230-243.

[38] Long, Bridget Terry. 2014. "The Financial Crisis and College Affordability: How Have Students and Their Families Responded?" In *How the Recession Affected Higher Education*, Jeffrey Brown and Caroline M. Hoxby (Eds.). University of Chicago Press: Chicago, IL.

[39] Notowidigdo, Matthew. 2013. "The Incidence of Local Labor Demand Shocks." Mimeo.

[40] Stratford, Michael. 2017. "Trump and DeVos fuel a for-profit college comeback." Politico: https://www.politico.com/story/2017/08/31/devos-trump-forprofit-college-education-242193, last accessed 11/6/2017.

[41] Turner, Nicholas. 2011. "Do Students Profit from For-Profit Education? Estimating the Returns to Postsecondary Education with Tax Data." Available at: http://works.bepress.com/nicholas_turner/6/.

Figure 1: Labor Demand Changes by CBSA, 1997-2000

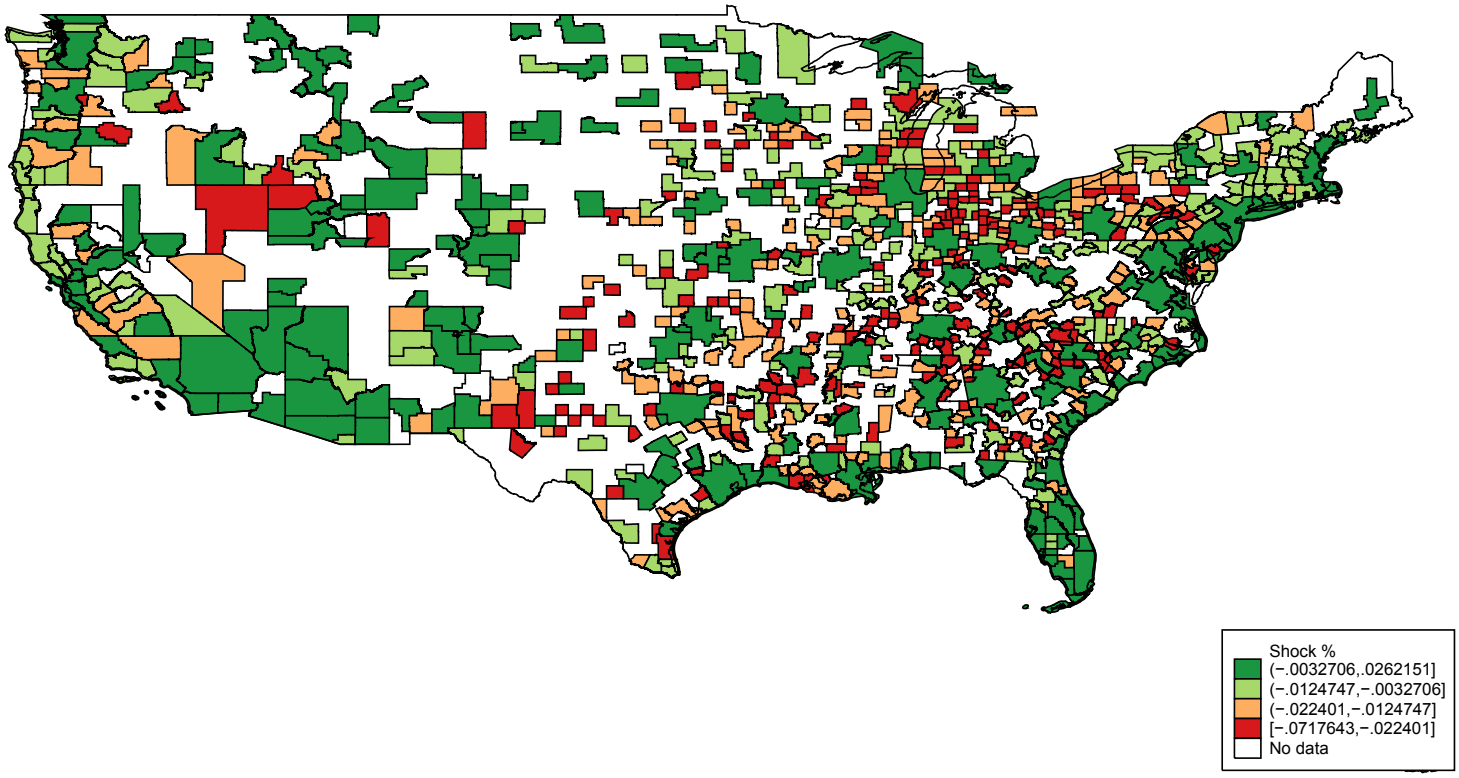


Figure 2: Quartiles of Labor Demand Changes by Year, 2000-2014

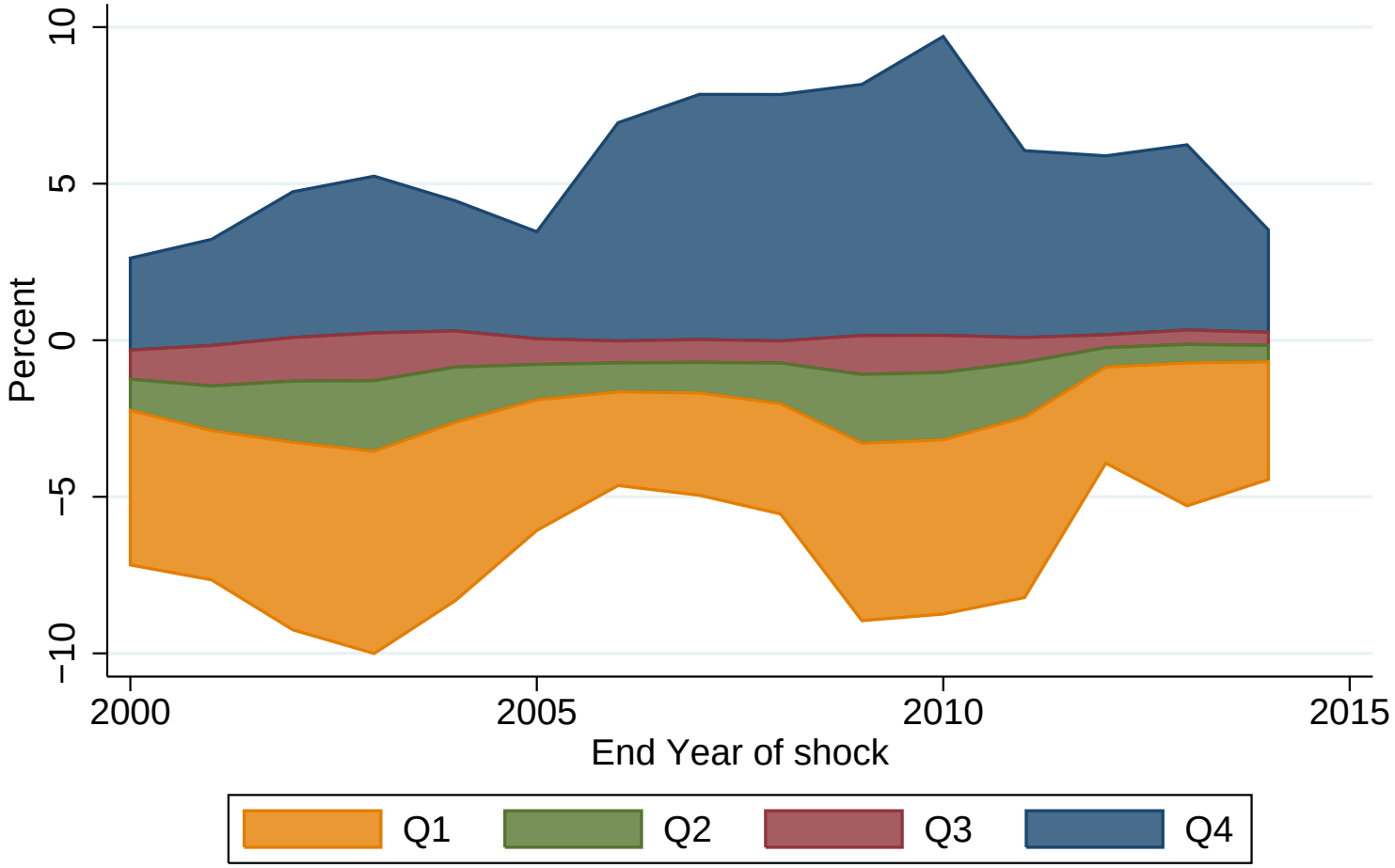


Figure 3: Percent of Two-year and Less-than-two-year For-profit Postsecondary Institutions by CBSA, 2000

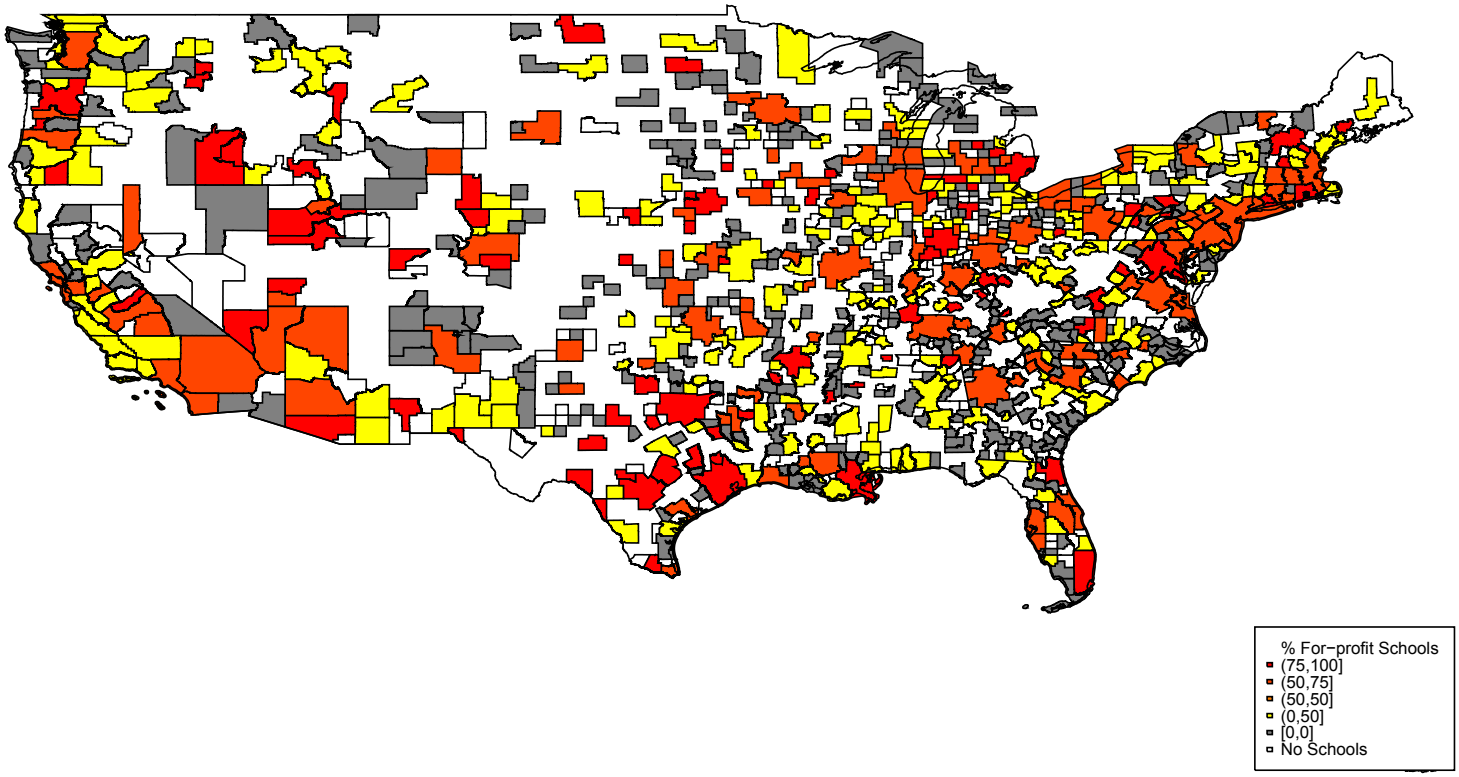


Figure 4: Percent of Four-year For-profit Postsecondary Institutions by CBSA, 2000

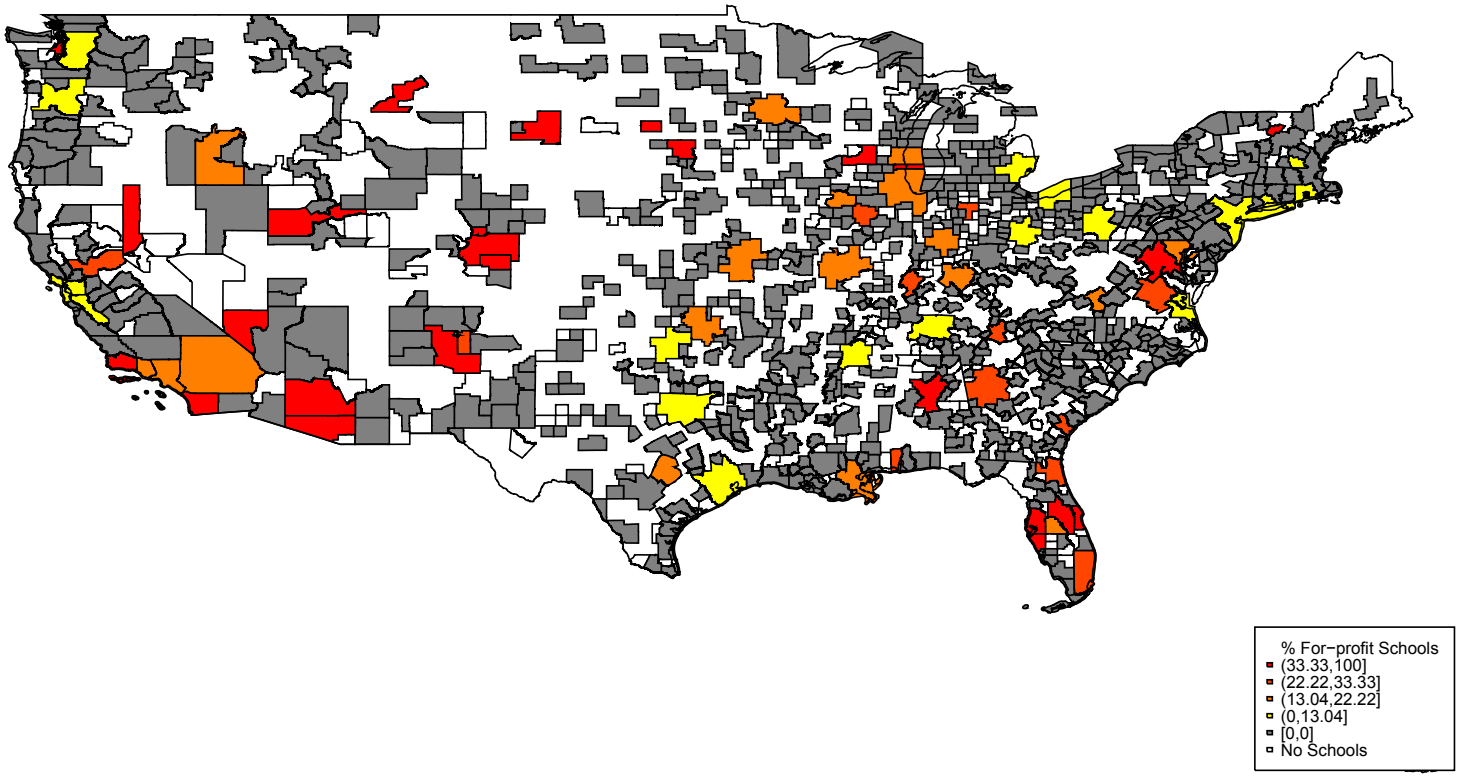
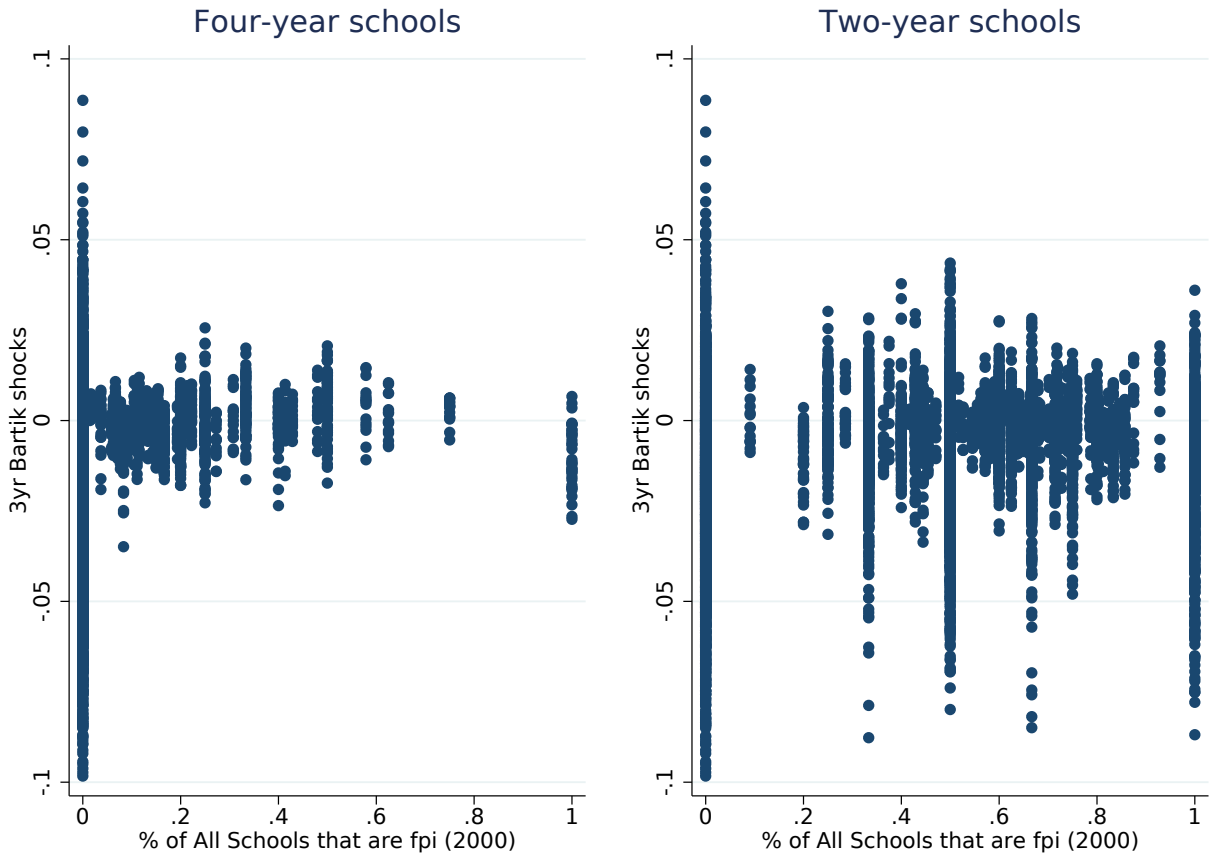
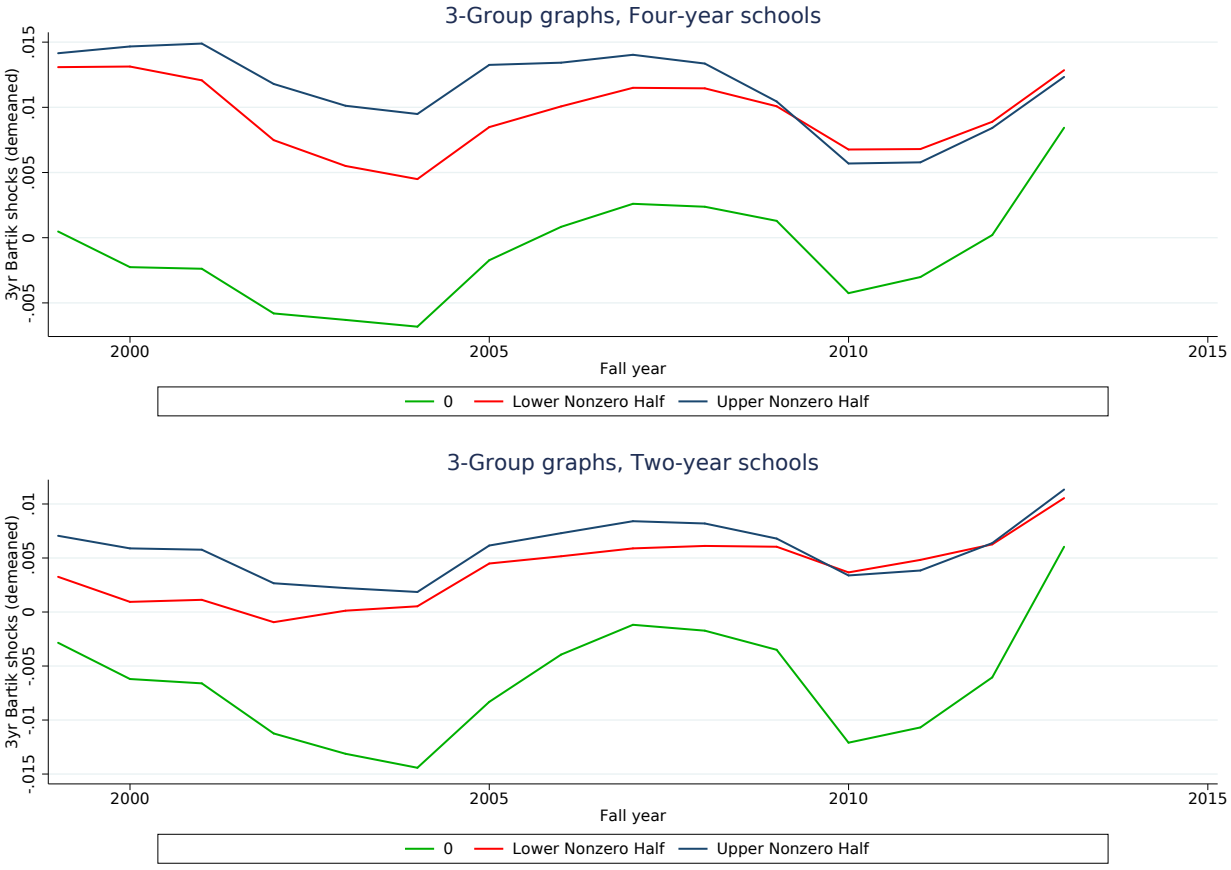


Figure 5: The Correlation of Demeaned Labor Demand Changes and 2000 For-Profit Supply



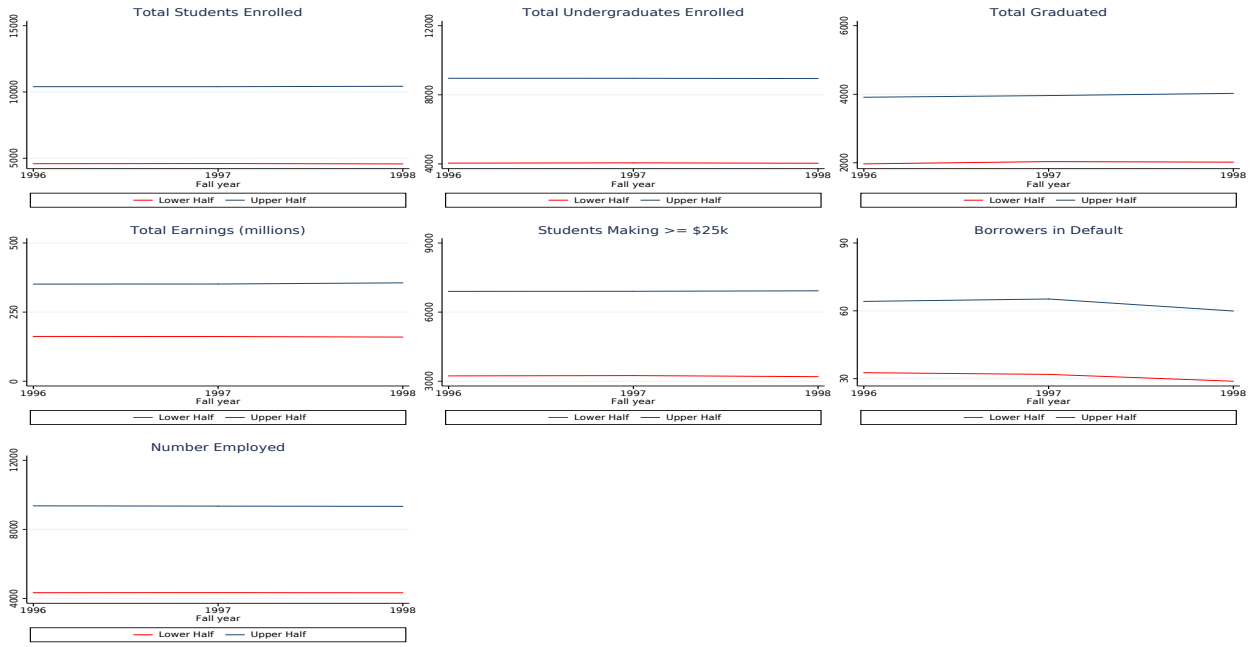
This figure plots the relationship between 3-year rolling predicted labor demand changes at the CBSA-year level that are residual to the CBSA-specific mean with 2000 for-profit supply. Each point shows a separate a CBSA-year observation, but only labor demand changes by year. The time span is 2000-14.

Figure 6: Demeaned Predicted Labor Demand Trends by For-Profit Supply and Postsecondary Sector



This figure plots trends over time in 3-year rolling predicted labor demand changes that are residual to the CBSA-specific mean for CBSAs that have no for-profits in 2000 and for CBSAs with above and below median for-profit shares among those with any for-profit institutions in 2000. The top panel shows trends among four-year institutions while the bottom panel shows trends among two-year schools.

Figure 7: Pretrends by For-Profit SupplyxLabor Demand, Below and Above Median



This figure plots the coefficient of the three-way interaction between for-profit dummy, Bartik labor demand shock and for-profit supply from specification xx and the corresponding 95% confidence interval. The time span is 2000-14 and the coefficients capture the relative shifts in for-profit enrollment relative to the corresponding public counterpart. The dependent variable is total enrollment aggregated to geography (here CBSA), sector (public or for-profit) and level (two-year or four-year). The supply measure is percentage of for-profit institutions at the corresponding level (two year or four-year) in the specific geography at the start of the sample period in fall 2000. All regressions include geography and year fixed effects. Two year institution group includes two-year and less than two year institutions.

Figure 8: First Stage Enrollment Estimates and 95% Confidence Intervals Using Different Enrollment Measures

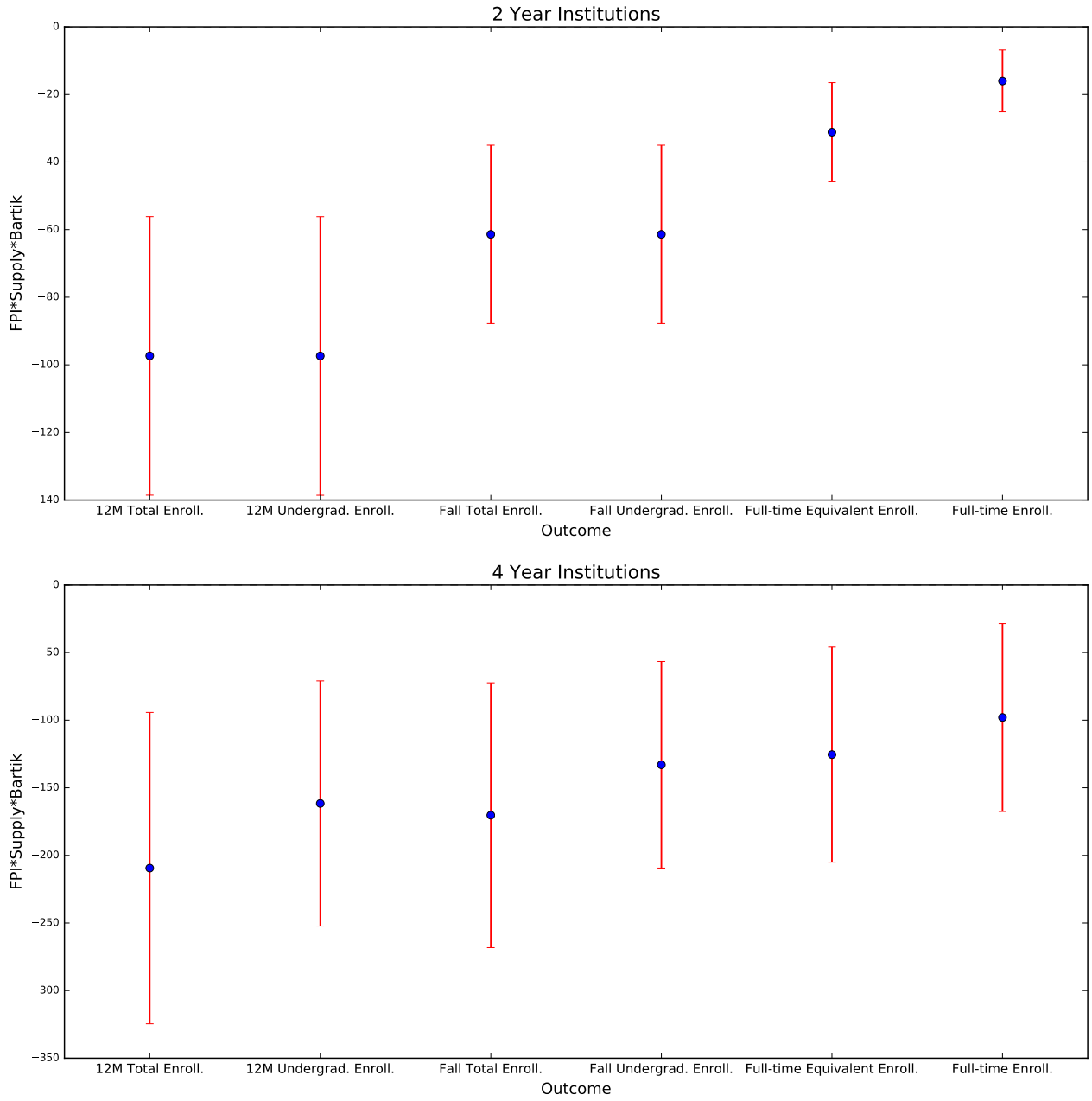


Table 1: Summary Statistics (2000-2014)

	All (1)	Two-year For-profit (2)	Two-year Public (3)	Four-year For-profit (4)	Four-year Public (5)
Total Enrollment	6795.33	1313.93	13506.19	1812.81	10642.37
	(27899.94)	(5452.56)	(45732.31)	(16596.17)	(25522.42)
Undergraduate Enrollment	6127.71	1313.20	13505.70	1463.80	8424.12
	(26146.06)	(5450.77)	(45732.30)	(12979.28)	(20098.62)
Number of (Subsidized) Direct Loan Recipients	868.52	388.57	627.30	494.96	1908.09
	(4525.77)	(2082.72)	(2444.71)	(6265.48)	(5341.76)
Number of (Unsubsidized) Direct Loan Recipients	795.53	352.11	489.85	523.19	1759.70
	(4513.01)	(1936.99)	(1973.82)	(6600.59)	(5143.62)
Number of (Subsidized) FFEL Loan Recipients	923.56	543.54	731.71	716.77	1658.11
	(4393.75)	(2275.37)	(1688.12)	(7129.23)	(3907.05)
Number of (Unsubsidized) FFEL Loan Recipients	744.24	457.94	527.74	671.99	1280.50
	(3997.14)	(1910.71)	(1311.67)	(6855.46)	(3013.82)
Number of Federal Loans	3601.57	1865.44	2274.18	2733.97	7298.46
	(15364.28)	(7515.53)	(5653.43)	(23738.66)	(15625.84)
Total Loan Origination (in $1000s)	15038.06	6241.13	6516.21	12077.12	33993.63
	(71651.02)	(25869.04)	(17057.94)	(110068.06)	(79014.35)
Number of Borrowers in Default, 100% completion time	61.64	57.53	57.07	62.34	68.94
	(461.43)	(249.95)	(123.44)	(850.75)	(151.58)
Total students employed 6 years after enrolling	5305.94	749.43	10804.75	800.37	8942.68
	(21529.78)	(3043.83)	(36068.29)	(7219.96)	(21009.97)
Mean Earnings for non-enrolled students 6 years after enrolling	24371.16	15459.54	22969.88	30379.27	33276.77
	(9047.43)	(7482.15)	(5084.39)	(7918.68)	(6124.14)
Share earning $25k or more 6 years after enrolling	0.46	0.25	0.46	0.53	0.65
	(0.21)	(0.18)	(0.14)	(0.21)	(0.11)
Number of students graduating in 150% completion time	1903.09	697.03	2911.91	201.62	3786.64
	(8233.03)	(2959.98)	(11420.83)	(1880.38)	(10953.21)
3 year Bartik Shock	-1.10	-1.10	-1.10	-1.10	-1.10
	(1.78)	(1.78)	(1.78)	(1.79)	(1.79)
Base employment in 1999-2000	139436.63	143809.94	143809.94	135397.69	135397.69
	(460414.98)	(469288.35)	(469288.35)	(452057.46)	(452057.46)
% For-Profit schools in 2000	14.76	27.98	27.98	2.56	2.56
	(24.93)	(29.13)	(29.13)	(10.12)	(10.12)
Observations	45688	10968	10968	11876	11876

Means of variables reported in cells. Standard deviations in parentheses. Predicted labor demand shocks are constructed using equation (1) in the text. Two-digit industry employment data from QCEW are used for computation of predicted labor demand shocks. Earnings, number working and number not working are measured six years after entering school. Number of borrowers in default relate to imputed freshmen cohorts assuming 100% or 150% completion time, as labeled. All years are indexed by spring year. Both total and undergraduate enrollment refer to twelve-month enrollment.

Table 2: The Correlation Between the Instrument and the Composition of Students in For-Profits

Dependent Variable	Two-Year	Four-Year
% Undergraduates Black	-0.009	0.004
	(0.012)	(0.026)
% Undergraduates Hispanic	0.005	0.024
	(0.004)	(0.016)
% Undergraduates White	-0.015	-0.047
	(0.014)	(0.050)
% Undergraduates Female	-0.050*	0.022
	(0.026)	(0.044)
% Undergraduates Over 25	-0.007	0.067
	(0.013)	(0.063)
% Undergraduates Receiving	-0.001	-0.110*
Pell Grant	(0.024)	(0.060)
% Undergraduates Receiving	0.023	-0.019
Federal Loans	(0.036)	(0.090)
% Undergraduates Sending FAFSA to 1 College	0.02	0.008
	(0.015)	(0.067)
% Undergraduates Sending FAFSA to 2 Colleges	-0.015	-0.011
	(0.011)	(0.033)
% Undergraduates Sending FAFSA to 3 Colleges	-0.0005	-0.009
	(0.003)	(0.011)
% Undergraduates Sending FAFSA to 4 Colleges	-0.002	-0.0005
	(0.001)	(0.008)
% Undergraduates Sending FAFSA to 5 Colleges	-0.003	0.016
	(0.003)	(0.028)
% Parents with HS Diploma	-0.001	-0.048
	(0.017)	(0.044)
% Parents with MS	-0.004	-0.00004
	(0.004)	(0.006)
% Parents with College Degree	0.008	-0.132***
	(0.013)	(0.049)
Average Family Income	7.165	-92.104
	(15.546)	(57.910)
College Average SAT Score	-0.315	-0.282
	(0.202)	(1.327)
% Arts Degrees	-0.023	0.028
	(0.029)	(0.083)
% STEM Degrees	0.029	0.007
	(0.040)	(0.073)
% Vocational Degrees	-0.0001	-0.028
	(0.040)	(0.021)
% Associates Degrees	0.020	0.033
	(0.048)	(0.091)
% Bachelors Degrees	0.00004	-0.099
	(0.00003)	(0.093)
% Certificates	-0.020	0.020
	(0.048)	(0.039)

This table uses public and for-profit institutions. Each cell is a separate estimate that comes from estimation of equation (2) and shows the coefficient on (For-profit)*(2000 Supply)*$\hat{\eta}$. Robust standard errors adjusted for clustering by CBSA are in parentheses: *, **, *** indicate significance at the 10, 5, and 1 percent level, respectively. All % measures range from 0 to 100. Labor demand shocks are proxied by three-year rolling predicted changes as described in the text. Two- digit industry employment data from QCEW are used for computation of labor demand shocks. The supply measure is percentage of for-profit institutions at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000 or 2006-2007 (for 2008-14 phase). All regressions include the following CBSA level variables as controls: base-year employment, gender composition (% female), racial composition (% black, %Hispanic, %American Indian, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 3: First Stage Enrollment Estimates

Panel A: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	52.299***	50.074***	64.029***	62.654***	31.870***	31.233***
	(11.856)	(12.058)	(13.751)	(14.002)	(8.4252)	(8.5910)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-97.355***	-97.355***	-129.19***	-129.19***	-65.285***	-65.285***
	(21.025)	(21.366)	(25.890)	(26.274)	(16.359)	(16.603)
	[-4.6304]	[-4.5564]	[-4.9900]	[-4.9171]	[-3.9907]	[-3.9322]
Time Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	21666	21666	10172	10172	10130	10130
R-squared	0.1548	0.1574	0.1634	0.1647	0.1351	0.1363
IV P-value	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001
Panel B: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	55.879***	53.654***	67.996***	66.621***	28.155***	27.518***
	(12.964)	(13.152)	(14.654)	(14.980)	(8.6978)	(8.8361)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-104.51***	-104.51***	-137.12***	-137.12***	-57.855***	-57.855***
	(22.740)	(23.110)	(27.555)	(27.965)	(16.756)	(17.006)
	[-4.5961]	[-4.5224]	[-4.9764]	[-4.9034]	[-3.4529]	[-3.4020]
Time Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21666	21666	10172	10172	10130	10130
R-squared	0.2582	0.2607	0.2705	0.2719	0.2320	0.2332
IV P-value	0.0000	0.0000	0.0000	0.0000	0.0007	0.0005
Panel C: 4 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	12.624	25.154	39.933	46.130	88.091***	89.163***
	(46.124)	(42.462)	(36.827)	(35.519)	(24.026)	(24.044)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-209.42***	-209.42***	-148.95***	-148.95***	-292.37***	-292.37***
	(58.760)	(59.639)	(53.599)	(54.328)	(68.765)	(69.709)
	[-3.5640]	[-3.5114]	[-2.7791]	[-2.7418]	[-4.2518]	[-4.1942]
Time Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	23482	23482	11044	11044	10954	10954
R-squared	0.1801	0.1894	0.2293	0.2320	0.1333	0.1384
IV P-value	0.0017	0.0021	0.0022	0.0035	0.0001	0.0002
Panel D: 4 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	22.778	35.308	53.451	59.648*	92.657***	93.729***
	(42.845)	(41.869)	(33.447)	(32.397)	(20.359)	(21.510)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-229.73***	-229.73***	-175.99***	-175.99***	-301.51***	-301.51***
	(71.427)	(72.499)	(49.555)	(50.233)	(60.072)	(60.901)
	[-3.2162]	[-3.1687]	[-3.5514]	[-3.5035]	[-5.0191]	[-4.9508]
Time Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23482	23482	11044	11044	10954	10954
R-squared	0.2308	0.2400	0.2840	0.2867	0.2053	0.2105
IV P-value	0.0059	0.0067	0.0004	0.0005	0.0000	0.0000

This table uses public and for-profit institutions. The dependent variable is total 12-month enrollment aggregated by CBSA, sector (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *, **, *** indicate significance at the 10, 5, and 1 percent level, respectively. "IV P-value' shows the p-value of F-tests for joint significance of instruments. Labor demand shocks are proxied by three-year rolling predicted changes as described in the text. Two- digit industry employment data from QCEW are used for computation of labor demand shocks. The supply measure is percentage of for-profit institutions at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000 or 2006-2007 (for 2008-14 phase). All regressions include the following CBSA level variables as controls: base-year employment, gender composition (% female), racial composition (% black, %Hispanic, %American Indian, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 4: Instrumental Variables Estimates of the Impact of For-profit Attendance on Student Borrowing, by Loan Type

	Panel A: Number Recipients: Direct Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.007570	0.008813	-0.03506	-0.03150	0.3427**	0.3291**	0.3535**	0.3361**
	(0.1224)	(0.1181)	(0.2229)	(0.2111)	(0.1561)	(0.1500)	(0.1767)	(0.1697)
Enroll*For-profit	0.1630	0.1568	-0.09018	-0.09904	0.6144**	0.6340**	0.6706**	0.6919**
	(0.7213)	(0.7418)	(1.3295)	(1.3495)	(0.2932)	(0.2816)	(0.3046)	(0.3033)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02039	0.02138	-0.01534	-0.01242	0.2962**	0.2926***	0.3098**	0.3017**
	(0.1056)	(0.1017)	(0.1901)	(0.1801)	(0.1262)	(0.1112)	(0.1469)	(0.1338)
Enroll*For-profit	0.2448	0.2399	0.03260	0.02535	0.5292**	0.5344**	0.6000**	0.6099**
	(0.6173)	(0.6357)	(1.1308)	(1.1483)	(0.2305)	(0.2244)	(0.2544)	(0.2553)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel C: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.02747	-0.02607	-0.09283	-0.09015	0.1553**	0.1676***	0.1755**	0.1856***
	(0.03327)	(0.03266)	(0.09426)	(0.09123)	(0.06832)	(0.06062)	(0.08143)	(0.07136)
Enroll*For-profit	0.007155	0.0002299	-0.4260	-0.4359	0.4569***	0.4416***	0.4867***	0.4758***
	(0.2128)	(0.2152)	(0.6254)	(0.6306)	(0.1168)	(0.1135)	(0.1342)	(0.1336)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.004041	-0.002334	-0.04253	-0.03997	0.09596*	0.1104**	0.1116*	0.1246**
	(0.02401)	(0.02368)	(0.05640)	(0.05460)	(0.04997)	(0.04310)	(0.05827)	(0.05052)
Enroll*For-profit	0.1621	0.1537	-0.09294	-0.1024	0.4145***	0.3966***	0.4376***	0.4236***
	(0.1519)	(0.1532)	(0.3663)	(0.3683)	(0.07399)	(0.07396)	(0.08604)	(0.08738)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicate significance at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 5: Instrumental Variables Estimates of the Impact of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults

	Panel A: Number of Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.06371	0.06718	0.01160	0.01845	0.4349***	0.4903***	0.4509**	0.5024***
	(0.1276)	(0.1229)	(0.2333)	(0.2204)	(0.1624)	(0.1136)	(0.1915)	(0.1472)
Enroll*For-profit	1.1479	1.1307	0.8385	0.8214	1.0874***	1.0079***	1.1707***	1.1079***
	(0.7267)	(0.7468)	(1.3644)	(1.3813)	(0.3050)	(0.2551)	(0.3272)	(0.2861)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel B: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	511.30	509.73	596.42	590.47	1401.5***	1823.2***	1492.1**	1893.8***
	(380.01)	(365.75)	(606.55)	(577.29)	(480.40)	(324.99)	(585.54)	(313.97)
Enroll*For-profit	5907.5***	5915.3***	6412.9*	6427.7*	3375.8***	2770.5**	3846.7***	3356.1***
	(2176.6)	(2252.0)	(3576.3)	(3654.1)	(1206.6)	(1108.8)	(1220.2)	(1034.7)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel C: Number of Borrowers in Default							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02593	0.02591	0.04402	0.04379	0.06415*	0.05814*	0.05686*	0.05099*
	(0.01622)	(0.01596)	(0.06728)	(0.06593)	(0.03609)	(0.02978)	(0.03391)	(0.02748)
Enroll*For-profit	0.2142*	0.2143*	0.3416	0.3421	0.1079***	0.1159***	0.1009***	0.1061***
	(0.1174)	(0.1186)	(0.4818)	(0.4835)	(0.03755)	(0.04028)	(0.03759)	(0.03982)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14500	14500	14500	14500	15347	15347	15347	15347

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicate significance at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 6: Instrumental Variables Estimates of the Impact of For-profit Attendance on Graduation, Employment, and Earnings

	Panel A: Total Graduated, assuming 150% Completion Time			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.1733	0.1787*	0.4110***	0.3988***
	(0.1136)	(0.1080)	(0.1028)	(0.09279)
Enroll*For-profit	0.2982	0.2812	-0.1353	-0.1225
	(0.7683)	(0.7798)	(0.2569)	(0.2659)
State*FP & Year*FP	No	Yes	No	Yes
Observations	17392	17392	18854	18854
	Panel B: Number Employed, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.8000***	0.8000***	0.8590***	0.8753***
	(0.09900)	(0.09892)	(0.02905)	(0.02196)
Enroll*For-profit	-0.3583	-0.3557	-0.09293**	-0.1104**
	(0.7803)	(0.7852)	(0.04414)	(0.04817)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452
	Panel C: Total Earnings, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	25879.6***	25865.2***	35981.2***	36887.6***
	(6618.6)	(6674.9)	(3396.5)	(2830.5)
Enroll*For-profit	-9718.7	-8541.2	-5138.1	-6107.6
	(52342.5)	(53038.9)	(6306.9)	(6624.4)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452
	Panel D: Total Students Making $25k, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.6230***	0.6226***	0.7015***	0.7134***
	(0.2072)	(0.2105)	(0.05117)	(0.04280)
Enroll*For-profit	0.3793	0.4195	-0.07259	-0.08525
	(1.6067)	(1.6323)	(0.09966)	(0.1085)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicate significance at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include CBSA and state-by-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 7: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Tuition

	Panel A: In-state Total Tuition Paid							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	2884.1***	2851.1***	3319.2**	3256.8**	1359.6**	1832.8***	1479.9**	1924.4***
	(818.11)	(795.41)	(1681.5)	(1590.4)	(530.90)	(336.23)	(671.42)	(414.70)
Enroll*For-profit	9215.3**	9379.2**	11798.9	11953.9	3443.2**	2764.1*	4067.9**	3524.9**
	(4475.7)	(4646.9)	(9724.2)	(9849.1)	(1638.7)	(1607.9)	(1678.4)	(1586.3)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel B: In-state Average Tuition Paid							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.08467	-0.1658	-0.08803	-0.2662	0.1768	0.1327	-0.03279	-0.1730
	(0.07574)	(0.1505)	(0.09586)	(0.3886)	(0.4099)	(6.3262)	(0.1292)	(0.2912)
Enroll*For-profit	-0.3305	-1.0385	-0.3489	-1.7695	-0.04672	-0.06611	0.1174	0.1690
	(0.5607)	(1.2080)	(0.6698)	(3.0025)	(0.2298)	(4.9799)	(0.08467)	(0.1853)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14493	14493	14493	14493	7423	7423	7423	7423

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling shocks. Two-digit industry employment data from QCEW are used for computation of the labor demand shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions. Panel A includes total tuition payments across all students while Panel B shows average tuition payments per student.

Table 8: Instrumental Variables Estimates Including Current-year and One-year Leads of Demand Shocks

	Panel A: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.01728	0.009313	0.003364	-0.01550	0.1367***	0.1533***	0.1407***	0.1570***
	(0.01257)	(0.01257)	(0.01534)	(0.01685)	(0.03867)	(0.02595)	(0.03980)	(0.02674)
Enroll*For-profit	0.2969***	0.2325***	0.2051*	0.06563	0.4175***	0.3993***	0.4219***	0.4051***
	(0.08685)	(0.08926)	(0.1128)	(0.1350)	(0.04275)	(0.03121)	(0.04400)	(0.03231)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15892	15892	15892	15892	17210	17210	17210	17210
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.007382	0.004560	-0.006633	-0.01788	0.1058***	0.1226***	0.1092***	0.1261***
	(0.009394)	(0.01086)	(0.01228)	(0.01511)	(0.02982)	(0.02039)	(0.03031)	(0.02079)
Enroll*For-profit	0.2326***	0.1938***	0.1401	0.04295	0.4129***	0.4008***	0.4174***	0.4069***
	(0.06487)	(0.07474)	(0.08670)	(0.1133)	(0.03069)	(0.02394)	(0.03222)	(0.02565)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15892	15892	15892	15892	17210	17210	17210	17210
	Panel C: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.1879	66.340	-362.67	-340.57	1956.9***	2273.1***	2070.3***	2370.1***
	(199.32)	(206.59)	(292.85)	(311.07)	(621.01)	(334.60)	(681.79)	(392.52)
Enroll*For-profit	2968.5**	3143.6**	810.90	576.61	4380.3***	4077.4***	4724.8***	4501.9***
	(1180.3)	(1266.3)	(1761.9)	(1971.8)	(1013.7)	(1053.2)	(918.87)	(879.38)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	18736	18736	18736	18736	20290	20290	20290	20290
	Panel D: Number Employed, 6 years after enrollment							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.8271***	0.8247***	0.8273***	0.8249***	0.8813***	0.8911***	0.8873***	0.8961***
	(0.01950)	(0.01802)	(0.01933)	(0.01784)	(0.02486)	(0.01630)	(0.02556)	(0.01753)
Enroll*For-profit	-0.1284	-0.1465	-0.1271	-0.1449	-0.01760	-0.04418	-0.008274	-0.03412
	(0.1373)	(0.1272)	(0.1361)	(0.1259)	(0.03391)	(0.02902)	(0.03432)	(0.02943)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8684	8684	8684	8684	9410	9410	9410	9410

This table uses public and for-profit institutions. We include current-year (year t) and a one-year lead ($t+1$) labor demand changes as well as all interactions among these labor demand changes, the for-profit indicator, and for-profit supply. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 9: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Student Borrowing, by Loan Type, 4-year Schools by Different Categorizations

	Panel A: Number Recipients: FFEL Subsidized Loans							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.1511**	0.1602***	0.1674**	0.1727***	0.1319**	0.1415**	0.1371***	0.1467***
	(0.06569)	(0.05330)	(0.06768)	(0.05491)	(0.05647)	(0.05785)	(0.05194)	(0.05375)
Enroll*For-profit	0.4412***	0.4320***	0.4704***	0.4651***	0.4110***	0.4010***	0.4182***	0.4083***
	(0.1251)	(0.1325)	(0.1357)	(0.1428)	(0.08366)	(0.08237)	(0.07922)	(0.07745)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16160	16160	16160	16160	15876	15876	15876	15876
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.07749	0.09471**	0.09060*	0.1048**	0.06303	0.07014	0.06569	0.07281*
	(0.05084)	(0.03984)	(0.05215)	(0.04126)	(0.04519)	(0.04425)	(0.04209)	(0.04166)
Enroll*For-profit	0.3932***	0.3759***	0.4167***	0.4024***	0.3805***	0.3730***	0.3842***	0.3768***
	(0.06848)	(0.07614)	(0.07613)	(0.08355)	(0.04239)	(0.04362)	(0.03838)	(0.03930)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16160	16160	16160	16160	15876	15876	15876	15876
	Panel C: Loan Origination Amount ($)							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1156.9***	1668.1***	1200.3***	1675.6***	1341.8***	1310.6***	1345.9***	1313.9***
	(424.60)	(475.88)	(458.60)	(388.58)	(491.30)	(401.96)	(508.31)	(419.29)
Enroll*For-profit	1773.5	1319.0	2323.4	1894.2	3171.7***	3203.4***	3230.8***	3262.9***
	(1849.1)	(1779.2)	(1458.2)	(1379.7)	(1020.6)	(937.01)	(985.02)	(892.79)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21992	21992	21992	21992	21492	21492	21492	21492
	Panel D: Number Employed, 6 years after enrollment							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.8439***	0.8610***	0.8433***	0.8606***	0.8264***	0.8248***	0.8284***	0.8268***
	(0.02728)	(0.02077)	(0.02911)	(0.02158)	(0.03204)	(0.02530)	(0.03364)	(0.02673)
Enroll*For-profit	-0.1439**	-0.1656***	-0.1452**	-0.1670***	-0.1438**	-0.1418**	-0.1401**	-0.1381***
	(0.05851)	(0.05580)	(0.06094)	(0.06017)	(0.06585)	(0.05614)	(0.06193)	(0.05315)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8824	8824	8824	8824	8710	8710	8710	8710

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (four-year schools, varying definitions). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Results for one-year Bartik shocks are not presented in tables, but are qualitavely similar. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (four-year schools, varying definitions) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects.

Table 10: Effect of Labor Demand Changes on Entry/Exit of Postsecondary Institutions, by Sector

Panel A: 2000-2014	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.02279**	-0.002090	0.002680	0.0009238	-0.02023***	-0.01484*	-0.003092	0.002028
	(0.01137)	(0.01260)	(0.004358)	(0.004510)	(0.006137)	(0.007938)	(0.002665)	(0.002858)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	10968	10968	10968	10968	11876	11876	11876	11876
R-squared	0.3150	0.3660	0.07552	0.2492	0.4425	0.4845	0.06926	0.2253
Mean of Dep. Var.	3.1477	3.1477	1.6693	1.6693	0.6508	0.6508	0.7695	0.7695
Mean of Dep. Var. $\mid > 0$	5.9452	5.9452	2.0339	2.0339	4.1024	4.1024	1.6028	1.6028
Mean of $\hat{\eta}$	-1.0976	-1.0976	-1.0976	-1.0976	-1.0964	-1.0964	-1.0964	-1.0964
Percent Effect	0.3834	0.03516	0.1318	0.04542	0.4931	0.3618	0.1929	0.1266
Panel B: 2000-2006	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.004761	0.01976	-0.009583*	-0.01862***	-0.003838	0.002965	0.003018	0.0007639
	(0.009099)	(0.01448)	(0.005589)	(0.006602)	(0.006447)	(0.007039)	(0.002480)	(0.003413)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5118	5118	5118	5118	5554	5554	5554	5554
R-squared	0.06608	0.1431	0.03987	0.2572	0.2137	0.2697	0.02455	0.1012
Mean of Dep. Var.	2.8839	2.8839	1.7411	1.7411	0.4354	0.4354	0.7420	0.7420
Mean of Dep. Var. $\mid > 0$	5.5530	5.5530	2.0613	2.0613	3.6252	3.6252	1.5905	1.5905
Mean of $\hat{\eta}$	-1.3096	-1.3096	-1.3096	-1.3096	-1.3134	-1.3134	-1.3134	-1.3134
Percent Effect	0.08574	0.3558	0.4649	0.9034	0.1059	0.08178	0.1898	0.04803
Panel C: 2008-2014	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.001299	-0.006986	0.006999*	0.008336**	-0.0007722	-0.0009775	-0.0006604	0.002921
	(0.007830)	(0.009935)	(0.003634)	(0.003654)	(0.004013)	(0.005445)	(0.002060)	(0.002674)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5120	5120	5120	5120	5532	5532	5532	5532
R-squared	0.3426	0.3884	0.04988	0.2023	0.5011	0.5358	0.04720	0.1959
Mean of Dep. Var.	3.4264	3.4264	1.5945	1.5945	0.8733	0.8733	0.7986	0.7986
Mean of Dep. Var. $\mid > 0$	6.3585	6.3585	2.0025	2.0025	4.4240	4.4240	1.6159	1.6159
Mean of $\hat{\eta}$	-0.9122	-0.9122	-0.9122	-0.9122	-0.9051	-0.9051	-0.9051	-0.9051
Percent Effect	0.02043	0.1099	0.3495	0.4163	0.01746	0.02209	0.04086	0.1808

This table uses number of for-profit or public institutions (as indicated). Standard errors clustered by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. All regressions include CBSA controls as described in the text, CBSA fixed effects and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table 11: Effect of Labor Demand Changes and For-Profit Supply on Entry/Exit of Postsecondary Institutions, by Sector

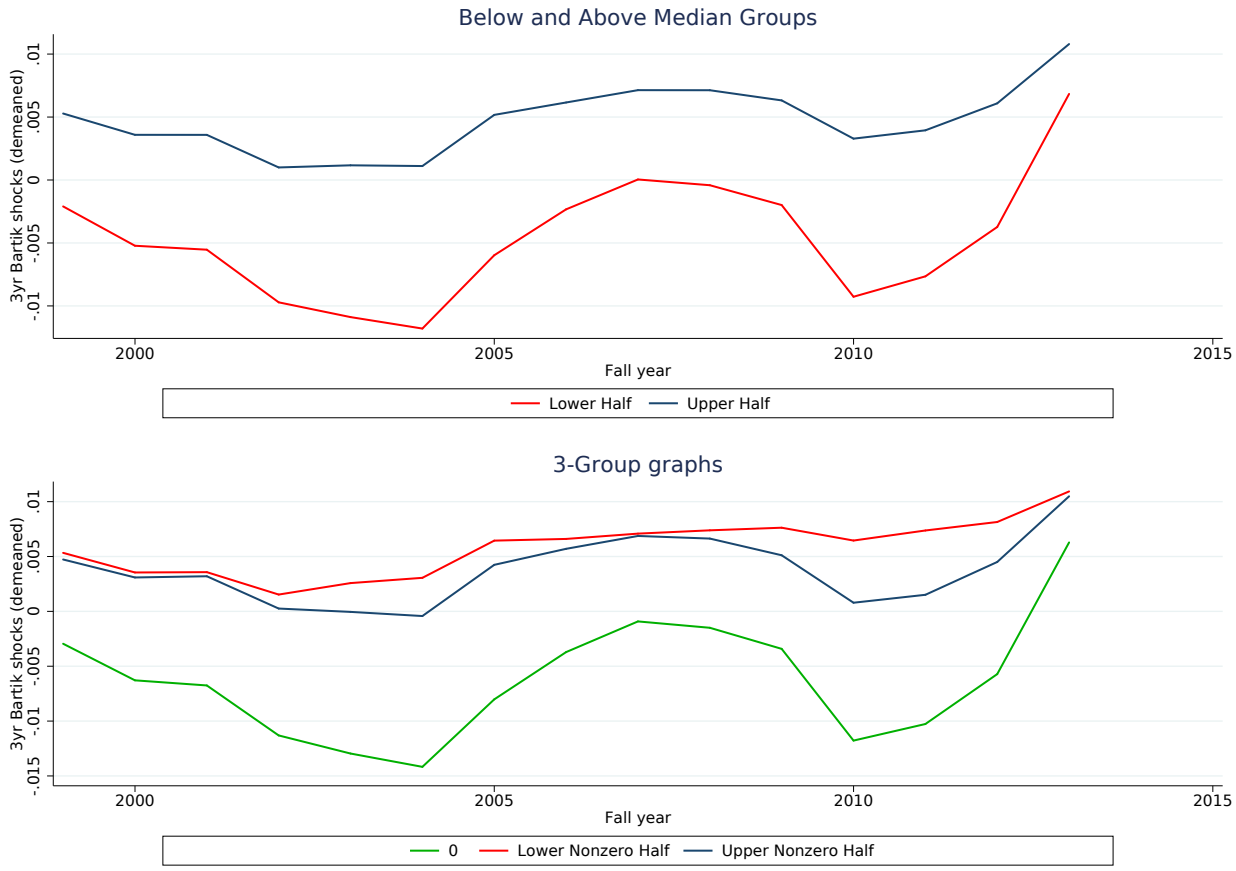
Panel A: 2000-2014	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.002228	0.01513	0.003855	0.004042	-0.01964***	-0.01393*	-0.001612	0.002574
	(0.009323)	(0.01137)	(0.004153)	(0.004793)	(0.005447)	(0.007895)	(0.002322)	(0.002717)
(2000 Supply)*($\hat{\eta}$)	-0.001330*	-0.001153*	-0.00007970	-0.0002073	-0.0008160	-0.001384	-0.002127	-0.001040
	(0.0007249)	(0.0006508)	(0.0002027)	(0.0002014)	(0.003707)	(0.003694)	(0.001737)	(0.001392)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	10833	10833	10833	10833	11741	11741	11741	11741
R-squared	0.3157	0.3666	0.07573	0.2498	0.4427	0.4852	0.07299	0.2263
Mean of Dep. Var.	3.1787	3.1787	1.6868	1.6868	0.6583	0.6583	0.7784	0.7784
Mean of Dep. Var. $\mid > 0$	6.0127	6.0127	2.0380	2.0380	4.1024	4.1024	1.6028	1.6028
Mean of $\hat{\eta}$	-1.0779	-1.0779	-1.0779	-1.0779	-1.0782	-1.0782	-1.0782	-1.0782
Percent Effect	0.03706	0.2516	0.1892	0.1983	0.4788	0.3396	0.1005	0.1606
Panel B: 2000-2006	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.01148	0.01581	-0.003018	-0.01084*	-0.005862	0.0008536	0.003029	0.0007977
	(0.009022)	(0.01569)	(0.005050)	(0.006393)	(0.005299)	(0.007110)	(0.002258)	(0.003259)
(2000 Supply)*($\hat{\eta}$)	0.0004231	0.0002636	-0.0004171	-0.0005077*	0.002718	0.002465	-0.00001994	-0.00007062
	(0.0005869)	(0.0006517)	(0.0002540)	(0.0002685)	(0.003048)	(0.003075)	(0.001072)	(0.0009406)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5086	5086	5086	5086	5522	5522	5522	5522
R-squared	0.06624	0.1432	0.04091	0.2589	0.2150	0.2711	0.02456	0.1014
Mean of Dep. Var.	2.8991	2.8991	1.7507	1.7507	0.4379	0.4379	0.7463	0.7463
Mean of Dep. Var. $\mid > 0$	5.5768	5.5768	2.0630	2.0630	3.6252	3.6252	1.5905	1.5905
Mean of $\hat{\eta}$	-1.2949	-1.2949	-1.2949	-1.2949	-1.2999	-1.2999	-1.2999	-1.2999
Percent Effect	0.2059	0.2835	0.1463	0.5254	0.1617	0.02355	0.1904	0.05015
Panel C: 2008-2014	2 year schools				4 year schools			
	For-Profit		Public		For-Profit		Public	
3 year rolling shocks	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	0.02207***	0.01387	0.004683	0.006797*	0.002646	0.002040	-0.0002332	0.002847
	(0.006718)	(0.009922)	(0.003417)	(0.003924)	(0.003598)	(0.005208)	(0.001537)	(0.002293)
(2000 Supply)*($\hat{\eta}$)	-0.001327***	-0.001246***	0.0001348	0.00009452	-0.002066	-0.001885	-0.0002480	0.000002534
	(0.0004309)	(0.0004429)	(0.0001373)	(0.0001292)	(0.001459)	(0.001282)	(0.0006282)	(0.0005103)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5065	5065	5065	5065	5477	5477	5477	5477
R-squared	0.3443	0.3900	0.05024	0.2028	0.5025	0.5369	0.04761	0.1967
Mean of Dep. Var.	3.4561	3.4561	1.6093	1.6093	0.8821	0.8821	0.8066	0.8066
Mean of Dep. Var. $\mid > 0$	6.4333	6.4333	2.0057	2.0057	4.4240	4.4240	1.6159	1.6159
Mean of $\hat{\eta}$	-0.8974	-0.8974	-0.8974	-0.8974	-0.8913	-0.8913	-0.8913	-0.8913
Percent Effect	0.3431	0.2156	0.2335	0.3389	0.05980	0.04610	0.01443	0.1762

This table uses number of for-profit or public institutions (as indicated). Standard errors clustered by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. All regressions include CBSA controls as described in the text, CBSA fixed effects and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Online Appendix

Online Appendix: Not for Publication

Figure A-1: Demeaned Predicted Labor Demand Trends by For-Profit Supply



The top panel of this figure plots trends over time in 3-year rolling predicted labor demand changes that are residual to the CBSA-specific mean for CBSAs that have above and below median 2000 for-profit shares. In the bottom panel, we plot demeaned 3-year rolling predicted labor demand changes for CBSAs with no for-profits and for CBSAs with above and below median for-profit shares among those with any for-profit institutions.

Figure A-2: Pretrends by For-Profit Supply, Below and Above Median

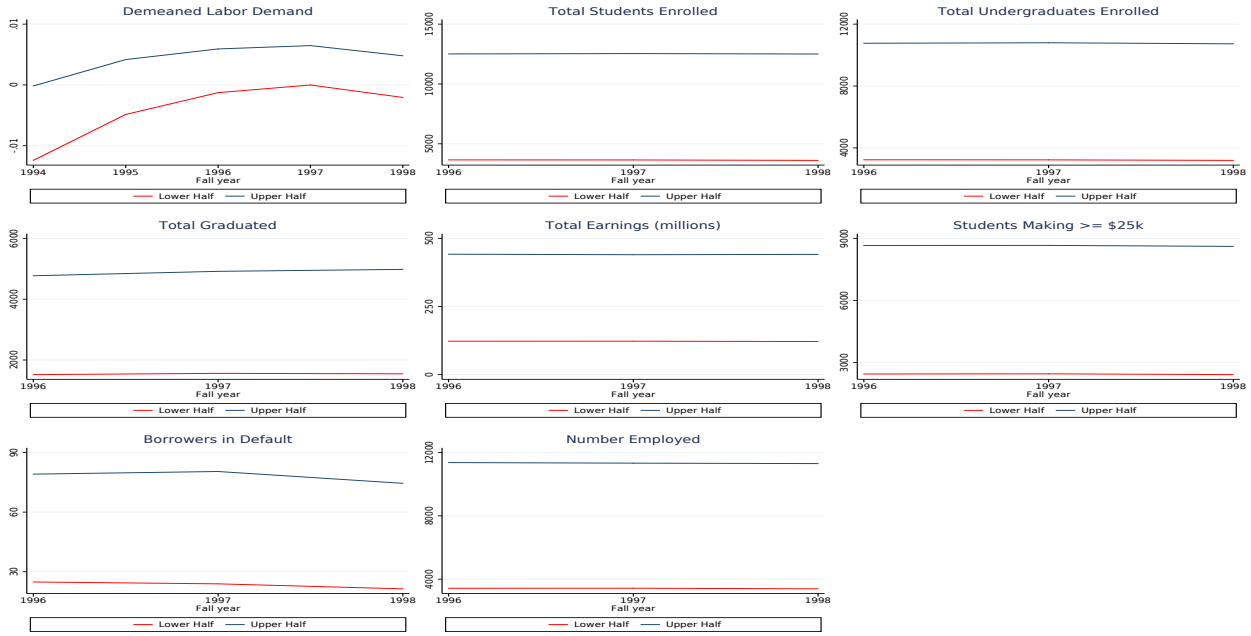
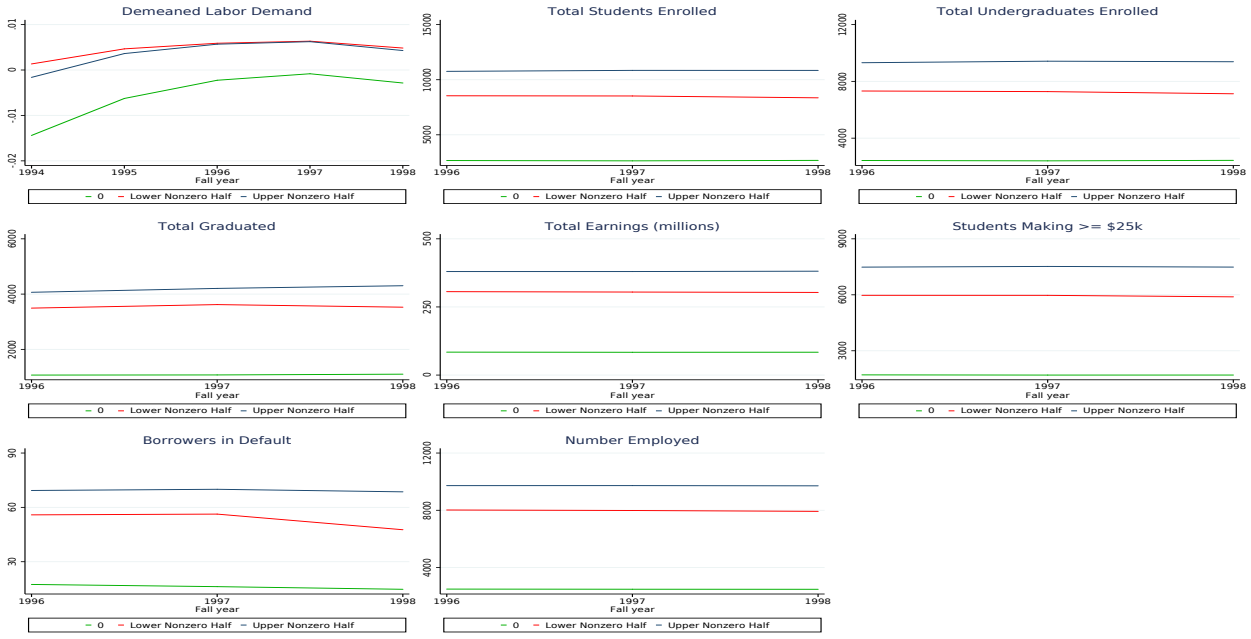


Figure A-3: Pretrends by For-Profit Supply, Three Groups



These charts consider demeaned labor demand trends, enrollment,and outcome variables for all schools together without splitting into 2-year and 4-year schools. In the upper graphs, the trendlines denote the level of each variable over time for CBSAs below and above the median in baseline supply distribution. In the lower graphs, the trendlines denote the level of each variable over time for CBSAs in 3 groups: the group with 0 supply, and below and above median groups in the non-zero baseline supply distribution.

Figure A-4: Pretrends by Labor Demand, Below and Above Median

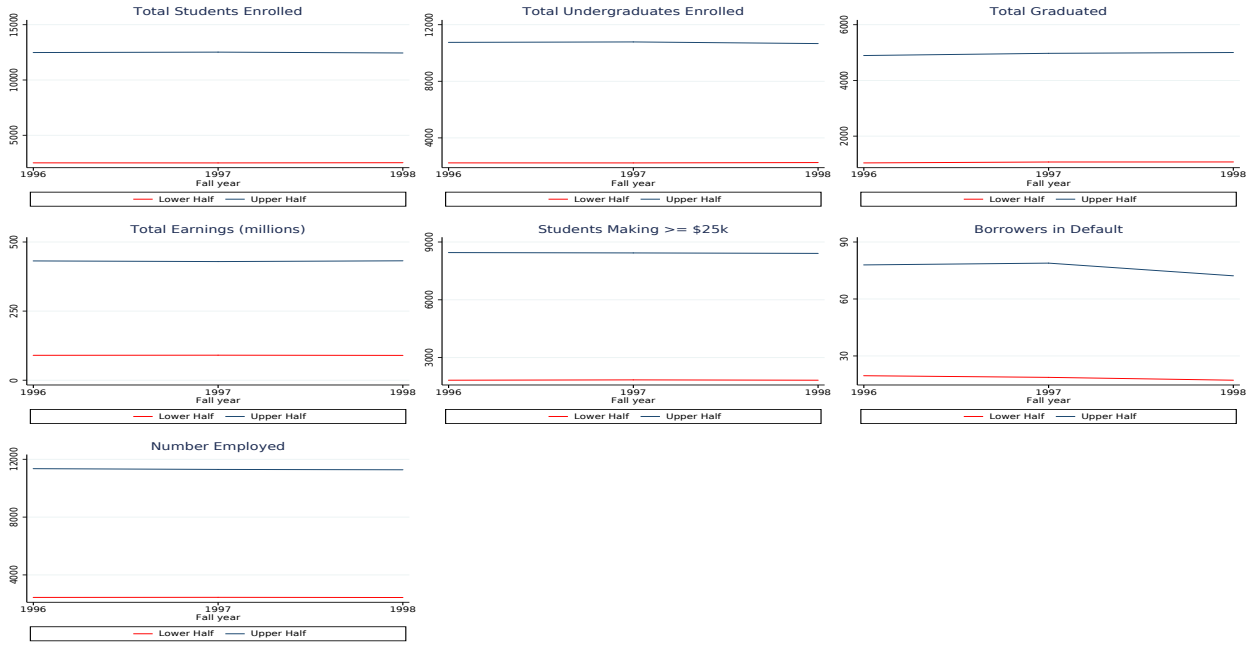
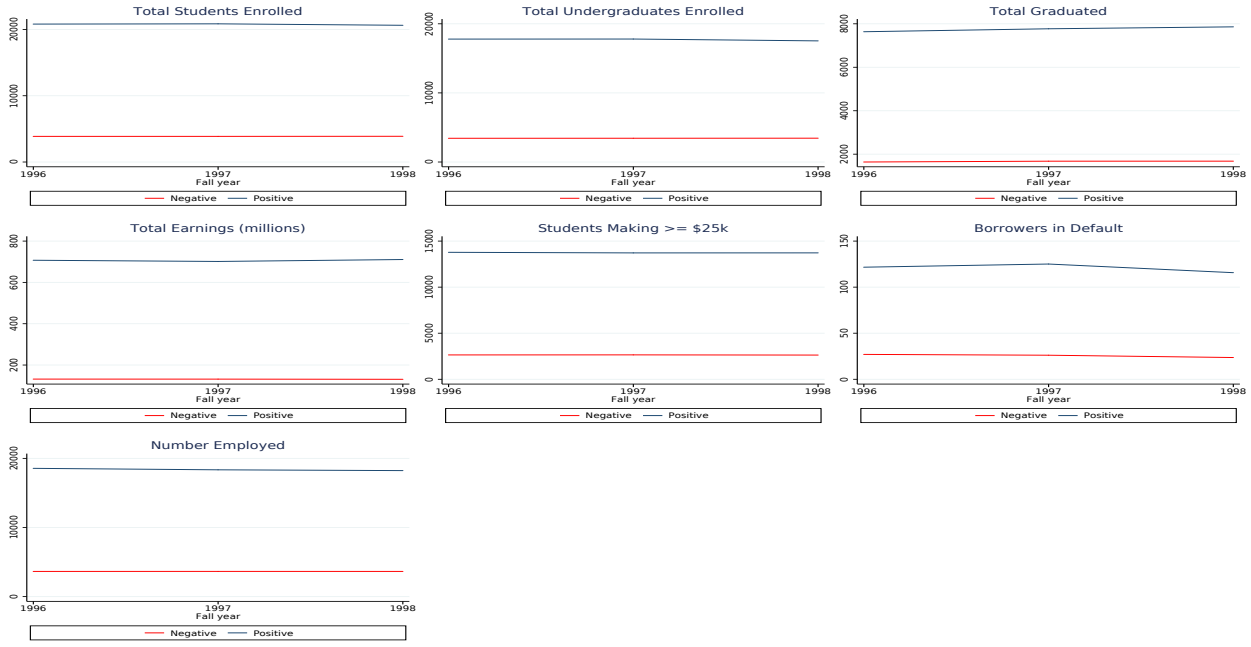


Figure A-5: Pretrends by Labor Demand, Below and Above Zero



These charts consider enrollment and outcome variables for all schools together without splitting into 2-year and 4-year schools. In the upper graphs, the trendlines denote the level of each variable over time for CBSAs below and above the median in labor demand as measured in 1998. In the lower graphs, the trendlines denote the level of each variable over time for CBSAs with negative and positive labor demand as measured in 1998.

Figure A-6: Pretrends by For-Profit SupplyxLabor Demand, Below and Above Median

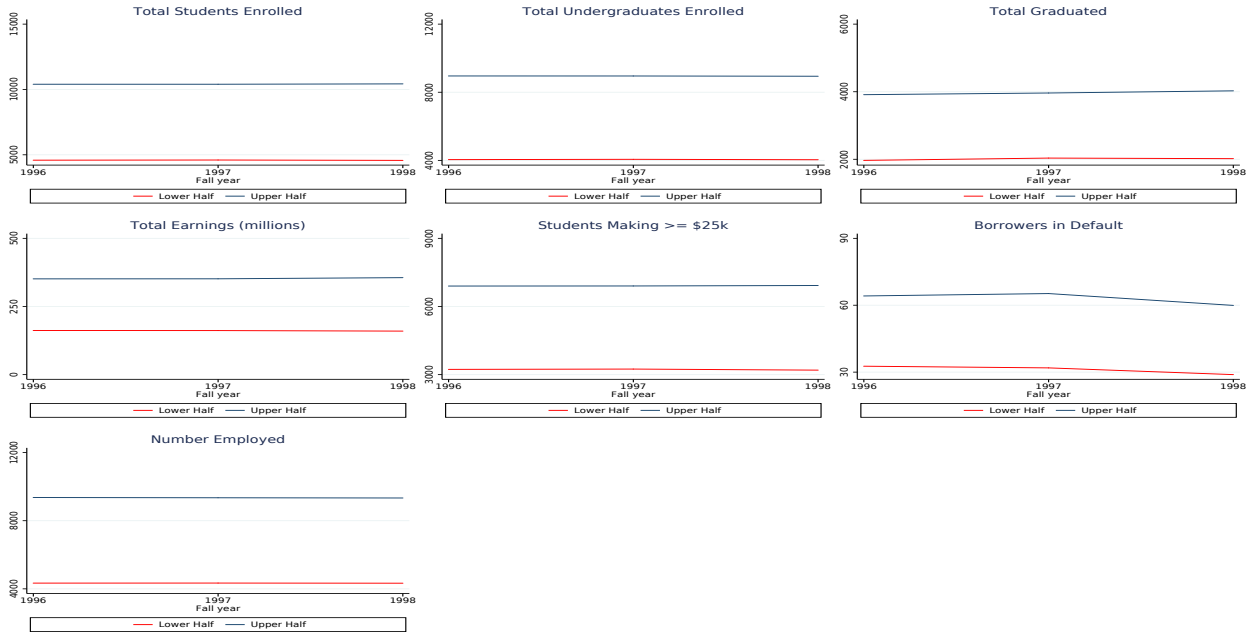
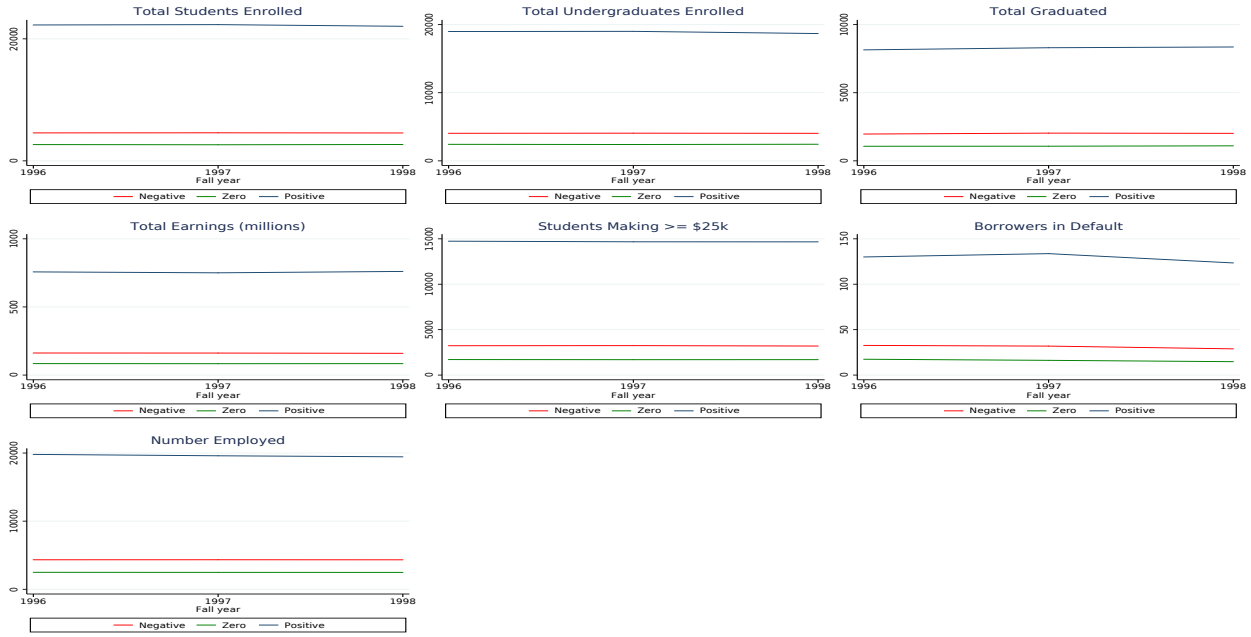


Figure A-7: Pretrends by For-Profit SupplyxLabor Demand, Below and Above Zero



These charts consider enrollment and outcome variables for all schools together without splitting into 2-year and 4-year schools. In the upper graphs, the trendlines denote the level of each variable over time for CBSAs below and above the median in labor demand interacted with for-profit supply as measured in 1998. In the lower graphs, the trendlines denote the level of each variable over time for CBSAs with negative and positive labor demand interacted with for-profit supply as measured in 1998.

Table A-1: Summary Statistics, 2000-2006 and 2008-2014

	2000-2006					2008-2014				
	All	Two-year For-profit	Two-year Public	Four-year For-profit	Four-year Public	All	Two-year For-profit	Two-year Public	Four-year For-profit	Four-year Public
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Total Enrollment	6156.20	1004.62	13135.73	910.34	9717.59	7463.46	1627.86	13931.81	2745.48	11595.84
	(25897)	(4313.05)	(44842.69)	(8071.36)	(23082.38)	(29950)	(6418.01)	(46875.13)	(22347.12)	(27822.57)
Undergraduate Enrollment	5537.19	1003.09	13135.01	720.22	7530.97	6745.65	1627.85	13931.59	2233.36	9343.82
	(24535)	(4308.58)	(44842.65)	(5956.14)	(17569.84)	(27833)	(6417.87)	(46875.15)	(17624.77)	(22410.84)
Number of (Subsidized) Direct Loan Recipients	357.55	119.41	130.69	40.77	1102.82	1454.54	696.37	1192.36	1017.18	2836.26
	(1773)	(469.96)	(555.95)	(505.34)	(3253.35)	(6299)	(2976.75)	(3439.84)	(9138.06)	(6888.77)
Number of (Unsubsidized) Direct Loan Recipients	250.27	89.01	80.53	32.04	773.51	1419.34	652.51	954.84	1087.32	2890.97
	(1232)	(358.41)	(370.15)	(416.00)	(2246.07)	(6419)	(2778.68)	(2787.82)	(9631.31)	(6968.40)
Number of (Subsidized) FFEL Loan Recipients	774.68	484.08	600.94	443.59	1533.65	1214.20	656.08	999.41	1290.08	1853.81
	(2945)	(2091.96)	(1335.46)	(3675.60)	(3654.71)	(6604)	(2598.81)	(2264.55)	(11750.59)	(4265.04)
Number of (Unsubsidized) FFEL Loan Recipients	579.85	388.63	406.06	398.86	1097.20	1083.87	598.78	779.88	1253.35	1644.84
	(2421)	(1668.14)	(987.50)	(3470.65)	(2580.92)	(6273)	(2349.93)	(1818.15)	(11351.09)	(3732.84)
Number of Federal Loans	2424.72	1351.33	1363.00	1201.74	5615.20	4828.85	2394.95	3234.38	4300.24	9085.81
	(8280)	(5486.91)	(2886.93)	(9166.75)	(11414.53)	(20293)	(9161.83)	(7495.65)	(32689.07)	(19016.57)
Total Loan Origination (in $1000s)	9282.51	4199.07	3346.37	5193.59	23525.96	21027.65	8389.54	9891.86	19180.00	44878.63
	(36460)	(17647.48)	(7305.95)	(42359.20)	(51988.76)	(95636)	(32384.01)	(23094.14)	(152050.00)	(99038.42)
Number of Borrowers in Default, 100% completion time	55.18	53.11	52.48	52.32	62.43					
	(413)	(231.14)	(112.05)	(758.99)	(134.78)					
Total students employed 6 years after enrolling	5305.94	749.43	10804.75	800.37	8942.68					
	(21530)	(3043.83)	(36068.29)	(7219.96)	(21009.97)					
Mean Earnings for non-enrolled students 6 years after enrolling	24371.16	15459.54	22969.88	30379.27	33276.77					
	(9047)	(7482.15)	(5084.39)	(7918.68)	(6124.14)					
Share earning $25k or more 6 years after enrolling	0.46	0.25	0.46	0.53	0.65					
	(0.21)	(0.18)	(0.14)	(0.21)	(0.11)					
Number of students graduating in 150% completion time	2193.87	573.44	3008.59	214.03	4916.17	1283.79	907.48	2788.27	123.72	1399.40
	(8888)	(2474.72)	(12172.25)	(1468.52)	(12037.52)	(6733)	(3668.47)	(10433.72)	(2099.62)	(7309.08)
3 year Bartik Shock	-1.31	-1.31	-1.31	-1.31	-1.31	-0.91	-0.91	-0.91	-0.91	-0.91
	(1.9)	(1.84)	(1.84)	(1.86)	(1.86)	(1.7)	(1.74)	(1.74)	(1.73)	(1.73)
Base employment in 1999-2000	133970.07	138340.32	138340.32	129942.89	129942.89	144730.65	149094.74	149094.74	140691.57	140691.57
	(444554)	(453387.13)	(453387.13)	(436277.75)	(436277.75)	(474920)	(483836.06)	(483836.06)	(466545.04)	(466545.04)
% For-Profit schools in 2000	14.70	27.83	27.83	2.60	2.60	14.82	28.11	28.11	2.53	2.53
	(25)	(29.12)	(29.12)	(10.35)	(10.35)	(25)	(29.14)	(29.14)	(9.91)	(9.91)
Observations	21344	5118	5118	5554	5554	21304	5120	5120	5532	5532

Means of variables reported in cells. Standard deviations in parentheses. Labor demand shocks are proxied by three-year or one-year Bartik shocks. Two-digit industry employment data from QCEW are used for computation of Bartik Shocks. Number of borrowers in default relate to imputed freshmen cohorts assuming 100% or 150% completion time, as labeled. Earnings, number working and number not working are measured six years after entering school. All years are indexed by spring year. Both total and undergraduate enrollment refer to twelve-month enrollment.

Table A-2: Summary Statistics: Student Composition (2000-2014)

	All	Two-year For-profit	Two-year Public	Four-year For-profit	Four-year Public
	(1)	(2)	(3)	(4)	(5)
% Undergraduates Black	18.72	22.66	13.72	21.52	13.52
	(22.42)	(25.31)	(16.61)	(19.91)	(20.81)
% Undergraduates Hispanic	12.30	15.69	8.90	12.24	7.56
	(18.42)	(21.83)	(14.10)	(14.43)	(12.45)
% Undergraduates White	56.77	50.99	66.35	44.20	65.35
	(29.22)	(31.39)	(24.37)	(24.26)	(25.06)
% Undergraduates Female	67.53	77.66	60.05	54.42	56.53
	(23.58)	(26.50)	(14.11)	(22.48)	(10.61)
% Undergraduates Over 25	43.60	46.50	44.15	59.83	24.42
	(19.38)	(18.10)	(14.26)	(20.84)	(15.83)
% Undergraduates Receiving Pell grant	53.08	62.12	40.08	60.99	36.09
	(22.45)	(21.19)	(18.55)	(18.16)	(14.03)
% Undergraduates Receiving Federal Loans	52.53	63.24	22.27	77.21	50.58
	(30.06)	(25.47)	(23.77)	(15.64)	(17.67)
% Undergraduates Sending FAFSA to 1 College	77.83	85.18	75.64	79.45	57.89
	(14.86)	(12.71)	(10.56)	(12.57)	(10.55)
% Undergraduates Sending FAFSA to 2 Colleges	14.59	11.85	15.80	14.12	21.09
	(9.98)	(11.54)	(7.09)	(10.23)	(5.42)
% Undergraduates Sending FAFSA to 3 Colleges	3.31	1.59	4.02	2.57	7.70
	(3.29)	(2.77)	(2.38)	(1.93)	(2.43)
% Undergraduates Sending FAFSA to 4 Colleges	1.59	0.53	1.84	1.30	4.67
	(1.95)	(0.84)	(1.37)	(1.68)	(2.34)
% Undergraduates Sending FAFSA to 5 Colleges	2.66	0.82	2.73	2.56	8.64
	(4.01)	(1.22)	(2.37)	(2.81)	(6.75)
% Parents with HS Diploma	37.25	37.49	45.56	16.47	33.11
	(22.60)	(23.21)	(20.03)	(18.24)	(17.87)
% Parents with MS	5.37	6.05	6.62	2.12	2.85
	(5.96)	(7.25)	(4.81)	(2.96)	(2.84)
% Parents with College Degree	37.96	33.17	42.52	20.03	55.85
	(25.45)	(21.91)	(15.60)	(23.07)	(38.08)
Average Family Income	34036.52	29122.06	28513.67	43749.69	58420.80
	(16888.90)	(13070.48)	(10424.15)	(12694.77)	(18409.70)
Average SAT score	1023.82	974.89	901.92	996.65	1030.66
	(104.35)	(86.59)	(62.98)	(96.67)	(102.30)
% Arts degrees	27.64	9.69	39.17	40.20	58.00
	(31.21)	(23.74)	(25.81)	(35.22)	(21.28)
% STEM degrees	37.01	35.32	40.53	41.73	32.14
	(35.21)	(42.95)	(24.54)	(35.33)	(19.45)
% Vocational degrees	33.14	52.98	19.99	9.53	7.23
	(40.11)	(47.69)	(18.69)	(16.17)	(7.46)
% Associates Degrees	54.84	54.46	65.06	51.14	23.09
	(32.53)	(36.66)	(24.59)	(31.49)	(28.68)
% Bachelors Degrees	54.27	1.34	1.02	35.01	67.63
	(29.72)	(4.43)	(2.95)	(28.83)	(21.59)
% Certificates	70.69	90.41	53.18	24.30	6.67
	(37.40)	(24.19)	(34.88)	(26.17)	(10.80)
Observations	74883	36097	21284	7733	9769

Means of variables reported in cells. Standard deviations in parentheses.
All years are indexed by spring year. Fraction of non-missing observations are in brackets.

Table A-3: First Stage Enrollment Estimates, Excluding Selective Institutions

Panel A: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	52.365***	50.121***	64.112***	62.729***	31.892***	31.260***
	(11.861)	(12.069)	(13.767)	(14.024)	(8.4242)	(8.5923)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-97.477***	-97.477***	-129.32***	-129.32***	-65.317***	-65.317***
	(21.037)	(21.383)	(25.915)	(26.304)	(16.361)	(16.607)
	[-4.6337]	[-4.5587]	[-4.9902]	[-4.9165]	[-3.9921]	[-3.9330]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	21396	21396	10046	10046	10018	10018
R-squared	0.1549	0.1574	0.1634	0.1648	0.1351	0.1363
P-value [1]	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001
Panel B: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	55.952***	53.708***	68.177***	66.794***	28.203***	27.571***
	(12.978)	(13.172)	(14.685)	(15.018)	(8.7081)	(8.8494)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-104.65***	-104.65***	-137.45***	-137.45***	-57.939***	-57.939***
	(22.770)	(23.145)	(27.612)	(28.029)	(16.780)	(17.034)
	[-4.5960]	[-4.5214]	[-4.9780]	[-4.9040]	[-3.4528]	[-3.4013]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21396	21396	10046	10046	10018	10018
R-squared	0.2584	0.2610	0.2707	0.2721	0.2322	0.2334
P-value [1]	0.0000	0.0000	0.0000	0.0000	0.0007	0.0005
Panel C: 4 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	9.0311	23.566	34.854	41.339	86.051***	87.915***
	(40.720)	(38.838)	(32.889)	(31.989)	(23.431)	(23.523)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-187.28***	-187.28***	-131.66***	-131.66**	-271.69***	-271.69***
	(59.415)	(60.314)	(50.677)	(51.375)	(69.395)	(70.360)
	[-3.1521]	[-3.1052]	[-2.5980]	[-2.5627]	[-3.9152]	[-3.8614]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	23212	23212	10918	10918	10814	10814
R-squared	0.1816	0.1908	0.2324	0.2350	0.1344	0.1395
P-value [1]	0.0055	0.0066	0.0034	0.0067	0.0005	0.0005
Panel D: 4 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	18.959	33.494	47.390	53.875*	89.491***	91.356***
	(39.676)	(40.819)	(29.084)	(28.559)	(20.159)	(21.172)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-207.14***	-207.14***	-156.73***	-156.73***	-278.57***	-278.57***
	(71.009)	(72.087)	(44.962)	(45.584)	(61.769)	(62.633)
	[-2.9171]	[-2.8735]	[-3.4858]	[-3.4382]	[-4.5099]	[-4.4477]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23212	23212	10918	10918	10814	10814
R-squared	0.2332	0.2424	0.2880	0.2907	0.2062	0.2114
P-value [1]	0.0122	0.0128	0.0004	0.0007	0.0000	0.0000

This table uses public and for-profit institutions, excluding all institutions with a rating of "More Selective" or higher in the 2001 Barron's rankings. The dependent variable is 12-month enrollment aggregated to CBSA, sector (public or for-profit) and level (two-year or four-year). *, **, ***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two- digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is percentage of for-profit institutions at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000 or 2006-2007 (for 2008-14 phase). All regressions include the following CBSA level variables as controls: base-year employment, gender composition (% female), racial composition (% black, %Hispanic, %American Indian, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty and total population. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-4: Instrumental Variables Estimates Impact of For-profit Attendance on Borrowing, by Loan Type Excluding Selective Institutions

	Panel A: Number Recipients: Direct Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.007316	0.008577	-0.04379	-0.03988	0.2937	0.2670	0.2997	0.2674
	(0.1224)	(0.1182)	(0.2331)	(0.2210)	(0.2105)	(0.2289)	(0.2406)	(0.2594)
Enroll*For-profit	0.1615	0.1552	-0.1421	-0.1518	0.7067*	0.7463*	0.7783*	0.8189*
	(0.7214)	(0.7413)	(1.3932)	(1.4133)	(0.3940)	(0.4172)	(0.4134)	(0.4491)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21396	21396	21396	21396	23212	23212	23212	23212
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02009	0.02111	-0.02286	-0.01963	0.2608*	0.2482	0.2684	0.2487
	(0.1056)	(0.1018)	(0.1989)	(0.1885)	(0.1572)	(0.1616)	(0.1916)	(0.2005)
Enroll*For-profit	0.2430	0.2379	-0.01212	-0.02019	0.5921*	0.6109*	0.6819**	0.7066*
	(0.6175)	(0.6354)	(1.1853)	(1.2028)	(0.3052)	(0.3251)	(0.3415)	(0.3724)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21396	21396	21396	21396	23212	23212	23212	23212
	Panel C: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.02814	-0.02677	-0.09977	-0.09712	0.1566**	0.1671***	0.1799**	0.1866**
	(0.03352)	(0.03293)	(0.1019)	(0.09878)	(0.06930)	(0.06006)	(0.08733)	(0.07462)
Enroll*For-profit	0.002700	-0.004007	-0.4720	-0.4819	0.4629***	0.4496***	0.5031***	0.4959***
	(0.2146)	(0.2169)	(0.6769)	(0.6823)	(0.1245)	(0.1238)	(0.1512)	(0.1550)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15748	15748	15748	15748	17092	17092	17092	17092
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.004660	-0.002942	-0.04749	-0.04489	0.09651*	0.1106***	0.1140*	0.1256**
	(0.02419)	(0.02386)	(0.06113)	(0.05920)	(0.04951)	(0.04110)	(0.06084)	(0.05102)
Enroll*For-profit	0.1580	0.1495	-0.1257	-0.1354	0.4121***	0.3942***	0.4423***	0.4299***
	(0.1531)	(0.1543)	(0.3981)	(0.4000)	(0.07953)	(0.08182)	(0.09685)	(0.1013)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15748	15748	15748	15748	17092	17092	17092	17092

This table uses public and for-profit institutions, excluding all institutions with a rating of "More Selective" or higher in the 2001 Barron's rankings. The enrollment measure is 12-month enrollment. Enrollment and outcomes are aggregated to CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-5: Instrumental Variables Estimates of the Impact of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults, Excluding Selective Institutions

	Panel A: Number of Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.06250	0.06604	-0.001501	0.005904	0.4300**	0.4854***	0.4391*	0.4861**
	(0.1279)	(0.1233)	(0.2450)	(0.2316)	(0.1983)	(0.1432)	(0.2414)	(0.1963)
Enroll*For-profit	1.1405	1.1229	0.7604	0.7419	1.1269***	1.0445***	1.2359***	1.1767***
	(0.7292)	(0.7486)	(1.4379)	(1.4545)	(0.4054)	(0.3473)	(0.4395)	(0.4003)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21396	21396	21396	21396	23212	23212	23212	23212
	Panel B: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	505.63	504.58	557.61	553.93	1449.1***	1940.9***	1496.6**	1945.0***
	(380.45)	(366.60)	(615.01)	(586.47)	(537.31)	(379.30)	(695.77)	(349.00)
Enroll*For-profit	5874.1***	5879.3***	6182.8*	6192.0*	3319.0**	2587.5*	3884.3**	3319.8**
	(2179.2)	(2252.0)	(3619.1)	(3694.5)	(1506.3)	(1350.5)	(1585.7)	(1332.1)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21396	21396	21396	21396	23212	23212	23212	23212
	Panel C: Number of Borrowers in Default							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02581	0.02581	0.04433	0.04415	0.06746*	0.05955*	0.06092	0.05306*
	(0.01624)	(0.01600)	(0.07125)	(0.06998)	(0.04055)	(0.03288)	(0.03910)	(0.03074)
Enroll*For-profit	0.2133*	0.2133*	0.3437	0.3441	0.1177**	0.1294**	0.1115**	0.1195**
	(0.1175)	(0.1187)	(0.5098)	(0.5115)	(0.04736)	(0.05560)	(0.05038)	(0.05680)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14320	14320	14320	14320	15171	15171	15171	15171

This table uses public and for-profit institutions, excluding all institutions with a rating of "More Selective" or higher in the 2001 Barron's rankings. The enrollment measure is 12-month enrollment. Enrollment and outcomes are aggregated to CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-6: Instrumental Variables Estimates of the Impact of For-profit Attendance on Graduation, Employment, and Earnings, Excluding Selective Institutions

	Panel A: Total Graduated, assuming 150% Completion Time			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.1728	0.1781	0.4268***	0.4054***
	(0.1163)	(0.1108)	(0.1054)	(0.09651)
Enroll*For-profit	0.2949	0.2779	-0.09365	-0.07138
	(0.7857)	(0.7970)	(0.3153)	(0.3302)
State*FP & Year*FP	No	Yes	No	Yes
Observations	17176	17176	18638	18638
	Panel B: Number Employed, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.7939***	0.7939***	0.8664***	0.8835***
	(0.1037)	(0.1036)	(0.02785)	(0.02083)
Enroll*For-profit	-0.4053	-0.4015	-0.08673*	-0.1053*
	(0.8159)	(0.8205)	(0.05167)	(0.05838)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8602	8602	9344	9344
	Panel C: Total Earnings, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	25858.0***	25845.1***	36936.1***	37895.3***
	(6748.4)	(6808.0)	(3523.6)	(2863.5)
Enroll*For-profit	-9879.4	-8659.3	-5529.8	-6569.1
	(53313.5)	(53987.0)	(7409.3)	(7994.5)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8602	8602	9344	9344
	Panel D: Total Students Making $25k, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.6238***	0.6234***	0.7158***	0.7275***
	(0.2122)	(0.2157)	(0.05412)	(0.04356)
Enroll*For-profit	0.3851	0.4258	-0.07250	-0.08513
	(1.6444)	(1.6694)	(0.1201)	(0.1325)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8602	8602	9344	9344

This table uses public and for-profit institutions, excluding all institutions with a rating of "More Selective" or higher in the 2001 Barron's rankings. The enrollment measure is 12-month enrollment. Enrollment and outcomes are aggregated to CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and state-year fixed effects.Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-7: First Stage Enrollment Estimates, Using 12-month Undergraduate Enrollment

Panel A: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	52.308***	50.079***	64.040***	62.663***	31.872***	31.234***
	(11.856)	(12.058)	(13.751)	(14.002)	(8.4252)	(8.5909)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-97.379***	-97.379***	-129.24***	-129.24***	-65.285***	-65.285***
	(21.029)	(21.370)	(25.898)	(26.281)	(16.359)	(16.603)
	[-4.6308]	[-4.5567]	[-4.9904]	[-4.9176]	[-3.9906]	[-3.9322]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	21666	21666	10172	10172	10130	10130
R-squared	0.1548	0.1574	0.1634	0.1647	0.1351	0.1363
P-value [1]	0.0000	0.0000	0.0000	0.0000	0.0001	0.0001
Panel B: 2 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	55.888***	53.660***	68.008***	66.631***	28.157***	27.519***
	(12.964)	(13.152)	(14.655)	(14.981)	(8.6978)	(8.8362)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-104.54***	-104.54***	-137.18***	-137.18***	-57.855***	-57.855***
	(22.744)	(23.114)	(27.563)	(27.973)	(16.756)	(17.006)
	[-4.5965]	[-4.5228]	[-4.9769]	[-4.9039]	[-3.4528]	[-3.4020]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21666	21666	10172	10172	10130	10130
R-squared	0.2582	0.2607	0.2705	0.2719	0.2320	0.2332
P-value [1]	0.0000	0.0000	0.0000	0.0000	0.0007	0.0005
Panel C: 4 year schools	2000-2014		2000-2006		2008-2014	
	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	-4.9699	6.9710	30.796	35.778	73.436***	74.788***
	(39.716)	(36.958)	(28.405)	(27.481)	(22.266)	(22.013)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-161.60***	-161.60***	-113.32***	-113.32***	-254.15***	-254.15***
	(46.267)	(46.959)	(39.400)	(39.937)	(63.841)	(64.717)
	[-3.4928]	[-3.4413]	[-2.8761]	[-2.8374]	[-3.9810]	[-3.9271]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	No	No	No	No
Observations	23482	23482	11044	11044	10954	10954
R-squared	0.1868	0.1979	0.2402	0.2431	0.1399	0.1462
P-value [1]	0.0019	0.0024	0.0023	0.0038	0.0003	0.0005
Panel D: 4 year schools	2000-2014		2000-2006		2008-2014	
3 year rolling shocks	(1)	(2)	(3)	(4)	(5)	(6)
(2000 For-profit Supply)*$\hat{\eta}$	1.4269	13.368	40.260	45.241*	76.622***	77.975***
	(37.595)	(36.583)	(26.317)	(25.486)	(17.834)	(18.435)
(For-profit)*(2000 Supply)*$\hat{\eta}$	-174.40***	-174.40***	-132.24***	-132.24***	-260.52***	-260.52***
	(56.169)	(57.012)	(36.760)	(37.263)	(54.120)	(54.866)
	[-3.1048]	[-3.0590]	[-3.5975]	[-3.5490]	[-4.8139]	[-4.7483]
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year
State*FP & Year*FP	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23482	23482	11044	11044	10954	10954
R-squared	0.2360	0.2471	0.2937	0.2965	0.2136	0.2198
P-value [1]	0.0074	0.0083	0.0004	0.0006	0.0000	0.0000

This table uses public and for-profit institutions. The dependent variable is 12-month undergraduate enrollment aggregated to CBSA, sector (public or for-profit) and level (two-year or four-year). *, **, ***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two- digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is percentage of for-profit institutions at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000 or 2006-2007 (for 2008-14 phase). All regressions include the following CBSA level variables as controls: base-year employment, gender composition (% female), racial composition (% black, %Hispanic, %American Indian, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty and total population. All regressions also include geography and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-8: Instrumental Variables Estimates Impact of For-profit Attendance on Outcomes, Using 12-month Undergraduate Enrollment

	Panel A: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.02749	-0.02609	-0.09403	-0.09134	0.1862**	0.2069***	0.2120**	0.2307***
	(0.03352)	(0.03291)	(0.09684)	(0.09376)	(0.07888)	(0.06743)	(0.10000)	(0.08543)
Enroll*For-profit	0.006973	-0.000001231	-0.4349	-0.4451	0.5590***	0.5333***	0.5965***	0.5767***
	(0.2150)	(0.2175)	(0.6441)	(0.6497)	(0.1275)	(0.1216)	(0.1500)	(0.1487)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.003884	-0.002186	-0.04285	-0.04029	0.1128**	0.1347***	0.1324*	0.1532***
	(0.02413)	(0.02380)	(0.05759)	(0.05577)	(0.05593)	(0.04679)	(0.06949)	(0.05886)
Enroll*For-profit	0.1635	0.1550	-0.09530	-0.1049	0.5093***	0.4819***	0.5378***	0.5156***
	(0.1531)	(0.1544)	(0.3751)	(0.3772)	(0.07955)	(0.07835)	(0.09300)	(0.09456)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel C: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	513.46	511.76	605.09	598.68	1632.6***	2261.6***	1731.8***	2342.1***
	(380.36)	(366.08)	(611.95)	(582.35)	(555.01)	(613.42)	(613.41)	(425.38)
Enroll*For-profit	5930.2***	5938.7***	6475.3*	6491.4*	3586.1**	2645.1*	4126.0***	3355.8***
	(2183.5)	(2259.5)	(3618.3)	(3697.6)	(1479.3)	(1457.4)	(1425.6)	(1245.9)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel D: Number Employed, 6 years after enrollment							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.6110	0.6094	0.7972***	0.7972***	0.8447***	0.8633***	0.8444***	0.8637***
	(0.9896)	(1.0078)	(0.09585)	(0.09575)	(0.02930)	(0.02383)	(0.03234)	(0.02600)
Enroll*For-profit	-1.8264	-1.8501	-0.3800	-0.3778	-0.1107**	-0.1317**	-0.1112**	-0.1309**
	(7.6234)	(7.8149)	(0.7591)	(0.7638)	(0.04571)	(0.05224)	(0.05062)	(0.05549)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8710	8710	8710	8710	9452	9452	9452	9452

This table uses public and for-profit institutions. The enrollment measure is 12-month undergraduate enrollment. Enrollment and outcomes are aggregated to CBSA, institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-9: Instrumental Variables Estimates Including Current-year Demand Shocks

	Panel A: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.03003	-0.009840	0.02104	-0.1038	0.1083***	0.1269***	0.1134***	0.1317***
	(0.02947)	(0.03024)	(0.03783)	(0.1006)	(0.03668)	(0.02535)	(0.03816)	(0.02628)
Enroll*For-profit	0.3850**	0.1063	0.3249	-0.5326	0.3936***	0.3709***	0.3986***	0.3774***
	(0.1916)	(0.1951)	(0.2516)	(0.6936)	(0.03863)	(0.03146)	(0.03723)	(0.02932)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15924	15924	15924	15924	17254	17254	17254	17254
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02878	0.009208	0.01877	-0.05029	0.07480***	0.09470***	0.07914***	0.09908***
	(0.02427)	(0.02313)	(0.03075)	(0.06110)	(0.02741)	(0.01975)	(0.02824)	(0.02019)
Enroll*For-profit	0.3780**	0.2291	0.3112	-0.1753	0.3868***	0.3705***	0.3915***	0.3769***
	(0.1635)	(0.1501)	(0.2068)	(0.4130)	(0.03117)	(0.02605)	(0.02930)	(0.02392)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15924	15924	15924	15924	17254	17254	17254	17254
	Panel C: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-317.45	-109.09	-763.40	-544.73	1821.4**	2155.3***	1925.9**	2234.4***
	(552.25)	(503.17)	(822.39)	(790.06)	(786.17)	(438.31)	(936.85)	(604.01)
Enroll*For-profit	1154.3	2149.9	-1513.1	-632.44	4624.1***	4172.8***	5092.6***	4726.9***
	(3283.6)	(3158.1)	(4920.2)	(5074.4)	(1621.0)	(1503.7)	(1613.1)	(1416.1)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	20198	20198	20198	20198	21882	21882	21882	21882
	Panel D: Number Employed, 6 years after enrollment							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.8052***	0.7983***	0.8057***	0.7990***	0.8692***	0.8786***	0.8726***	0.8805***
	(0.02222)	(0.02131)	(0.02192)	(0.02096)	(0.02315)	(0.01666)	(0.02529)	(0.01897)
Enroll*For-profit	-0.3163*	-0.3716**	-0.3126*	-0.3665**	-0.07156	-0.1042**	-0.06474	-0.09939*
	(0.1801)	(0.1731)	(0.1780)	(0.1705)	(0.04551)	(0.04687)	(0.04951)	(0.05105)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8696	8696	8696	8696	9430	9430	9430	9430

This table uses public and for-profit institutions. We include current-year (year t) labor demand change as well as all interactions among this labor demand change, the for-profit indicator, and for-profit supply. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit) and level (two-year or four-year). Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include CBSA and year or state-year fixed effects. Two year estimates include two-year and less than two year institutions and exclude the 65 CBSAs that only have a for-profit college in the base year.

Table A-10: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Student Borrowing, by Loan Type Including Baseline Degree Composition X Year Controls

	Panel A: Number Recipients: Direct Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.01080	0.006201	-0.02433	-0.05554	0.3429**	0.3294**	0.3535**	0.3379*
	(0.1214)	(0.1553)	(0.2091)	(0.3708)	(0.1538)	(0.1579)	(0.1768)	(0.1817)
Enroll*For-profit	0.1831	0.1391	-0.02459	-0.2577	0.6128**	0.6344**	0.6729**	0.6958**
	(0.7142)	(0.9945)	(1.2439)	(2.4086)	(0.2762)	(0.2754)	(0.2821)	(0.2902)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.01972	0.02042	-0.01285	-0.03085	0.2971**	0.2903**	0.3100**	0.3006**
	(0.1064)	(0.1337)	(0.1840)	(0.3156)	(0.1224)	(0.1163)	(0.1436)	(0.1414)
Enroll*For-profit	0.2406	0.2334	0.04777	-0.09624	0.5186**	0.5318**	0.5904**	0.6076**
	(0.6230)	(0.8531)	(1.0926)	(2.0467)	(0.2148)	(0.2201)	(0.2337)	(0.2442)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel C: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.01173	-0.01179	-0.06506	-0.1051	0.1546**	0.1651**	0.1741**	0.1840**
	(0.03129)	(0.04362)	(0.07005)	(0.1632)	(0.06935)	(0.06477)	(0.08401)	(0.07790)
Enroll*For-profit	0.1154	0.1026	-0.2374	-0.5413	0.4568***	0.4440***	0.4854***	0.4762***
	(0.1958)	(0.2868)	(0.4576)	(1.1338)	(0.1167)	(0.1112)	(0.1347)	(0.1322)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.007358	0.01454	-0.02284	-0.03176	0.09482*	0.1060**	0.1090*	0.1200**
	(0.02350)	(0.03360)	(0.04373)	(0.08454)	(0.05046)	(0.04591)	(0.05982)	(0.05488)
Enroll*For-profit	0.2406	0.2747	0.04075	-0.04465	0.4142***	0.4007***	0.4349***	0.4246***
	(0.1494)	(0.2249)	(0.2796)	(0.5751)	(0.07362)	(0.07219)	(0.08527)	(0.08549)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Results for one-year Bartik shocks are not presented in tables, but are qualitavely similar. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions. In addition, all regressions pertaining to 2-year schools include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all regressions pertaining to 4-year schools include %Bachelors Degrees awarded in the baseline year interacted with year dummies.

Table A-11: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults, Including Baseline Degree Composition X Year Controls

	Panel A: Number of Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.1028	0.1243	0.08324	0.1199	0.4412***	0.4684***	0.4527***	0.4774***
	(0.1195)	(0.1468)	(0.1965)	(0.2997)	(0.1376)	(0.1117)	(0.1573)	(0.1375)
Enroll*For-profit	1.3911**	1.5178*	1.2760	1.4912	1.0165***	0.9830***	1.0777***	1.0559***
	(0.6739)	(0.9092)	(1.1359)	(1.9013)	(0.2651)	(0.2519)	(0.2762)	(0.2668)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel B: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	617.12	759.67	782.95	1211.3	1438.8***	1700.7***	1502.7***	1754.2***
	(376.36)	(480.96)	(616.15)	(1279.1)	(411.06)	(381.13)	(432.22)	(320.68)
Enroll*For-profit	6566.7***	7609.7**	7552.1**	10524.9	2954.2**	2631.7**	3291.3***	3065.0***
	(2198.6)	(3164.2)	(3720.7)	(8599.3)	(1175.6)	(1151.9)	(1107.6)	(1050.1)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel C: Number of Borrowers in Default (100%)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02552*	0.04436	0.03975	0.2012	0.06572*	0.06169**	0.05957*	0.05548*
	(0.01371)	(0.04165)	(0.04682)	(1.2747)	(0.03626)	(0.03065)	(0.03560)	(0.02980)
Enroll*For-profit	0.2113**	0.3504	0.3112	1.4850	0.1091***	0.1140***	0.1036***	0.1062***
	(0.1008)	(0.3136)	(0.3378)	(9.2899)	(0.03749)	(0.03851)	(0.03939)	(0.04022)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14500	14500	14500	14500	15347	15347	15347	15347

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Results for one-year Bartik shocks are not presented in tables, but are qualitavely similar. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions. In addition, all regressions pertaining to 2-year schools include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all regressions pertaining to 4-year schools include %Bachelors Degrees awarded in the baseline year interacted with year dummies.

Table A-12: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Employment, Earnings, and Graduation, Including Baseline Degree Composition X Year Controls

	Panel A: Total Graduated, assuming 150% Completion Time			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.1639	0.1436	0.4178***	0.4169***
	(0.1190)	(0.1607)	(0.1117)	(0.1060)
Enroll*For-profit	0.2362	0.03428	-0.09888	-0.1066
	(0.8159)	(1.1712)	(0.2759)	(0.2791)
State*FP & Year*FP	No	Yes	No	Yes
Observations	17392	17392	18854	18854
	Panel B: Number Employed, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.8027***	0.8029***	0.8583***	0.8724***
	(0.09573)	(0.1016)	(0.02969)	(0.02374)
Enroll*For-profit	-0.3359	-0.3290	-0.09562**	-0.1098**
	(0.7529)	(0.8090)	(0.04551)	(0.04791)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452
	Panel C: Total Earnings, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	25876.8***	25965.8***	36024.9***	37247.4***
	(6390.9)	(6899.9)	(3392.3)	(3014.9)
Enroll*For-profit	-9741.6	-7630.6	-4976.5	-6182.4
	(50441.0)	(55001.5)	(6280.9)	(6600.2)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452
	Panel D: Total Students Making $25k, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.6201***	0.6261***	0.7022***	0.7187***
	(0.1971)	(0.2211)	(0.05098)	(0.04541)
Enroll*For-profit	0.3548	0.4521	-0.06994	-0.08637
	(1.5260)	(1.7202)	(0.1001)	(0.1082)
State*FP & Year*FP	No	Yes	No	Yes
Observations	8710	8710	9452	9452

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Results for one-year Bartik shocks are not presented in tables, but are qualitavely similar. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and state-by-year fixed effects. Two year institution group includes two-year and less than two year institutions. In addition, all regressions pertaining to 2-year schools include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all regressions pertaining to 4-year schools include %Bachelors Degrees awarded in the baseline year interacted with year dummies.

Table A-13: Instrumental Variables Estimates of the Impact of For-profit Attendance on Outcomes, Fixing Institution Types in the Base Year

	Panel A: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.02765	-0.02661	-0.04140	-0.04011	0.1692**	0.1775***	0.1810**	0.1869***
	(0.03353)	(0.03317)	(0.03768)	(0.03714)	(0.07467)	(0.06311)	(0.07401)	(0.06208)
Enroll*For-profit	0.04108	0.03724	-0.04253	-0.04634	0.4303***	0.4220***	0.4439***	0.4380***
	(0.1931)	(0.1947)	(0.2186)	(0.2202)	(0.09574)	(0.1016)	(0.09590)	(0.1015)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.003265	-0.001755	-0.01224	-0.01050	0.09462*	0.1101**	0.1044*	0.1179***
	(0.02604)	(0.02569)	(0.02880)	(0.02835)	(0.05664)	(0.04530)	(0.05576)	(0.04496)
Enroll*For-profit	0.1854	0.1799	0.1309	0.1258	0.3963***	0.3808***	0.4076***	0.3940***
	(0.1528)	(0.1544)	(0.1685)	(0.1700)	(0.05713)	(0.06354)	(0.05739)	(0.06308)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15946	15946	15946	17290	17290	17290	17290
	Panel C: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-281.01	-257.69	8.8964	23.101	1397.6***	1834.0***	1476.3***	1883.6***
	(1468.9)	(1413.4)	(1028.2)	(983.46)	(498.39)	(412.31)	(503.70)	(332.80)
Enroll*For-profit	1489.6	1440.0	3137.1	3120.3	2760.0**	2351.1**	3083.2***	2694.9***
	(8376.4)	(8481.9)	(5847.8)	(5884.1)	(1185.5)	(1139.7)	(1048.6)	(956.01)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21666	21666	21666	23482	23482	23482	23482
	Panel D: Number Employed, 6 years after enrollment							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.7349***	0.7348***	0.7800***	0.7799***	0.8670***	0.8819***	0.8694***	0.8839***
	(0.2593)	(0.2591)	(0.1904)	(0.1901)	(0.02897)	(0.02024)	(0.03045)	(0.02194)
Enroll*For-profit	-0.8056	-0.8040	-0.4766	-0.4743	-0.1007***	-0.1181***	-0.09665***	-0.1133***
	(1.8886)	(1.8879)	(1.3994)	(1.4002)	(0.03172)	(0.03348)	(0.03526)	(0.03789)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8710	8710	8710	8710	9452	9452	9452	9452

This table uses public and for-profit institutions, classifying the 5% of schools that switch from two-year to four-year during the period of our analysis as two-year schools. The enrollment measure is 12-month undergraduate enrollment. Robust standard errors adjusted for clustering by CBSA are in parentheses: *,**,*** indicates significance at the 10, 5, and 1 percent level, respectively. All regressions include CBSA level variables as described in the text, CBSA and year or state-year fixed effects.

Table A-14: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Outcomes, Excluding Online Institutions

	Panel A: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.02724	-0.02862	-0.09541	-0.09279	0.2158**	0.2245**	0.2328**	0.2436**
	(0.03338)	(0.03423)	(0.09762)	(0.09466)	(0.1038)	(0.09432)	(0.1136)	(0.1058)
Enroll*For-profit	0.008026	-0.01458	-0.4403	-0.4504	0.6980**	0.6704**	0.7453**	0.7320**
	(0.2138)	(0.2263)	(0.6481)	(0.6537)	(0.3124)	(0.3052)	(0.3291)	(0.3326)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15792	15792	15792	17268	17062	17062	17062
	Panel B: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	-0.003745	-0.004180	-0.04491	-0.04243	0.1471**	0.1581**	0.1618**	0.1734**
	(0.02409)	(0.02466)	(0.05892)	(0.05714)	(0.07331)	(0.06641)	(0.08029)	(0.07451)
Enroll*For-profit	0.1637	0.1436	-0.1066	-0.1162	0.5733***	0.5490***	0.6125***	0.5983***
	(0.1526)	(0.1599)	(0.3835)	(0.3857)	(0.2085)	(0.2056)	(0.2224)	(0.2255)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	15946	15792	15792	15792	17268	17062	17062	17062
	Panel C: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	510.95	472.23	559.26	553.84	1872.2**	2285.3***	2177.2***	2458.8***
	(380.57)	(392.18)	(634.17)	(605.17)	(842.06)	(596.50)	(837.20)	(605.37)
Enroll*For-profit	5905.3***	5684.0**	6186.0*	6200.3	4492.3**	4087.7**	5171.0***	4753.1***
	(2182.3)	(2402.8)	(3724.5)	(3806.4)	(1906.1)	(1650.2)	(1743.4)	(1589.6)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21666	21456	21456	21456	23452	23174	23174	23174
	Panel D: Number Employed, 6 years after enrollment							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.6296	0.6642	0.8052***	0.8052***	0.8564***	0.8716***	0.8587***	0.8720***
	(0.8954)	(0.6749)	(0.09290)	(0.09278)	(0.04046)	(0.03298)	(0.04087)	(0.03531)
Enroll*For-profit	-1.6788	-1.4145	-0.3160	-0.3154	-0.1883	-0.2119	-0.1912	-0.2100
	(6.8794)	(5.2074)	(0.7305)	(0.7346)	(0.1295)	(0.1368)	(0.1318)	(0.1404)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8710	8626	8626	8626	9440	9324	9324	9324

This table uses public and for-profit institutions, excluding institutions classified as online at the start of the sample period in 1999-2000. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. Robust standard errors adjusted for clustering by CBSA are in parentheses. Labor demand shocks are proxied by three-year or one-year rolling Bartik shocks. Results for one-year Bartik shocks are not presented in tables, but are qualitavely similar. Two-digit industry employment data from QCEW are used for computation of Bartik shocks. The supply measure is For-profit Attendance at the corresponding level (two-year or four-year) in the specific geography at the start of the sample period in 1999-2000. All regressions include the following CBSA level variables as controls: base-year employment, gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions.

Table A-15: OLS Estimates of the Impact of For-profit Attendance on Student Borrowing, by Loan Type

	Panel A: Number Recipients: Direct Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02622***	0.03119***	0.02556***	0.03052***	0.1692***	0.1689***	0.1691***	0.1688***
	(0.0007216)	(0.0007521)	(0.0007227)	(0.0007532)	(0.001813)	(0.001854)	(0.001802)	(0.001843)
Enroll*For-profit	0.2809***	0.3040***	0.2759***	0.2991***	0.1026***	0.1082***	0.1018***	0.1074***
	(0.005400)	(0.005443)	(0.005413)	(0.005454)	(0.002621)	(0.002690)	(0.002618)	(0.002687)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21936	21936	21936	21936	23752	23752	23752	23752
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.02148***	0.02599***	0.02088***	0.02536***	0.1496***	0.1497***	0.1498***	0.1500***
	(0.0006182)	(0.0006465)	(0.0006182)	(0.0006465)	(0.001905)	(0.001947)	(0.001891)	(0.001932)
Enroll*For-profit	0.2744***	0.2951***	0.2698***	0.2904***	0.1320***	0.1367***	0.1313***	0.1362***
	(0.004627)	(0.004679)	(0.004630)	(0.004682)	(0.002755)	(0.002825)	(0.002746)	(0.002817)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21936	21936	21936	21936	23752	23752	23752	23752
	Panel C: Number Recipients: FFEL Subsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.003123***	0.005048***	0.002774***	0.004671***	0.1268***	0.1288***	0.1269***	0.1289***
	(0.0004537)	(0.0004756)	(0.0004578)	(0.0004800)	(0.001010)	(0.001009)	(0.001018)	(0.001017)
Enroll*For-profit	0.2672***	0.2783***	0.2643***	0.2753***	0.3881***	0.3860***	0.3878***	0.3856***
	(0.003781)	(0.003816)	(0.003819)	(0.003855)	(0.001572)	(0.001577)	(0.001595)	(0.001600)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16082	16082	16082	16082	17426	17426	17426	17426
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.0006467*	0.002550***	0.0002689	0.002141***	0.09952***	0.1016***	0.09965***	0.1018***
	(0.0003651)	(0.0003831)	(0.0003672)	(0.0003853)	(0.0008690)	(0.0008688)	(0.0008741)	(0.0008734)
Enroll*For-profit	0.2308***	0.2424***	0.2275***	0.2391***	0.3961***	0.3947***	0.3960***	0.3946***
	(0.003043)	(0.003074)	(0.003063)	(0.003095)	(0.001353)	(0.001357)	(0.001370)	(0.001374)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16082	16082	16082	16082	17426	17426	17426	17426

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions.

Table A-16: OLS Estimates of the Impact of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults

	Panel A: Number of Loans							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.04550***	0.05750***	0.04378***	0.05570***	0.5288***	0.5305***	0.5294***	0.5311***
	(0.001306)	(0.001332)	(0.001316)	(0.001343)	(0.002745)	(0.002783)	(0.002742)	(0.002779)
Enroll*For-profit	1.0564***	1.1091***	1.0441***	1.0968***	0.8188***	0.8213***	0.8178***	0.8204***
	(0.009771)	(0.009639)	(0.009859)	(0.009723)	(0.003969)	(0.004038)	(0.003983)	(0.004052)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21936	21936	21936	21936	23752	23752	23752	23752
	Panel B: Loan Origination Amount ($)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	135.61***	172.61***	129.54***	166.21***	2669.2***	2684.6***	2676.7***	2692.8***
	(4.1541)	(4.2573)	(4.1790)	(4.2818)	(13.952)	(14.010)	(13.891)	(13.933)
Enroll*For-profit	3808.4***	3972.7***	3764.4***	3928.2***	3489.6***	3506.6***	3480.4***	3497.4***
	(31.089)	(30.811)	(31.298)	(31.009)	(20.171)	(20.325)	(20.175)	(20.313)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	21936	21936	21936	21936	23752	23752	23752	23752
	Panel C: Number of Borrowers in Default (100%)							
	2-year Schools				4-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.0007786***	0.001066***	0.0007216***	0.001004***	0.003607***	0.003476***	0.003724***	0.003599***
	(0.00006402)	(0.00006776)	(0.00006414)	(0.00006789)	(0.0003146)	(0.0003226)	(0.0003122)	(0.0003196)
Enroll*For-profit	0.04182***	0.04376***	0.04126***	0.04317***	0.02339***	0.02183***	0.02431***	0.02270***
	(0.0005637)	(0.0005739)	(0.0005651)	(0.0005754)	(0.0005269)	(0.0005408)	(0.0005281)	(0.0005412)
Time FE Type	Year	Year	State*Year	State*Year	Year	Year	State*Year	State*Year
State*FP, Yr*FP	No	Yes	No	Yes	No	Yes	No	Yes
Observations	14616	14616	14616	14616	15463	15463	15463	15463

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and year or state-year fixed effects. Two year institution group includes two-year and less than two year institutions.

Table A-17: OLS Estimates of the Impact of For-profit Attendance on Employment, Earnings, and Graduation

	Panel A: Total Graduated, assuming 150% Completion Time			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.2717***	0.2649***	0.3265***	0.3257***
	(0.001119)	(0.001160)	(0.002745)	(0.002725)
Enroll*For-profit	0.7247***	0.6997***	-0.2482***	-0.2520***
	(0.008833)	(0.008823)	(0.004082)	(0.004070)
Time FE Type	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	17560	17560	19022	19022
	Panel B: Number Employed, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.7879***	0.7904***	0.9082***	0.9083***
	(0.0007800)	(0.0008213)	(0.0008089)	(0.0008135)
Enroll*For-profit	-0.3394***	-0.3279***	-0.04321***	-0.04688***
	(0.007105)	(0.007171)	(0.001895)	(0.001927)
Time FE Type	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	8774	8774	9516	9516
	Panel C: Total Earnings, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	23793.0***	23915.4***	37571.5***	37464.0***
	(49.116)	(52.416)	(68.413)	(68.286)
Enroll*For-profit	-12986.2***	-12305.0***	-96.458	-306.33*
	(447.38)	(457.62)	(160.25)	(161.79)
Time FE Type	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	8774	8774	9516	9516
	Panel D: Total Students Making $25k, 6 years after enrollment			
	2-year Schools		4-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.5026***	0.5051***	0.7094***	0.7081***
	(0.0009819)	(0.001043)	(0.0009547)	(0.0009515)
Enroll*For-profit	-0.2526***	-0.2386***	-0.02534***	-0.02837***
	(0.008944)	(0.009110)	(0.002236)	(0.002254)
Time FE Type	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	8774	8774	9516	9516

This table uses public and for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated to geography (here CBSA), institution-type (public or for-profit) and level (two-year or four-year). *,**,***: significant at the 10, 5, and 1 percent level, respectively. All regressions include the following CBSA level variables as controls: gender composition (%female), racial composition (%black, %Hispanic, %American India, %Asian, %Two or more races), age composition (%20-29, %30-39, %40 or over), %poverty, and total population. All regressions also include geography and state-year fixed effects. Two year institution group includes two-year and less than two year institutions.

Table A-18: Summary Statistics: CBSA-level Variables (2000-2014)

Population Count	344530.90
	(1.09e+06)
Percent Female	0.51
	(0.01)
Percent White	0.85
	(0.14)
Percent Black	0.10
	(0.13)
Percent American Indian	0.02
	(0.05)
Percent Asian	0.02
	(0.04)
Percent Two or more races	0.02
	(0.02)
Percent Hispanic	0.09
	(0.14)
Percent Age 0-19 years	0.27
	(0.03)
Percent Age 20-29 years	0.14
	(0.04)
Percent Age 30-39 years	0.13
	(0.02)
Percent Age 40+ years	0.46
	(0.06)
Poverty Rate	0.15
	(0.05)
Observations	47504

Means of variables reported in cells. Standard deviations in parentheses. All years are indexed by spring year.

Table A-19: Data Ranges and Cohorts for Outcomes

Outcome Variables	Data Ranges (1)	Cohorts (2)
Number of Recipients: Direct Subsidized loans	2000-2014	2000-2014
Number of Recipients: Direct Unsubsidized loans	2000-2014	2000-2014
Number of Recipients: FFEL Subsidized loans	2000-2014	2000-2014
Number of Recipients: FFEL Unsubsidized loans	2000-2014	2000-2014
Number of Federal Loans	2000-2014	2000-2014
Loan Origination Amounts	2000-2014	2000-2014
Number of Borrowers in Default, assuming 100% completion time (2 year schools)	2002-2011	2000-2009
Number of Borrowers in Default, assuming 100% completion time (4 year schools)	2004-2011	2000-2007
Number of Borrowers in Default, assuming 150% completion time (2 year schools)	2003-2011	2000-2008
Number of Borrowers in Default, assuming 150% completion time (4 year schools)	2006-2011	2000-2005
Number Employed, 6 years after enrollment	2005-2011	2001-2006
Total Earnings, 6 years after enrollment	2005-2011	2001-2006
Number of Students Making $25k, 6 years after enrollment	2005-2011	2001-2006
Number of Students Graduated, assuming 150 % completion time (2 year schools)	2002-2013	2000-2011
Number of Students Graduated, assuming 150 % completion time (4 year schools)	2005-2013	2000-2008

Years are reported in terms of the spring semester of academic years. Data for number of borrowers in default come in fiscal years.