STAFF
REPORTS

NO. 811
APRIL 2017

REVISED
OCTOBER 2021

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FEDERAL RESERVE BANK *of* NEW YORK

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Federal Reserve Bank of New York Staff Reports, no. 811
April 2017; revised October 2021
JEL classification: D14, H81, I22, I23, J24

Abstract

For-profit providers have become an important fixture of U.S. higher education markets. Students who attend for-profit institutions take on more educational debt and are more likely to default on their student loans than those 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 student debt and repayment outcomes as well as the educational and labor market mechanisms that drive any such effects. We approach this problem using a novel instrument combined with comprehensive institution-level data on student debt, default, educational attainment, and labor market outcomes. Our instrument leverages the interaction between changes in the demand for college due to labor demand shocks and the baseline supply of for-profit schools. We compare how enrollment and subsequent outcomes change 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. Among four-year students, for-profit enrollment leads to more loans, higher loan amounts, an increased likelihood of borrowing, and an increased risk of default. Two-year for-profit students also take out more loans, originate more student debt, and have higher default rates. We present evidence that these debt and default outcomes are driven by higher for-profit tuition and a negative effect of for-profit enrollment on labor market outcomes. Our results point to high costs and low returns to for-profit enrollment that generate worse student debt and repayment outcomes. These findings have important implications for public investments in higher education as well as for how students make postsecondary choices.

Key words: postsecondary education, for-profits schools, student loans, default, returns to education

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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. For-profit enrollment peaked in 2010 at 9.6% of enrollments and then declined to 5.0% by 2018. In 2018, almost 1 million students were enrolled in a for-profit postsecondary institution. There were 498,647 degrees awarded by for-profits in 2018-2019, which is 9.8% of all degrees awarded in that year nationally.

The rise of for-profit higher education has coincided with large increases in student loan debt (Looney and Yannelis forthcoming). Total federal loans to undergraduate students rose from $34.1 billion in 1999-2000 to $57.5 billion in 2017-2018, a 66.3% increase (Trends in Student Aid 2019). The three-year cohort default rate (CDR) for student loans is over 10%, meaning that over 10% of students default on their loans in the first three years of repayment. While CDRs have declined from their peak in 2010 (14.7%), they are still well above their pre-recession levels. For-profit schools have been a central focus of the debate over the causes of these student debt and default trends. In 2016-2017, about 15% of Stafford loans went to for-profit students, even though they only comprise 5.6% of enrollments (Digest of Education Statistics 2019). Furthermore, 39% 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). The argument made by critics of for-profits is that these patterns are driven by a combination of higher tuition charged by for-profits and worse post-enrollment labor market outcomes among for-profit students relative to their public counterparts in similarly-selective institutions. Higher tuition induces students to take out more loans, while lower completion rates, longer time-to-degree, and lower earnings post-attendance reduce the ability of students to repay these loans. The high prevalence of adverse debt outcomes among for-profit students underscores the importance of more fully understanding how for-profit enrollment affects student finances.

Because they serve a larger proportion of students from disadvantaged backgrounds (Chakrabarti, Lovenheim and Morris 2016b; Deming, Goldin and Katz 2012), the worse financial outcomes associated with for-profit enrollment are not necessarily indicative of a causal effect of these institutions. Identifying this causal effect 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 15% of Pell grants and Stafford loans go to for-profit undergraduate students, totalling over $14.5 billion (Trends in Student Aid 2019). The Federal government also spends a large amount of veterans benefits from the post-9/11 GI Bill on veterans (Barr 2015). 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 2016), 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-profit colleges typically do not practice selective admissions, and there usually are local, lower-priced nonselective public colleges or universities that students can attend instead (Deming, Goldin and Katz 2013). Whether students and taxpayers would be better served by shifting attendance from for-profits to these other similarly-selective local options currently is poorly understood.

In this paper, we provide the first causal evidence on the effect of attending a for-profit college relative to a local public college on student debt and default. We use a novel identification strategy based on local labor demand shocks combined with the latent supply of postsecondary schools in an area. To understand the mechanisms driving the effects we find, we also examine how for-profit enrollment impacts educational attainment and labor market outcomes.

Employing institution-level data on enrollment, graduation, financial aid, student debt, default, and subsequent employment and earnings, we leverage the fact that higher education enrollment increases when there is an adverse local labor demand shock and vice versa (Betts and McFarland 1995; Christian 2007; Clark 2011; Hershbein 2012; Aguiar, Hurst and Karabarbounis 2013; Long 2014; Chakrabarti, Lovenheim and Morris 2016). This is because negative labor demand shocks lower the opportunity cost of college for both recent high school graduates and unemployed workers. With positive labor demand shocks, the opportunity cost of college increases, and these groups find it more remunerative to be in the labor force.

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 baseline employment share of an industry in a local area with national trends in that industry's employment shares. Our main innovation comes from interacting this labor demand measure with for-profit penetration in the base year (2000)[1] in the CBSA. The idea behind our approach is that most students attend college in

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

their local area, and so areas that have more for-profit schools will see an increase in for-profit enrollment when overall demand for college enrollment increases. We thus leverage the intuition 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. Our instrument exploits *the interaction of these two forces*: we compare changes in outcomes among two otherwise similar CBSAs that experience similar changes in labor demand at the same time but that have different baseline postsecondary supply conditions. The main identification assumption we invoke is that there are no contemporaneous shocks or trends in our outcomes of interest that are correlated with the timing, magnitude, and sign of local labor demand changes *as well as* with the initial supply of for-profit schools.

We measure for-profit supply by the percentage of for-profit schools in each CBSA of each level (two- or four-year) in 2000. We find that the interaction of the baseline percentage of four-year (or two-year) for-profit schools at the CBSA level with the predicted labor demand measure is a strong instrument for four-year (two-year) for-profit enrollment. For a 1 percentage point negative labor demand shock, enrollment at 4-year for-profit schools (relative to that in four-year public schools) increases by 230 students in a CBSA when the baseline for-profit share is 1 percentage point higher. Among two-year schools, the effect is 105 students. Both estimates are statistically different from zero at the 1% level. These findings 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 training during the great recession. To our 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 financial outcomes among for-profit students relative to public students. Among four-year students, for-profit students take out one additional federal loan on average and originate over $3,300 more in loan debt relative to public school students. This increased loan debt reflects a higher likelihood of taking out both subsidized and unsubsidized loans, and we show it largely is driven by higher tuition levels faced by for-profit students. We also find that four-year for-profit enrollment increases the likelihood of student loan default by 11 percentage points, which is nearly double the baseline default

academic year.

likelihood (6.95%). Results among two-year students are similar: for-profit students take out 1 additional loan and originate about $6,000 more in debt. We also find that they are much more likely to default, however these estimates are imprecise.

To understand the mechanisms driving the student debt and default effects, we examine how for-profit enrollment impacts tuition, educational, and labor market outcomes. We find that for-profit enrollment, in both two-year and four-year institutions, leads to markedly higher tuition prices compared to public enrollment. Our results further indicate that for-profit enrollment leads to a lower likelihood of being employed and lower labor market earnings among both two-year and four-year students. We also find evidence that four-year for-profit students are less likely to obtain a BA. Taken together, our results suggest that higher tuition levels induce for-profit students to take out more student loans, and the worse labor market outcomes combined with the higher debt levels induces more default.

Finally, we examine how for-profit as well as public college entry/exit responds to labor demand shocks. Entry of neither for-profits nor publics responds strongly to negative local labor demand shocks. We additionally show that entry/exit effects are not occurring differently across the pre-existing supply distribution in a manner that could drive our results.

This analysis relates most closely to the body of research that examines the return to for-profit enrollment. The literature on the effect of for-profit colleges on student debt and default is sparse. The prior research is mostly descriptive in nature and shows evidence that students at for-profit schools take on more debt and have higher default rates than their counterparts in non-for-profit schools (Looney and Yannelis 2015; Deming, Goldin and Katz 2012; Liu and Belfield 2014a). Of concern in interpreting the patterns in these papers as causal is that for-profit institutions typically serve a more disadvantaged population; this selection mechanism could drive much of the adverse outcomes associated with for-profit enrollment. Deming, Goldin and Katz (2012) and Liu and Belfield (2014a) adjust for such selection by controlling for student background characteristics, including pre-collegiate academic achievement. Deming, Goldin, and Katz find that for-profit students take out $2,000-$4,000 more in debt and are 7-8 percentage points more likely to default. Liu and Belfield report a for-profit borrowing effect of $4,000-$5,000, which increases to $15,000 among two-year students. While these studies represent an important advancement over examination of raw means, the identification assumption that even a rich set of observables can account for baseline heterogeneity across students is strong. We advance this literature by estimating the effect of for-profit attendance on student debt and default using an empirical strategy that more credibly identifies causal effects.

Our paper also relates to the much larger literature on the effects of for-profits on employment and earnings. Much of this research uses selection-on-observables methods (Deming, Goldin and Katz 2012; Liu and Belfield 2014a; Denice 2015; Lang and Weinstein 2012, 2013). Broadly, these papers find that for-profit students experience worse labor market outcomes in terms of earnings and employment than observationally-similar students who attend a non-for-profit.

Several papers examining labor market impacts of for-profit enrollment address the shortcomings of selection-on-observables methods. Notably, these papers do not examine student debt or loan outcomes. An increasingly common approach is to use individual fixed effects in earnings regressions. However, 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. Findings from these analyses point to for-profits raising earnings relative to those who do not attend college (Cellini and Chaudhary 2008) but lowering earnings growth relative to those who attend public institutions (Turner 2011; Cellini and Turner 2016; Liu and Belfield 2014b).

The papers that most credibly overcome the selection biases associated with identifying the causal effect of for-profit enrollment on labor market outcomes are two randomized audit studies (Darolia et al. 2015; Deming et al. 2016). These studies sent resumes to potential employers and randomly included 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 our analysis. Furthermore, while the audit studies provide important insight into employer perceptions, one cannot necessarily translate these findings to labor market outcomes (Heckman 1998).

The prior for-profit research is consistent with a negative effect relative to enrolling in another type of non-selective institution. However, the estimates in this literature based on identification strategies that credibly overcome selection bias only examine earnings (in fixed effects studies) or call-back rates (in audit studies). It is important to examine effects on student debt and default using methods that can plausibly account for selection to try to understand the myriad ways in which the decision to enroll in a for-profit college affects students in the long run. Our study starts to fill this important gap.

We make several contributions to the literature. First, we estimate causal effects of for-profit enrollment on student loan takeup and default, which allows us to paint a more complete picture of how for-profit enrollment affects students. Given the rise in student debt and high default rates of for-profit students, identifying how for-profit enrollment affects student loan outcomes is of high importance. Second, we propose a new method for identifying the effect of for-profit (relative to public) enrollment on students that we argue plausibly accounts for selection bias. Third, the variation we use is of interest given the high policy concern surrounding worker training. 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 localities recover from recessions. Our estimates are local to students who enroll in college because of labor demand variation, and we are the first to analyze the effect of for-profit enrollment on student debt and repayment among labor demand-induced college attendees. Fourth, we examine both four-year and two-year enrollment and outcomes. Taken together, our results provide important new insight into the role played by local labor demand and higher education supply conditions in postsecondary enrollment and subsequent student debt and repayment outcomes.

2 Data

The data we use in this analysis come from five sources: Quarterly Census of Employment and Wages (QCEW), Integrated Postsecondary 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.

2.1 Measuring Labor Demand Shocks

We measure labor demand shocks using the shift-share approach pioneered by Bartik (1991). Broadly, this entails combining industry employment shares in a local area with changes in national employment shares in that 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. Our main sample uses 2-digit North American Industry Classification System (NAICS) industry codes at the CBSA level to measure employment shares. 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.

Using the QCEW data, we construct predicted rolling 3-year labor demand shocks ($\hat{\eta}$). The predicted labor demand shock for CBSA (c) in year (t) and state (s) is given by:

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

where γ is the employment share of industry k in 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. We use national employment share changes outside of the CBSA in order to guard against the possibility that national changes are influenced by labor demand shifts within the CBSA. 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 base employment share changes are endogenous. We underscore that we control directly for $\hat{\eta}_{ct}$ in our empirical models. Furthermore, our estimates are robust to using CBSA employment shares in 2000 rather than varying baseline shares (available upon request).

Figure 1 shows the geographic variation in labor demand changes across CBSAs from 1997-2000. There is a large amount of variation: some areas experience modest positive labor demand increases while others experience sizable contractions. There also is little spatial 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, large labor demand changes also are prevalent. The recessions of the early 2000s and 2008 are clearly evident. 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.

2.2 Measuring Student Outcomes and Demographic Characteristics

We use total 12-month enrollment data from IPEDs, which include all students enrolled during the 12-month reporting period (July 1 - June 30) for credit. Since our outcome measures relate to students enrolled during the 12-month period, the 12-month enrollment measure is the most relevant. 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.

One complication with using IPEDS data is that while participation in IPEDS surveys is mandatory for institutions that disburse federal financial aid (Title IV institutions), it is voluntary for non-Title IV institutions. Consequently, non-Title IV schools are not fully represented in the data. Many for-profit institutions are not Title IV institutions (Cellini and Goldin 2014), which leads us to under-count for-profit colleges. Most of the previous literature uses IPEDS data to study for-profit institutions and also faces this limitation (e.g., Deming, Goldin and Katz 2012; Darolia 2013; Looney and Yannelis forthcoming). This essentially creates measurement error in our instrument. As long as the measurement error is uncorrelated with local labor demand shocks, it should attenuate the first stage but not bias our estimates. As we show later in Section 4.4, the number of for-profits in the IPEDS data is not correlated with our instrument, which provides direct evidence supporting this assumption.

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 measure the total number and value of loans originated as well as originations of four loan types: direct unsubsidized loans, direct subsidized loans, subsidized loans under the Federal Family Education Loan (FFEL) Program, and unsubsidized FFEL loans.

We supplement these data with information on two-year cohort 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. The two-year cohort default rate is defined as the proportion of students entering repayment in a fiscal year who default by the end of the subsequent fiscal year.

The final dataset we use to measure student outcomes is the College Scorecard (CSD), which contains earnings and employment post-enrollment for all students who received Title IV federal aid while in college. The fact that the data only include students who receive federal aid creates a potential endogeneity concern if our identifying variation is correlated with aid receipt. We show below in Section 3.2. that our instrument is unrelated to the share of students receiving Pell grants or federal loans, which suggests that our labor market estimates are not biased by endogenous selection into the College Scorecard sample. The College Scorecard data come from

US tax records and record earnings for each *incoming* cohort of students from each institution 6 years after first enrolling. We observe the number of students from each institution and cohort who are employed and not employed (including unemployed). The CSD also contain mean earnings by cohort and institution of the employed. 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 of the employed, $\%emp$ is the cohort-institution employment rate, and E is the number enrolled in 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-sector-year level, separately for four-year and two-year institutions. Institutional sectors are either for-profit or public. Thus, separately for two- and four-year institutions, for each CBSA and year there are two observations for each variable: 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 year's enrollment. 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. 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 (e.g., a shock that has 2004 as the end year) affects enrollment in the academic year starting the next fall (the 2005-06 academic year).

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 correspond to cohorts in the "repayment" year, which is 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 incoming cohorts, 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 enroll for 4 years and two-year students enroll for 2-years. Our results are robust to assuming 150% time as well.

Table 1 contains descriptive statistics of our main analysis variables, overall and by institutional level and sector. For both four-years and two-years, 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 standard deviation. As expected, for-profit students are 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 two-year than in four-year institutions. Online Appendix Table A-1 shows year ranges for all outcome variables we use in this study.

The final dataset we employ is from the US Census and contains year-by-CBSA demographic information. We obtain county-year-level estimates from the Census and use a county-CBSA crosswalk (also from the US Census) to aggregate up to the CBSA-year 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. Online Appendix Table A-2 shows tabulations of CBSA-level observables.

2.3 Measuring For-Profit Supply

Fundamental to our approach is the measure of for-profit supply. Our aim is to leverage a supply measure that captures the ability of different higher education sectors to absorb an enrollment demand increase. In the ideal scenario, we would observe excess capacity (i.e., empty seats) at each institution, but such data are not available. Instead, we rely on the fact that for-profit institutions are far more responsive to demand increases than are public institutions. This difference in responsiveness is driven by the differences in funding sources across sectors (Bound and Turner 2007; Gilpin, Saunders and Stoddard 2015). Over 90% of revenues of for-profit institutions come from tuition and fees. Among public institutions, tuition and fees account for only 20% of revenues, with public subsidies from the federal, state, and local government making up much of the remainder (Digest of Education Statistics 2019). Public subsidies respond slowly to changes in student demand (Bound and Turner 2007), while tuition and fee revenue responds in proportion to student enrollment. As a result, the marginal cost of an additional student is covered by tuition and fees in for-profit but not in public institutions. This makes for-profits much more responsive to enrollment demand increases than public schools.

The higher responsiveness in the for-profit sector can work through two channels – entry of new institutions and expansion of existing institutions. We show evidence below in Section 4.4

that the entry and exit of for-profit colleges (as well as public colleges) are not very responsive to changes in labor demand in our period of analysis, which is sensible given the length of time it takes to open new institutions and the significant startup costs of entry. Because the number of institutions does not respond strongly to demand variation, the higher responsiveness of the for-profit sector to increases in demand must largely reflect expanded enrollment in *existing* for-profit institutions. This observation is further supported by the fact that the share of for-profit schools has been highly persistent over time, as seen in the top panel of Figure A-1 that presents scatter plots of institutional for-profit shares by CBSA in 2000 and 2013. The stability of the institutional for-profit share implies that increases in enrollment share in the for-profit sector over time must have been due to expansion of seats in existing for-profit institutions. The share of for-profit institutions in a CBSA hence provides a measure of potential capacity and captures the ability of this sector to absorb enrollment demand.

An alternative measure of for-profit supply is the share of for-profit enrollment at baseline (2000). Figure A-1, middle panel, shows substantial variation in enrollment shares over time, in stark contrast to the persistence of for-profit school shares over time in the top panel. The correlations over time in the middle panel are noticeably weaker than those in the top panel. The lack of persistence of for-profit enrollment shares within a CBSA implies that the baseline enrollment share will not be as strong an instrument, since the baseline enrollment share is an increasingly weaker measure of for-profit supply as one moves farther away from 2000 (the base year). The same is not the case for the institutional share.

Furthermore, the middle panel of Figure A-1 shows that CBSAs with *lower* for-profit enrollment shares in 2000 experienced the largest increases in this share over time, while the areas with the highest for-profit enrollment shares in 2000 experienced much less growth or declines. Hence, areas with higher for-profit enrollment shares do not have a higher capacity to absorb increased demand, which implies that enrollment shares are not good indicators of capacity. The bottom panel of Figure A-1 tells a different story when we replace the horizontal axis with base-year institutional for-profit share: increases in for-profit enrollment share were uniformly spread across the base-year institutional share distribution of CBSAs, and CBSAs with a higher share of for-profit schools displayed, on average, larger increases in the for-profit enrollment share. The middle and the bottom panels of Figure A-1 together imply that CBSAs with higher for-profit institutional shares have greater capacities to absorb enrollment demand while CBSAs with higher enrollment shares have *lower* capacities of absorbing such demand. On the whole, this evidence implies that base-year share of for-profit schools better reflects the

capacity of for-profit schools to absorb increases in demand. We therefore use the base-year for-profit institutional share as our supply measure in this paper. We present further empirical evidence in favor of using this supply measure at the end of section 4.1, where we compare the performance of these two supply measures in our first-stage regressions.

Specifically, our measure of for-profit supply is the percentage of postsecondary institutions in 2000 in each CBSA that are for-profit. This is measured using 2000 IPEDS data. Census crosswalks locate schools in CBSAs, and we construct a separate measure for two-year and four-year institutions. Throughout, we exclude not-for-profit institutions. There are few 2-year not-for-profit institutions, and the 4-year not-for-profit institutions tend to be more selective institutions. Using estimates from the Digest of Education Statistics, only 7.1% of 2-year schools are not-for-profit, while 27.5% of 2-year schools are for-profit (the rest are public). Additionally, the distribution of degrees and school-types (2-year versus 4-year) are considerably different in not-for-profit schools in comparison to the for-profit and public sectors, and the tuition charged by not-for-profits also is substantially higher. The private, for-profit sector thus 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. Indeed, existing evidence suggests that students sort between the public and for-profit sectors, but not the not-for-profit sector, when state appropriations for higher education change (Goodman and Volz 2020) and when for-profits lose the ability to disperse Federal financial aid (Cellini, Darolia, and Turner 2020). The decision set of most students considering a for-profit likely consists of public and for-profit institutions.

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 CBSAs without a for-profit four-year school than without a for-profit two-year school, for-profits are not clustered in any one part of the country, and their prevalence varies even across nearby CBSAs. 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% among two-year schools. 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.

How correlated is the geographic variation with the demographic characteristics of local areas? Online Appendix Table A-3 shows means of CBSA characteristics by baseline for-

profit supply, where all variables are measured in the year 2000. The age, gender, and SES composition of local areas are balanced across CBSAs with different for-profit shares, however larger CBSAs and those with a higher Hispanic/Latino share have a higher for-profit share. In Online Appendix Table A-4, we estimate regressions of whether there is a for-profit in the CBSA (column 1) and the for-profit share (columns 2) on CBSA demographic characteristics. The coefficients on percent female, Hispanic/Latino, and age 30-39 years are significant at the 5% level, as are the population estimates, but the demographic variables in the table only explain 12-13 percent of the overall variation in for-profit supply. Hence, there is an extensive amount of idiosyncratic variation in the for-profit share across areas. While we ultimately cannot fully explain where this variation comes from, we show below in Section 3.2. that it is uncorrelated with secular trends or shocks in outcomes as well as with predicted labor demand. Furthermore, we directly control for these demographic characteristics and include CBSA fixed effects in our empirical models.

3 Empirical Approach

3.1 Methodology

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, background, and preferences that are difficult to observe and that correlate with outcomes of interest. 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, 2014), which means that the decision to enroll in a given institution is not strongly linked to where students live. In contrast, most enrollment in postsecondary institutions is local: the vast majority of students 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 with more limited options.

That the variation in supply of postsecondary institutions across areas can plausibly affect how students sort into different types of schools depending on where they live has been a focus of prior research. Previous studies attempting to use geographic variation in supply conditions either used cross-sectional variation in supply (Card 1995) or exploited changes in college openings over time (Currie and Moretti 2003). The former approach embeds the strong assumption that institutional location is exogenous, while the latter relies on there being sufficient variation in school openings (as well as the exogeneity of their locations).

We augment these prior approaches by using the interaction of demand shocks for college with pre-existing postsecondary supply conditions in CBSAs. This allows us to employ cross-CBSA variation in postsecondary supply conditions without having to rely solely on this variation for identification. Thus, our method provides a way to use variation in higher education supply to identify the returns to postsecondary investment decisions using more recent college-going cohorts and in a manner that does not rely on the strong cross-sectional assumptions invoked in prior work. 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. Resulting changes in the demand for college then interact with baseline postsecondary supply conditions to generate variation in where students enroll.

The thought experiment underlying our method is to consider two CBSAs that experience identical labor demand changes (say, negative) in a given year but that have a different pre-existing supply of for-profit colleges. The reduction in labor demand increases the demand for college enrollment in both areas, but enrollment in for-profits should increase more in the CBSA with a higher supply of for-profit institutions when this common demand shock occurs. Labor demand changes are not identical across CBSAs, however, and so we exploit the interaction between these shocks and pre-existing for-profit supply conditions in an area.

To motivate our main empirical model, first consider a sector-specific IV model separately for two-year and four-year institutions:

$$E^j_{cst} = \alpha^j_0 + \alpha^j_1 \hat{\eta}_{c,t-1} + \alpha^j_2 (\hat{\eta}_{c,t-1} * Supply_c) + \alpha^j_3 L_{c,t-4} + \lambda^j X_{ct} + \delta^j_c + \psi^j_{st} + \mu^j_{cst} \quad (2)$$

$$Y^j_{cst} = \beta^j_0 + \beta^j_1 \hat{\eta}_{c,t-1} + \beta^j_2 \hat{E}^j_{cst} + \beta^j_3 L_{c,t-4} + \kappa^j X_{ct} + \phi^j_c + \theta^j_{st} + \epsilon^j_{cst}, \quad (3)$$

where Y is our outcomes of interest in institution sector $j \in \{FP, Public\}$ in CBSA c in state

s and year t and E is total 12-month enrollment in the CBSA-sector-year. 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 enrollment for 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 (estimated separately by institution level). The model also includes CBSA (δ_c,ϕ_c) and state-by-year (ψ_{st},θ_{st}) fixed effects. We include 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.

Equation (2) shows the first stage and equation (3) shows the second stage of the IV model. The coefficients of interest are β_2^{FP} and β_2^{public}, which identify how aggregate outcomes in the CBSA-sector-year change when enrollment changes by one student in the CBSA-sector-year. The variation in enrollment is driven by the interaction of labor demand shocks between periods $t-4$ and $t-1$ and the baseline prevalence of for-profit institutions relative to public institutions in an area. Estimating this model separately by sector is akin to a difference-in-difference model with a continuous treatment measure, the sign of which varies over time.

It is feasible to estimate this model separately by sector, but interpreting the estimates is made difficult by the lack of a clear comparison group. The sector-specific instrumental variables model shows how aggregate outcomes change when enrollment grows by one student in for-profits or publics due to variation in the instrument. The motivating question of this analysis is how β_2^{FP} compares to β_2^{public}: we want to estimate the returns to enrollment in a for-profit relative to a public institution, as this is the margin of choice for most students. Additionally, the sector-specific regressions are highly demanding of the data due to the need to estimate separate sector-specific coefficients for each CBSA, state-year, and demographic variable. To increase statistical power and to facilitate comparisons across sectors, we stack the

CBSA-sector-year level data together and estimate an interacted model:

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

$$\begin{aligned} Y_{jcst} &= \beta_0 + \beta_1\hat{\eta}_{c,t-1} + \beta_2 FP_{jct} + \beta_3(\hat{\eta}_{c,t-1} * FP_{jct}) + \beta_4(Supply_c * FP_{jct}) \\ &\quad + \beta_5\hat{E}_{jcst} + \beta_6\hat{E}_{jcst} * FP_{jct} + \beta_7 L_{c,t-4} + \kappa X_{ct} + \phi_c + \theta_{st} + \omega_{sj} + \tau_{tj} + \epsilon_{jcst}. \end{aligned} \tag{5}$$

The variable FP is an indicator variable equal to 1 if the observation is for the for-profit sector. Thus, equations (4) and (5) are akin to a fully-interacted version of equations (2) and (3) subject to constraining the CBSA and state-year fixed effects as well as the effect of the CBSA-year composition controls and base period employment to be the same across the for-profits and public sectors. Instead, we include state-by-for-profit, year-by-for-profit, and supply-by-for-profit fixed effects to conserve power. The instruments in the first stage are the interaction between $\hat{\eta}$ and $Supply$ as well as the interaction between $\hat{\eta}$, $Supply$ and FP. The coefficient on the former interaction shows how public enrollment increases for a given labor demand change as a function of the for-profit supply in 2000. The coefficient on the latter interaction shows how a labor demand shock affects enrollment in for-profits relative to publics when the baseline share of for-profits is larger. The two endogenous independent variables in equation (5), $\hat{E}_{jcst}$ and $\hat{E}_{jcst} * FP_{jct}$, are instrumented with these variables. While there thus are two first-stage equations, we focus on the one with E_{jcst} as the dependent variable below because it is more straightforward to interpret. The main coefficient of interest in equation (4) is α_6, because it shows how a given labor demand shock and pre-existing for-profit supply translate into for-profit enrollment.

In addition to the CBSA fixed effects (δ,ϕ) and state-by-year fixed effects (ψ,θ), equations (4) and (5) include 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. These controls help relax the assumption that the state-year fixed effects are common across sectors while being less computationally demanding than a fully interacted set of controls. 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 estimate equations (4) and (5) separately for four-year and two–year institutions. Because there is likely to be serial correlation in the errors within CBSAs over time, we cluster all standard errors at the CBSA level.

The coefficients of interest in the second stage are β_5 and β_6. Importantly, all lower-level 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 as well as for changes in the industrial share over time within CBSAs that come from using a rolling labor demand measure (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). The $Supply_c * FP_{jct}$ variable is unique to equations (4)-(5) and is not found in the sector-specific models. This is because $Supply$ is fixed within CBSA (and thus is collinear with the CBSA fixed effects) and FP does not vary within sector. Hence, we can interpret α_5 and β_4 as parameterized versions of CBSA-by-sector fixed effects that show how for-profits differ systematically across CBSAs that have different baseline supply conditions.

Conditional on these controls, the main identifying variation comes from enrollment changes in each sector that are driven by the interaction of labor demand shocks and baseline supply. Our estimates thus reflect the effect on outcomes of a marginal student in the FP sector versus the public sector. The question of how for-profit attendance affects outcomes relative to non-attendance also is of interest, but our design does not lend itself to answering this question and thus we leave it for future study. We now turn to a discussion of the identification assumptions underlying our empirical approach and present evidence on the validity of these assumptions.

3.2 Validity of the Identifying Assumptions

The main identifying assumption we invoke is that predicted labor demand shocks only affect outcomes differently across the for-profit supply distribution because of differential enrollment in for-profits. There are two sources of bias that could confound our estimates. The first is trends or shocks in outcomes that are correlated with the timing, magnitude and sign of the predicted labor demand changes as well as with the baseline for-profit share. The second is changes to the composition of students or institutional quality that are correlated with the instrument. We discuss each of these identification assumptions in turn below.

3.2.1 Parallel Trends

In order to bias our estimates, any secular trends need to *differentially* impact for-profit schools in places where the pre-existing supply of for-profits is higher *and* be correlated with predicted labor demand. That is, a threat to identification comes from unobserved factors that mimic the timing, sign, magnitude and geographic location of local labor demand changes and that also are correlated with $Supply_c$. A particular strength of our empirical design is that our estimates are much less sensitive to bias from uni-directional secular trends than is the case in a typical panel data analysis because our labor demand measure varies in sign within a CBSA across time. Fifty-nine percent of CBSAs in our data experience both a positive and negative 3-year labor demand change between 2000 and 2014. Secular trends in outcomes should not impact our results unless they switch signs with the labor demand changes and are correlated with baseline for-profit supply.

Predicted labor demand shocks are likely to be serially correlated across years both because of persistence of labor demand shocks within an area and because we use a three-year rolling measure. Any serial correlation could be related to secular trends in outcomes within a CBSA. To account for this concern, we show below in Section 4.3 that our results are robust to controlling for leads of predicted labor demand shocks.

Of particular relevance here is the recent literature on exogeneity of the shift-share instrument. These studies posit that shift-share instruments are identified off of either cross-sectional variation in baseline shares (Goldsmith-Pinkham, Sorkin and Swift 2020) or off of temporal variation in shifts (Borusyak, Hull, and Jaravel 2018). In our approach, for-profit supply acts as the "share" and the yearly labor demand shocks (which themselves are shift-share measures) act as the shifters. Our causal claims rest on the assumptions that the baseline share is uncorrelated with trends in potential outcomes (Goldsmith-Pinkham, Sorkin and Swift 2020) *or* that the predicted labor demand shocks are uncorrelated with such trends (Borusyak, Hull, and Jaravel 2018). Goldsmith-Pinkham, Sorkin and Swift (2020) propose testing for the exogeneity of the baseline share by examining whether pre-baseline trends in outcomes vary as a function of the shares. Borusyak, Hull, and Jaravel (2018) propose a similar test for the exogeneity of the shift. We conduct each of these tests as well as for the instrument overall below. There is no evidence of differential pre-baseline trends in outcomes along any of these dimensions, which strongly supports our identification strategy.

We first examine whether there are parallel trends as a function of the for-profit share. Online

Appendix Figure A-2a shows that pre-2000 outcome trends are similar across CBSAs with above and below median baseline for-profit shares. Figure A-2b shows similar trends splitting out areas with no for-profits and by above and below median among areas with any for-profits. Second, we show in Online Appendix Figures A-3a and A-3b that there are parallel trends across CBSAs with above versus below median predicted labor demand shocks in 2000 as well as for those with positive versus negative labor demand shocks in 2000. In a supplemental analysis (available upon request), we show similar parallel trends by industrial shares that comprise part of the predicted labor demand measure. Together, these results provide supporting evidence of the exogeneity of for-profit supply and of predicted labor demand, and thus of the instrument itself. As Goldsmith-Pinkham, Sorkin and Swift (2020) and Borusyak, Hull, and Jaravel (2018) demonstrate, we need *only one* of these conditions to hold for our instrument to be exogenous. That both conditions hold in our data suggests our estimates are not biased by the existence of endogenous secular trends.

Third, we provide an additional test of exogeneity by examining whether pre-2000 outcomes are trending differently across the distribution of the instrument. Figure 5 presents trends in enrollment and outcome variables from 1996-1998 for CBSAs that are above and below the median interaction of 2000 supply and $\hat{\eta}$. Appendix Figure A-4 presents similar trends by whether CBSAs had a positive or negative value of the interaction between *Supply* and $\hat{\eta}$ in 2000. The trends are virtually identical across CBSAs with different instrument values in 2000, suggesting we are not picking up differential secular trends in our estimates.

The validity of our approach is further bolstered by the weak correlation between predicted labor demand changes and 2000 for-profit supply. The correlation between predicted labor demand and baseline for-profit supply is only 0.17 in our sample. Figures 6 and 7 provide graphical evidence of this result. Figure 6 shows the correlation between 2000 for-profit share and CBSA-year labor demand changes that are residual to the CBSA-specific mean. Both in the four-year and two-year markets, there is no evidence that labor demand changes are related to the relative size of the for-profit market in CBSAs. Figure 7 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-5, we show that trends across the supply distribution are similar after aggregating

across school types. 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 unlikely there are unobserved shocks that are correlated with both variables and with our outcomes.

3.2.2 Changes in Student Composition or Institutional Quality

Another source of bias comes from potential changes in the composition of students across institution type or changes in institutional quality induced by the instrument. If labor demand shocks generate changes in the types of students who attend each institution type or lead to quality changes in institutions differently across the for-profit supply distribution, our estimates will confound such compositional changes with treatment effects. Note that in such a case our estimates still would identify the effect of labor demand shocks on CBSA-level outcomes as a function of for-profit supply, but they no longer would reflect the causal effect of an individual attending a for-profit institution. 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 outcomes. This is an important concern because, as we show below, total enrollment (for-profit plus public) increases due to a given negative labor demand shock when the proportion of for-profits rises. This is consistent with for-profits alleviating capacity constraints, and it increases the scope for bias from changes in the composition of students.

In order to address this concern, we estimate a version of equation (4) in which the dependent variables are aggregate characteristics of students in each sector and various categories of institutional spending (proxies for college quality) in each sector. CBSA-level descriptive statistics of these variables by institutional sector and level are shown in Online Appendix Table A-5. We examine a wide range of outcomes: student racial/ethnic backgrounds, age, Federal financial aid and loan receipt, the number of colleges to which students sent a FAFSA, parental education, parental income, the academic achievement of the student body, the composition and types of degrees awarded, and institutional spending shares and per pupil spending in various spending categories.

Table 2 presents the results of estimating equation (4) using these outcomes. Only two of the 64 estimates in the table are statistically significantly different from zero at the 5% 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, the estimates are

small in magnitude with inconsistent signs, suggesting that our instrument does not generate systematic relative shifts in the composition of students or spending across institution types in a manner that would bias our estimates. 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 shock. The results in Table 2 from the extensive set of observed characteristics in our data make it unlikely that our results are being driven by compositional shifts of students or spending shifts across institutions.

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 necessarily 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. Table 2 shows labor demand shocks do not lead to changes to the composition of or degrees awarded to for-profit versus public students differentially across the for-profit supply distribution. That we do not see changes in broad categories of majors or degree types suggests our results are not being driven by changes in the types of academic credentials students earn.

The results in Table 2 also speak directly to concerns related to the composition of the complier population. The enrollment variation that identifies our estimates comes from two sources: students who otherwise would not have enrolled (extensive margin) and students who otherwise would have enrolled in another sector (switchers). That is, our estimates reflect both changes in *where* students enroll and *whether* students enroll in a postsecondary institution. Our second stage estimates compare outcomes of marginal for-profit to marginal public school students and are identified off of both groups. If the set of compliers in each sector is different due to variation in the instrument, our estimates of β_6 may be biased.

Table 2 provides direct evidence against this source of bias. We find that the relative composition of students in the for-profit sector is not changing due to variation in the instrument. While the compliers may come from multiple origins (extensive margin or switchers), Table 2 shows that the composition of compliers is not changing differently between the for-profit and public sectors, as measured by a wide range of characteristics. Although it is possible that the complier sets are different among unobserved dimensions that relate to outcomes, the set of characteristics in Table 2 is extensive. It is unlikely that differences in unobserved characteristics correlated with the outcomes of interest would arise without being detected by at least *some* of these variables. The results in Table 2 thus strongly support the assumption that our

identifying variation is not altering the composition of students in a manner that would bias the comparison of marginal enrollment effects in the for-profit versus the public sector.

4 Results

4.1 First Stage Estimates

First-stage estimates for four-year and two-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 $\hat{\eta} * Supply$. Even columns include state-by-year fixed effects, while columns (3) and (4) show estimates with state-by-for-profit and year-by-for-profit fixed effects.

Panel A presents estimates for 4-year institutions. Focusing on the estimate on $\hat{\eta} * Supply * FP$, which is the main coefficient of interest, a 1 percentage point decline in labor demand increases enrollment in the for-profit sector by 229.7 students (relative to the pubic sector) when the baseline for-profit supply is 1 percentage point higher. Panel B presents similar estimates for two-year institutions. A 1 percentage point reduction in labor demand increases enrollment by 104.5 students in the for-profit sector (relative to the public sector) for every 1 percentage point increase in baseline for-profit supply. In both sectors, the results change little with the addition of state-year, state-by-for-profit and year-by-for-profit fixed effects. Estimates in all columns are significant at the 1% level. The P-value of the first-stage F-test that the coefficients on the instruments are zero are shown at the bottom of each panel. In all columns, 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 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. The results in Table 3 imply that when there is a higher share of for-profit institutions, students are more likely to sort into those schools when a recession hits the local area. These estimates underscore the importance of for-profit institutions in worker training during recessions. To the extent that there are causal effects of college choice on student outcomes, it suggests that latent supply of postsecondary institutions can affect the dynamics of how local areas recover from recessions.

Also of interest in Table 3 is the coefficient on $\hat{\eta} * Supply$, corresponding to α_2 in equation

(4). These estimates show that public enrollment declines for a given negative labor demand shock when the for-profit share is higher, although the effect is much weaker among 4-year schools. This occurs because students are more likely to sort into for-profit schools in these areas 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 results. We consistently find that $|\alpha_2| < |\alpha_6|$, which implies 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.

We calculate the elasticity of enrollment with respect to a change in labor demand in the for-profit and public sectors, evaluated at mean for-profit shares. Among 4-year schools, the for-profit elasticity is -1.08 and the public elasticity is 0.15. For 2-year schools the elasticities are -0.69 and 0.09, respectively. These elasticities underscore the fact that for-profit institutions are far more elastic with respect to labor demand shocks than are public schools. Furthermore, at mean for-profit shares, labor demand increases have opposite-signed effects on enrollment in for-profit relative to public schools.

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 do not face much (or any) competition from the for-profit sector. In Appendix Table A-6, we show first-stage estimates that exclude any colleges and universities that have a rating of "Highly Competitive" or higher in the 2001 Barrons rankings. We perform this robustness check only among four-year schools as two-year institutions do not practice selective admissions. The estimates are extremely similar to those in Table 3 and in some cases are more precisely estimated.

In Section 2.3, we argued the for-profit institutional share is a better conceptual measure of for-profit supply than is the enrollment share, because the former captures the ability of for-profits to absorb increases in enrollment demand. We now show that the institutional share is a better empirical measure as well. Using the for-profit enrollment share leads to a weak instrument. This is consistent with the middle panel of Online Appendix Figure A-1, which shows that for-profit enrollment shares did not expand more in areas with higher baseline enrollment shares. In Online Appendix Table A-7, we present results from first stage models where we include both the institutional and enrollment for-profit shares, separately interacted with the labor demand shock. We essentially run a horse race between the two instruments that represent the potential supply measures discussed in Section 2.3. While imprecise, the estimates

clearly load on the institutional share, and the enrollment share estimates are opposite-signed. These results strongly support our approach of using the institutional for-profit share as our supply measure.

4.2 Second Stage Estimates

Tables 4-5 present second-stage estimates of β_5 and β_6 from equation (5) for our student debt outcomes. These estimates show the effect of adding an additional student to the public sector (β_5) and the incremental effect of adding an additional student to the for-profit sector relative to the public sector (β_6). Thus, they can be interpreted as the marginal effect of an additional enrollment in either sector. 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 four-year (columns 1-4) and two-year (columns 5-8) schools.

Table 4 presents our main second stage results that examine student borrowing. Panel A shows estimates for total number of loans originated. Among four-year students, there is a consistent positive effect for β_6 across columns that is significant at the 1% level. While a marginal public student originates 0.4-0.5 loans, a marginal for-profit enrollee originates 1.1 more loans than her counterpart in a public college or university. Estimates for β_6 among two-year students are similar in magnitude though less precise. Relative to the number of loans taken out by the marginal public student, these increases for the marginal for-profit student are substantial. It could be the case that these results reflect students taking out a larger number of smaller loans, and so in Panel B we present estimates of total loan origination amount. The results demonstrate that for-profit enrollment in both four-year and two-year schools leads students to take on more loan debt. A marginal four-year for-profit student originates $3,356 more in student loans than her public sector counterpart, and a marginal two-year student originates $6,428 more in student loans than a comparable student in the public sector. Thus, not only are for-profit students taking on a larger number of loans, they are taking on a larger volume of student debt. These estimates are consistent with prior results showing higher loan volumes among for-profit students (e.g., Deming, Goldin and Katz 2012; Liu and Belfield 2014a; Scott-Clayton 2018; Cellini and Darolia 2016), though they are based on a more credible identification strategy.

In Table 5, we unpack the loan estimates further by examining the effect of for-profit enrollment on the number of loan recipients by student loan type. Unlike the estimates for total

number of loans (Table 4 Panel A), these results show how for-profit enrollment affects the propensity to originate each type of loan. 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 percentage points more likely to take out a direct subsidized loan than her public counterpart. Panels B, C, and D show that for-profit enrollment in four-year schools increases the propensity of originating a direct unsubsidized loan, a FFEL subsidized loan and FFEL unsubsidized loan by 61, 48, and 42 percentage points, respectively. That the effect on subsidized loans is larger than the effect on unsubsidized loans suggests that students prefer subsidized loans on the margin, which is sensible given the more generous terms of subsidized loans. Examining results across columns shows the estimates are robust to the addition of various fixed effects we include in the regressions. Estimates for two-year students are economically smaller and are less precise. Overall, Tables 4 and 5 show that four-year for-profit students rely much more heavily on federal loans relative to public students.

Does this added debt burden among for-profit students increase the likelihood of default? Table 6 shows that for-profit enrollment increases the likelihood of default by about 11 percentage points among four-year students. These estimates are statistically different from zero at the 1 percent level and indicate that the increased loan burden associated with for-profit schools leads to a substantially higher risk of default. In two-year schools, the estimates are even larger, at 21 percentage points. The effects are significant at the 10% level when only using year fixed effects but become imprecise when state-year fixed effects are included in the model. However, these estimates are suggestive of much higher default among marginal two-year for-profit students.

To understand the mechanisms behind higher student loan debt and default for the marginal for-profit enrollee, we consider two factors: 1) higher tuition, which can induce higher debt levels and 2) lower likelihood of graduating and worse labor market outcomes. We examine each of these mechanisms in turn. Panel A of Table 7 shows how total tuition dollars paid by in-state students increase when enrollment in each sector increases by one student, which is effectively the average in-state tuition price paid by the marginal student. Tuition paid by marginal for-profit students is \$6,000-\$7,500 higher among four-year entrants and is over \$9,000 higher among marginal for-profit two-year college students relative to their public counterparts. These higher tuition prices are likely a main driver of the higher loan originations by both four-year and two-year for-profit students seen in Table 4. Indeed, the larger for-profit tuition effect among two-year students is reflected in a larger two-year loan origination effect (\$6,428 for

two-year versus $3,356 for four-year for-profit students). Our results thus suggest that students attending for-profits take on more loan debt because they face higher statutory tuition levels.

The higher tuition levels faced by for-profit students relative to public students are not driven by changes in average tuition from the enrollment demand shocks that form the basis of our identifying variation. Panel B of Table 7 shows that an additional for-profit student does not lead to increases in average tuition level (or sticker price). This implies that the enrollment demand increases stemming from our instrument do not generate increases in tuition prices. While prior work has shown that for-profit colleges set tuition in response to federal aid levels (Cellini and Goldin 2014), Table 7 shows they do not do so in response to increases in enrollment demand when there are negative labor market shocks.

To explore the second mechanism, Table 8 examines the effect of for-profit enrollment on educational attainment and labor market outcomes. Table 8 only presents results that include state-by-year fixed effects to conserve space. Even columns continue to include state-by-for-profit and year-by-for-profit 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. This result is consistent with prior research on for-profits (Deming, Goldin and Katz 2012; Liu and Belfield 2014a).

Looking past postsecondary outcomes, the remaining panels of Table 8 examine the labor market returns to for-profit enrollment relative to public enrollment. Panel B of Table 8 shows that four-year for-profit enrollment reduces the likelihood of employment by 11 percentage points, while two-year for-profit enrollment reduces the likelihood of employment by 36 percentage points. However, the two-year estimates are not statistically significant. Panel C shows that earnings, on average, are lower for both four-year and two-year for-profit students relative to their public counterparts, but the estimates are imprecise. A marginal four-year for-profit student earns about $6,000 less than her public counterpart six years after enrolling. For two-year students, the earnings penalty of for-profit enrollment is about $9,000. In both institution levels, earnings reductions are large relative to the *Enroll* estimates, which can be interpreted as the earnings of a marginal public postsecondary student.

Estimates in Panel D show less consistent results across school levels 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 aged 25 to 34 with only a high-school degree. Examining this outcome helps gauge whether students are better off from having decided to enroll in a for-profit (or public) college rather than not invest in a postsecondary degree. The likelihood that a marginal four-year public college student earns over $25,000 is 71 percent. Four-year for-profit students are about 9 percentage points (or 11%) less likely than their public counterparts to earn over this amount, and 2-year for-profit students are more likely to earn over $25,000. The estimates are imprecise, however. In both two-year and four-year schools, marginal students are substantially more likely to earn over $25,000 per year, which suggests a positive gross return to investing in a postsecondary education (both for-profit and public).

Taken together, the results in Tables 4-8 indicate that four-year for-profit enrollment leads to higher likelihood of borrowing, larger debt originations, and higher default. The results among two-year students are less clear, but we do find evidence that these students take on more debt and are more likely to default. Investigating mechanisms for these patterns, we find evidence that for-profit students pay more in tuition and experience lower graduation rates in 4-year schools and have worse labor market outcomes in both 4-year and 2-year institutions relative to their public school counterparts. The evidence thus indicates that the return to for-profit enrollment is rather low, which we argue is an important driver of the increased default risk we document. Students could take on less debt, default at lower rates, and earn more if they attended a public institution rather than a for-profit.

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-8 through A-10 present OLS results for our outcomes of interest. The estimates are qualitatively similar to those in Tables 4, 5, 6 and 8. The two-year OLS results generally paint a more negative picture of for-profit attendance on both labor market and loan outcomes, potentially because of negative selection into these for-profit schools. The four-year for-profit OLS estimates also are qualitatively similar to the IV results but tend to indicate less of an adverse effect on student outcomes. The differences in magnitudes between the OLS and IV estimates may be due to a variety of factors, such as differences in student selection across schools, differences in local labor markets, differences in school locations, and funding, as well as any interactions between these factors. Importantly, our IV estimates also capture a local average treatment effect driven in part by labor-demand induced changes in enrollment. OLS estimates, in addition to not being causal,

incorporate all enrollment variation.

4.3 Robustness Checks

In this section, we present a series of robustness checks that probe the sensitivity of our findings. A core identification concern in our analysis relates to serial correlation in the labor demand shock. We control for $\hat{\eta}_{t-1}$ 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 9, we show results for our main outcomes of interest in which we control for $\hat{\eta}_t$ and $\hat{\eta}_{t+1}$. Results are very similar to baseline. In results available on request, we find similar effects if we control for one lead of our labor demand shock ($\hat{\eta}_t$). These estimates indicate that serial correlation in the instrument is not biasing our results.

The IPEDS data define four-year institutions as institutions that grant at least one Bachelor's degree. Many universities have a mix of two- and four-year degree programs, and as shown in Table A-5, the proportion of Bachelors degrees in four-year for-profits is lower than among four-year publics. Table 10 explores the sensitivity of our results to different degree compositions of four-year for-profit and public institutions. Specifically, we investigate whether our results remain similar when we restrict our sample to public and for-profit institutions that have similar degree compositions. The first four columns show estimates in which we classify four-year schools as those with at least 25% of their degrees as Bachelors in 2000. In columns (5)-(8), we define schools above the 25th percentile of the Bachelors degree distribution in 2000 as four-year schools. Note that the number of observations changes little because the observation counts are at the sector-CBSA-year level rather than at the institutional level. The estimates in Table 10 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)-(2). But, they are qualitatively similar. Estimates in columns (5)-(8) match those in Table 4 quite closely in terms of magnitude and statistical significance. Second, the employment estimates are larger in absolute value, indicating that the for-profit enrollee is between 13-14% less likely to be employed than her public counterpart six years after enrollment.

One might be concerned that 25% of Bachelors degrees is too low a place in the distribution to cut the sample. In Online Appendix Table A-11 columns (1)-(4) and (5)-(8) respectively,

we define four-year institutions as institutions where (a) 25-50% of the degrees awarded were Bachelors Degrees in 2000 and (b) at least 75% of the degrees awarded were Bachelors. The results for the 25-50% group are very similar to the results in Table 10. The estimates are slightly smaller (in absolute value) among the 75%+ Bachelors institutions for the number of loans and loan origination amounts. But, they are larger (in absolute value) for the percent employed, indicating larger negative effects of for-profit attendance. In all cases the estimates using state-year fixed effects are within the 95% confidence intervals of the baseline results. Overall, we find little evidence of heterogeneity as the percent of Bachelors Degrees awarded increases among four-year institutions.

Rather than splitting the sample by the percentage of Bachelors degrees awarded, we can instead directly control for degree composition of schools. Since degree composition can potentially be endogenous, we instead control for interactions between baseline (2000) degree composition and year fixed effects. Online Appendix Tables A-12 through A-14 include controls for percent certificates awarded in 2000 interacted with year indicators for the two-year sample and percent Bachelor's degrees awarded in 2000 interacted with year indicators for the four-year sample. The results accord very closely with those shown above in sign, magnitude and statistical significance. These results, combined with those in Table 2, Table 10, and Table A-11, suggest that we are not simply picking up a mechanical effect of different degree types across institutions. We find no evidence of a change in the mix of degrees awarded (Table 2), little heterogeneity in effects when we restrict the sample to compare between institutions awarding like degrees in the four-year sector, and directly controlling for degree composition of the college leaves results virtually unchanged.

Although we do not find that our instruments generate changes in the composition of students along observed dimensions, there is a potential concern about switching across the two-year/four-year margin given that our models are level-specific. In Online Appendix Table A-15, we show estimates that include opposite-level supply controls interacted with the for-profit indicator and labor demand shocks. That is, in the four-year sector estimates we control for two-year baseline supply fully interacted with FP and $\hat{\eta}$ (and vice versa). These controls are not used as additional instruments but rather to account for the fact that the instruments are likely to be highly correlated across institutional levels so that not controlling for the other level may introduce an omitted variable bias driven by cross-level switching. The estimates of interest are very similar to the corresponding baseline results in magnitude, sign, and statistical significance, with the exception of the employment estimates that are no longer statistically sig-

nificant in the four-year results. However, they are qualitatively similar. These results suggest that the level-specific approach we take does not drive our results.

An additional concern is that our instrument may be 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.

Our identification strategy relies on local postsecondary enrollment, which means online options can generate bias if the online enrollment is coming differentially from areas with higher for-profit supply, given labor demand. We exclude online colleges and universities from our analysis in Online Appendix Table A-16. Despite the substantial growth in the online postsecondary sector during this period, the point estimates change little from baseline. The one exception is that the employment estimates in the four-year results are larger in absolute value, indicating larger negative effect of for-profit attendance, but they also are imprecise such that the 95% confidence interval includes the preferred estimate.

Online education expanded dramatically after 2006 due to the elimination of the Federal 50% rule that required less than half of students be involved in "distance learning" for colleges to be eligible to disburse Title IV aid (Deming et al. 2016). Prior to 2006, less than 2% of enrollment was in online programs (Deming, Lovenheim and Patterson 2020). Recall, however, that our outcome data are predominantly from the mid-2000s (Table A-1), which is a period during which online enrollment was far lower than it is today. We additionally present four-year estimates in Table A-17 where we restrict our sample to pre-2006 (columns 1-4) and pre-2007 (columns 5-8) entry cohorts when the size of the online sector was small. The estimates using this earlier, pre-online period are very similar to baseline. Furthermore, in Table A-18 we exclude all chains from the analysis set. Almost all chains are for-profit, and many of them are either fully online or have a strong online presence. The results are somewhat larger than baseline but also are less precise due to the reduced sample size. Our results and conclusions are not driven by the existence of chains.

As previously discussed, we also exclude selective institutions that have an admissions selectivity rating of Highly Competitive or higher in the 2001 Barrons rankings. The second-stage estimates shown in Appendix Tables A-19 through A-21 are very similar to those shown above. In Online Appendix Table A-22, we show estimates that include not-for-profit schools in the control group in addition to public schools. The results and conclusions using this sample are broadly similar, with the exception of the loan origination effect among four-year schools.

These estimates become negative (although they are not statistically significantly different from zero), which is sensible because students at not-for-profit institutions face higher tuition and thus initiate much higher loan amounts than do students at public schools. Taken together, the results presented in this section indicate that our main estimates are robust to the specific modeling and data assumptions we make.

4.4 For-Profit Entry and Exit

One of the mechanisms underlying our results could be entry or exit of for-profit (or public) schools in response to changes in labor demand, especially if such entry/exit is differentially responsive in areas with higher for-profit supply. To the extent that new for-profit entrants are less productive, this could drive some of the worse outcomes we find among for-profit students. In Table 11, we examine how for-profit and public entry respond to labor demand changes. Panel A shows there is at most a small relationship between predicted labor demand shocks and entry/exit of schools. Among four-year institutions, a 1 percentage point decrease in labor demand leads to 0.02 more for-profit schools. These estimates are significant at the 10% level or higher and are equivalent to only a 3% increase in for-profit institutions. Among two-year schools, labor demand shocks have no effect on the number of for-profits when we include state-by-year fixed effects.

The results in Panel B 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 for-profit supply are more amenable to for-profit schools, in which case entry would be even higher in areas with a higher baseline for-profit supply when there is a labor demand decrease. Among four-year institutions, there is little evidence that this is the case: the effect of a negative labor demand shock on entry is not different in areas that had a larger baseline share of for-profit institutions. We do find that two-year for-profits are slightly more likely to enter in areas in which they were more prevalent due to a negative labor demand shock, however the estimate is quite small. Given a 1 percentage point labor demand shock, a 1 percentage point higher for-profit supply leads to 0.001 more for-profit schools, which is equivalent to a 0.03% increase. The estimate is only significant at the 10% level.

Investigating these effects by sub-periods in Online Appendix Table A-23, we continue to find no evidence of differential four-year for-profit entry by latent supply in each period. For 2-year schools, Table A-23 shows that the for-profit entry effect in marginally higher supply areas in response to labor demand shocks presented in Table 11 is entirely coming from the 2008-14

period. Importantly, the 2008-14 period is after most of the cohorts for which we observe the outcomes of interest are enrolled. The timing of two-year for-profit entry response to labor demand shocks (in higher baseline supply areas) thus does not align with the period when the outcomes on which we focus are measured and hence are unlikely to affect our results.

The results in Table 11 are important because little currently is known about the for-profit entry decision. Our estimates indicate that for-profit institution entry/exit responds little to labor-market driven demand. Further, we find no evidence of differential entry of for-profits by baseline for-profit supply during our sample period. Entry effects hence are unlikely to be a core mechanism underlying our results.

5 Conclusion

For-profit providers have become an 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 select into colleges based on factors that are difficult to observe and that correlate with future outcomes, 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 also is possible that returns to investing in this type of school are positive. These arguments underscore the importance of identifying the return to for-profit enrollment.

We contribute to the literature on for-profit returns by using a novel instrument that we argue credibly overcomes selection problems associated with student sorting into college sectors, and we provide a more comprehensive analysis of the effect of for-profit enrollment on student debt and default outcomes than has been possible in prior work. In examining mechanisms, we supply additional evidence on the effect of for-profit enrollment on educational attainment and labor market outcomes that complements prior research. 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 is a higher share of such schools in the area in 2000.

Using predicted labor demand shocks interacted with 2000 for-profit supply as an instru-

ment for for-profit enrollment, we estimate the causal effect of for-profit enrollment relative to enrolling in a public college on student debt and default outcomes as well as on educational and labor market outcomes that provide insight into the underlying mechanisms. The four-year estimates point consistently to negative effects of enrolling in a for-profit college. Relative to their public counterparts, enrolling in a for-profit institution increases the number of loans by one, 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 face higher tuition prices, are less likely to graduate, and that they have substantially worse labor market outcomes, which we argue are core mechanisms underlying the adverse debt effects.

Estimates among 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. They also experience lower likelihoods of employment and lower earnings on average, but they are more likely to earn over $25,000 (the median earnings of high school graduates). Interestingly, these 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.

Overall, our results indicate that, on average, for-profit enrollment leads to worse student loan outcomes for students than enrolling in a public college or university, which is driven by higher loan takeup and worse labor market outcomes. 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. Second, the results suggest that students who attend local for-profit institutions when there is a negative labor demand shock may 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.

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

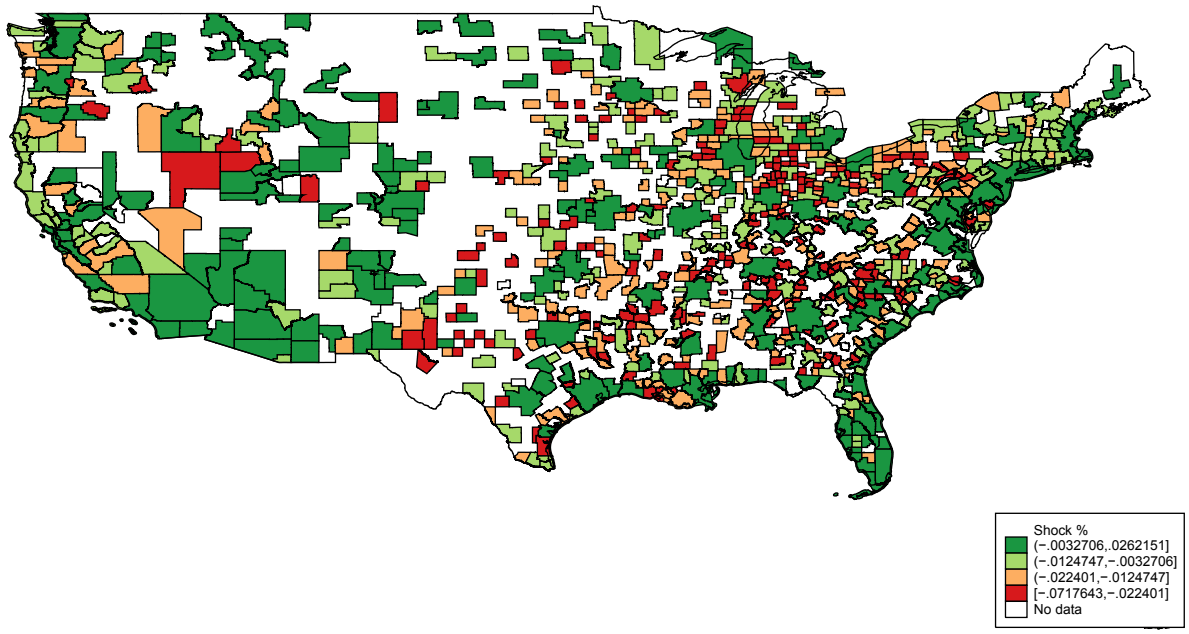


Figure 2: Quartiles of Labor Demand Changes by Year, 2000-2014

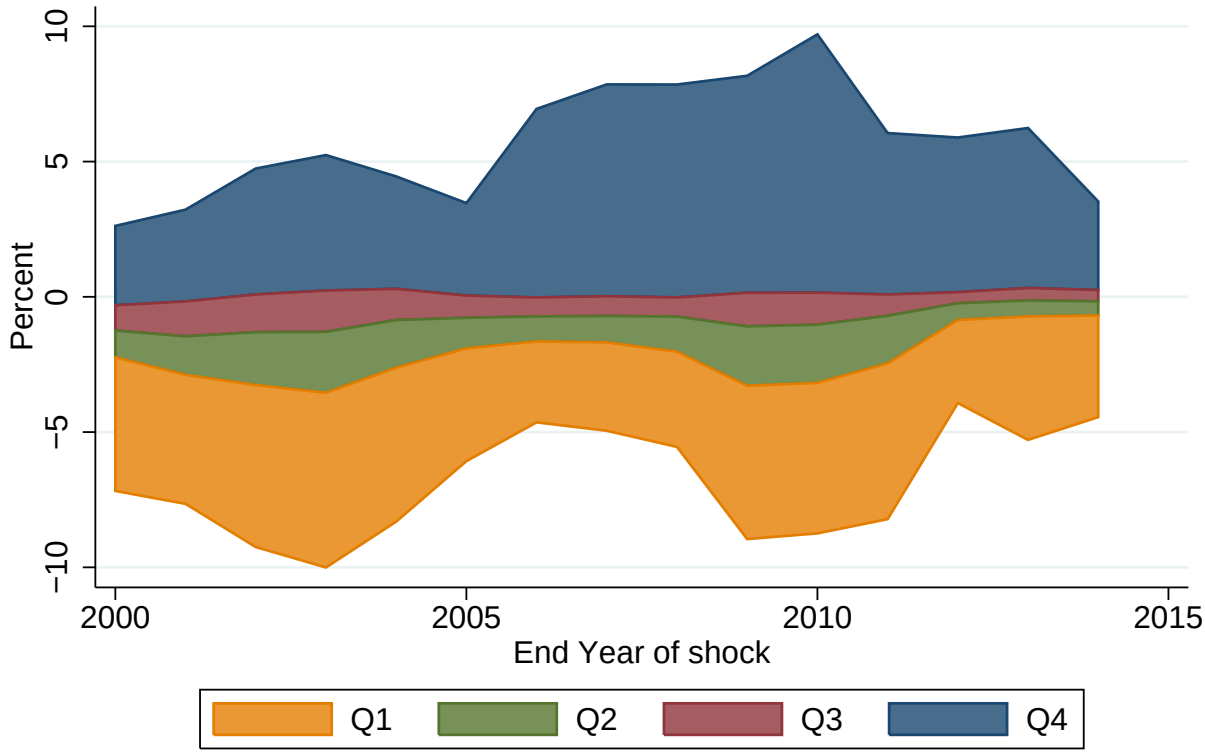


Figure 3: Percent of Two-year and Less-than-two-year For-profit Postsecondary Institutions by CBSA, 2000

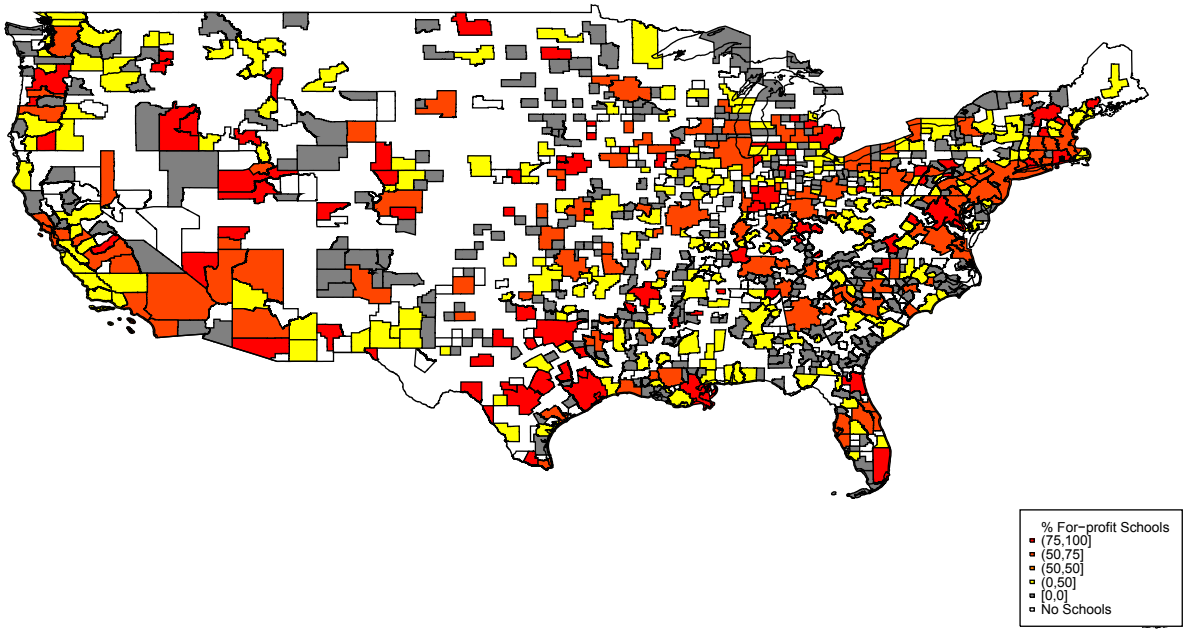


Figure 4: Percent of Four-year For-profit Postsecondary Institutions by CBSA, 2000

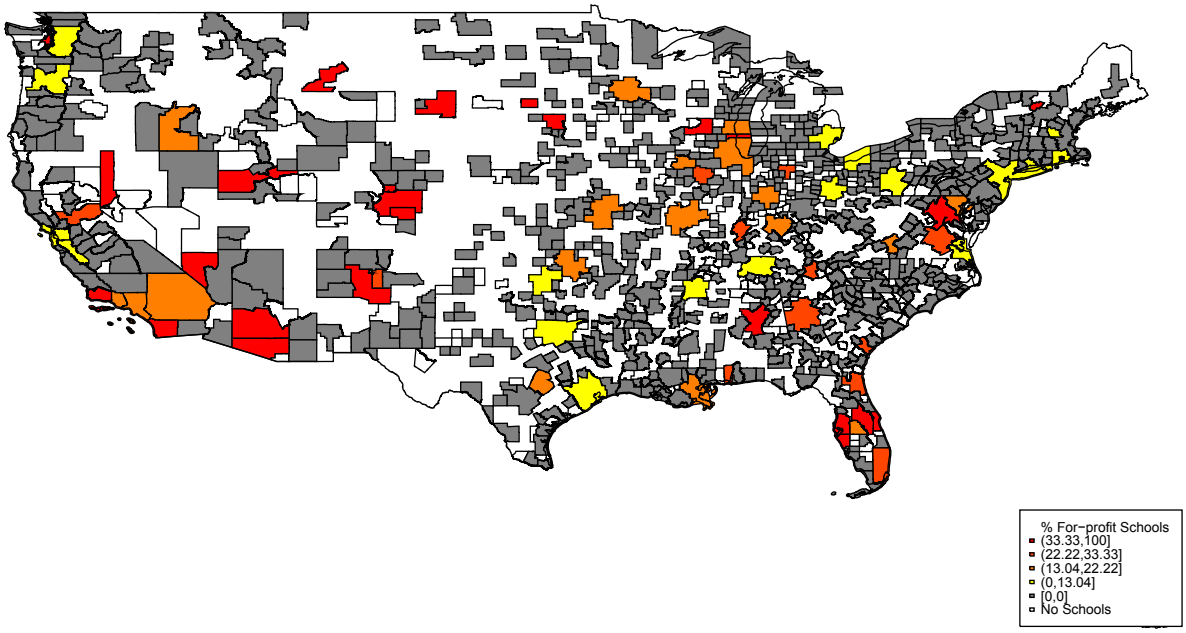
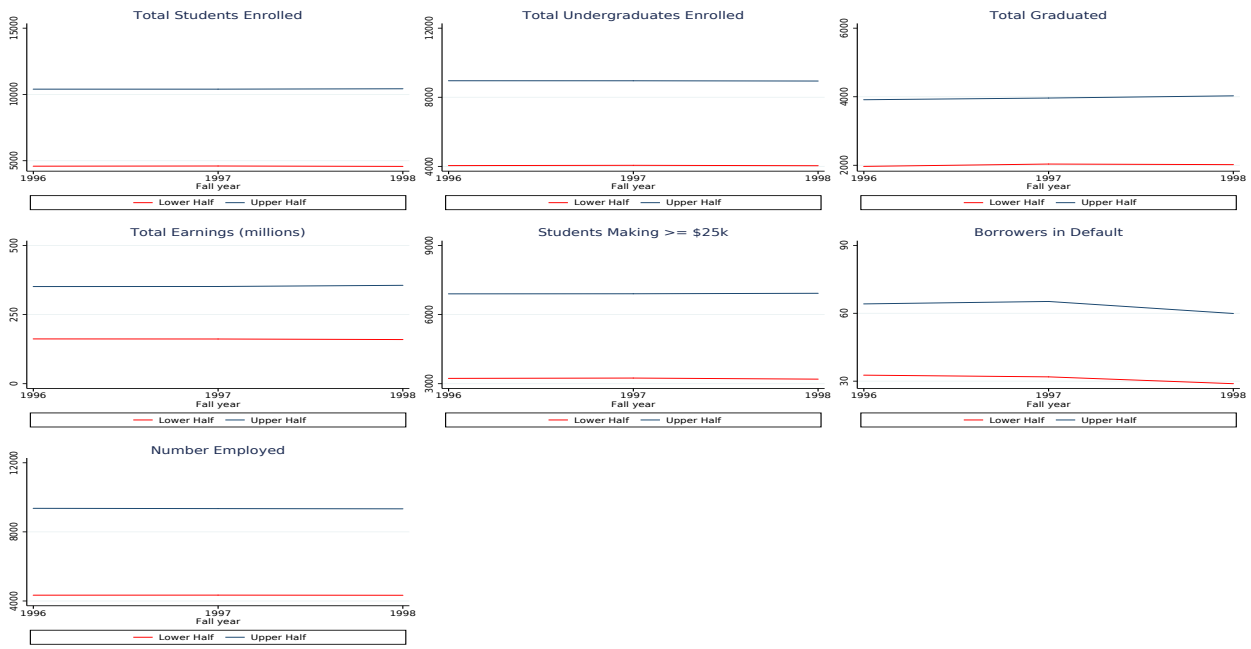
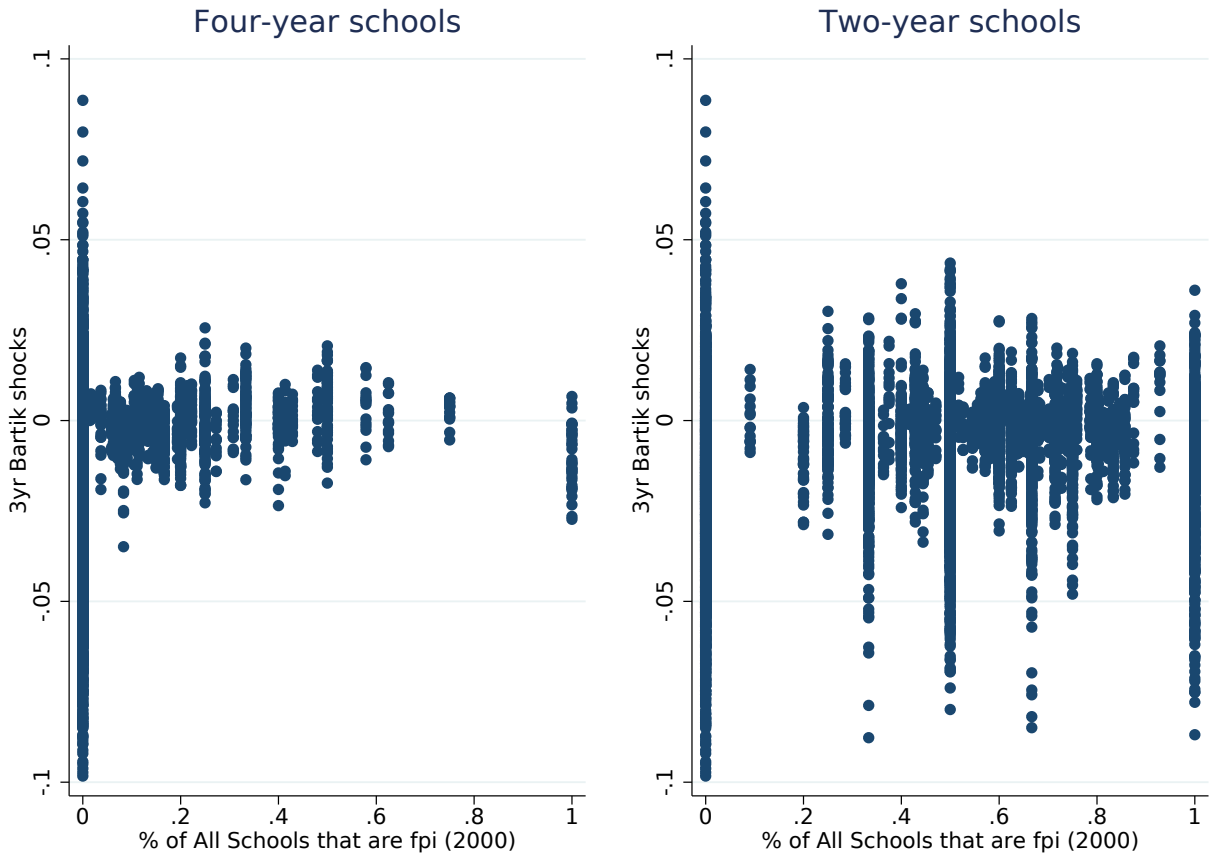


Figure 5: Pre-2000 Trends in Educational and Labor Market Outcomes by Labor Demand x For-Profit Supply, Below and Above Median



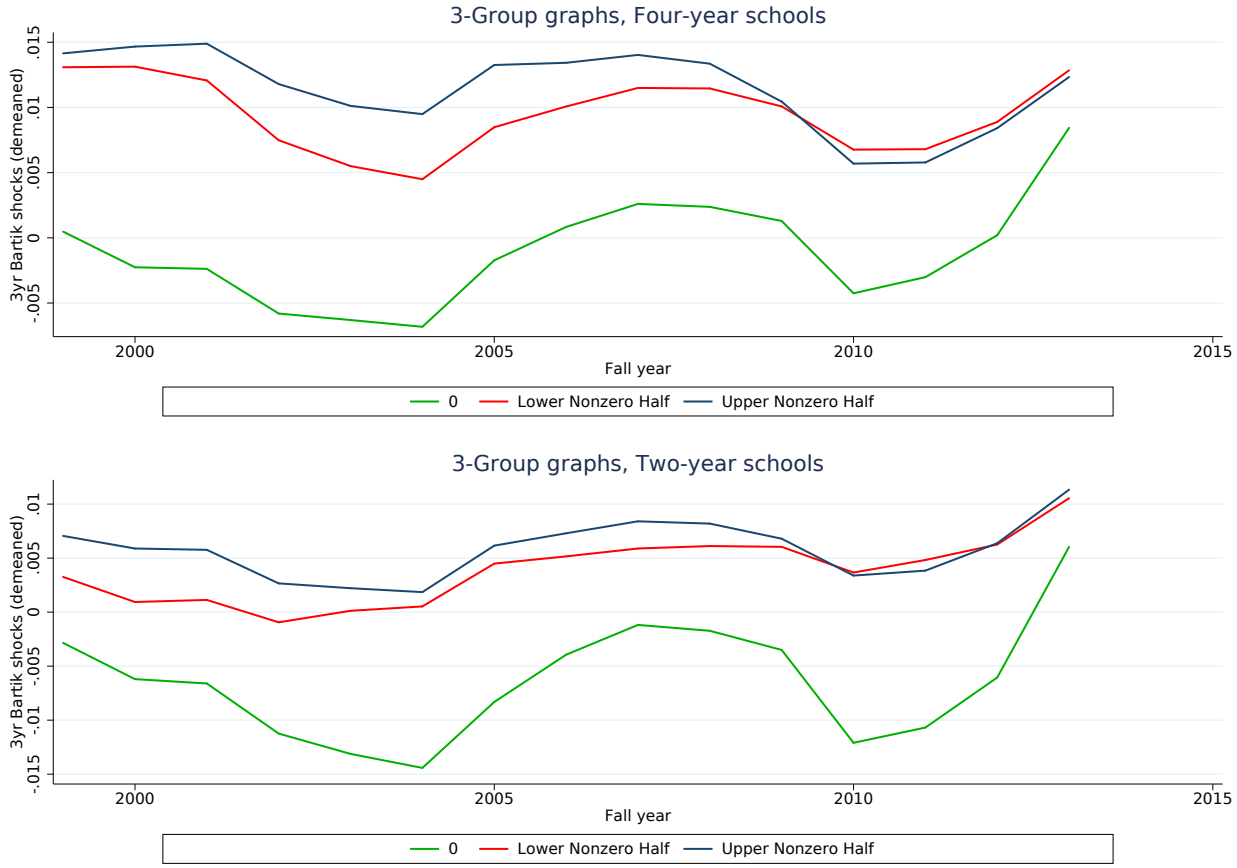
This figure plots pre-2000 trends in educational and labor market outcomes separately for CBSAs that have above and below median levels of labor demand interacted with for-profit supply ($\hat{\eta} * Supply$).

Figure 6: 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 CBSA-year observation, but only labor demand changes by year. The time span is 2000-14.

Figure 7: Demeaned Predicted Labor Demand Trends by For-Profit Supply and Institution Type



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.

Table 1: Summary Statistics (2000-2014)

	All	Four-year For-profit	Four-year Public	Two-year For-profit	Two-year Public
	(1)	(2)	(3)	(4)	(5)
Total Enrollment	6795.326	1812.815	10642.368	1313.935	13506.189
	(27899.942)	(16596.167)	(25522.423)	(5452.558)	(45732.309)
Undergraduate Enrollment	6127.707	1463.798	8424.117	1313.205	13505.704
	(26146.055)	(12979.283)	(20098.621)	(5450.774)	(45732.303)
Number of (Subsidized) Direct Loan Recipients	868.515	494.962	1908.092	388.569	627.301
	(4525.769)	(6265.482)	(5341.759)	(2082.722)	(2444.709)
Number of (Unsubsidized) Direct Loan Recipients	795.530	523.187	1759.697	352.108	489.853
	(4513.010)	(6600.587)	(5143.623)	(1936.988)	(1973.825)
Number of (Subsidized) FFEL Loan Recipients	923.562	716.775	1658.113	543.541	731.713
	(4393.746)	(7129.228)	(3907.055)	(2275.365)	(1688.119)
Number of (Unsubsidized) FFEL Loan Recipients	744.239	671.991	1280.502	457.941	527.744
	(3997.139)	(6855.462)	(3013.819)	(1910.712)	(1311.669)
Number of Federal Loans	3601.570	2733.968	7298.462	1865.439	2274.183
	(15364.285)	(23738.656)	(15625.840)	(7515.529)	(5653.429)
Total Loan Origination (in $1000s)	15038.06	12077.12	33993.63	6241.13	6516.21
	(71651.02)	(110068.06)	(79014.35)	(25869.04)	(17057.94)
Number of Borrowers in Default, 100% completion time	61.639	62.339	68.939	57.531	57.067
	(461.430)	(850.749)	(151.577)	(249.950)	(123.442)
Total students employed 6 years after enrolling	5305.940	800.374	8942.683	749.428	10804.749
	(21529.781)	(7219.958)	(21009.969)	(3043.829)	(36068.290)
Number of students graduating in 150% completion time	1903.092	201.623	3786.638	697.031	2911.915
	(8233.027)	(1880.381)	(10953.214)	(2959.978)	(11420.827)
Mean Earnings for non-enrolled students 6 years after enrolling	26538.17	30594.39	33276.99	20208.07	23551.17
	(7234.55)	(7791.45)	(6123.79)	(5333.66)	(3608.76)
Share earning $25k or more 6 years after enrolling	0.542	0.614	0.661	0.417	0.493
	(0.130)	(0.110)	(0.087)	(0.132)	(0.076)
In-State Tuition for full-time undergraduates	5093.84	13203.92	4273.72	10836.41	2284.48
	(4457.54)	(3351.73)	(2445.86)	(3520.67)	(1379.13)
3-year Labor Demand Shock	-1.097	-1.096	-1.096	-1.098	-1.098
	(1.783)	(1.787)	(1.787)	(1.779)	(1.779)
Base employment in 1999-2000	139436.6	135397.7	135397.7	143809.9	143809.9
	(460415.0)	(452057.5)	(452057.5)	(469288.4)	(469288.4)
% For-Profit schools in 2000	14.758	2.561	2.561	27.977	27.977
	(24.932)	(10.119)	(10.119)	(29.127)	(29.127)
Observations	45688	11876	11876	10968	10968

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 college. Number of borrowers in default relate to imputed freshmen cohorts assuming 100% completion time (four years for 4-year and two years for 2-year institutions). All years are indexed by calendar year in the spring of the academic 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 Relative to Publics

Dependent Variable	Four-Year	Two-Year	Dependent Variable	Four-Year	Two-Year
% Undergraduates Black	0.006	-0.008	% Arts Degrees	0.034	-0.027
	(0.028)	(0.013)		(0.085)	(0.030)
% Undergraduates Hispanic	0.025	0.006	% STEM Degrees	0.014	0.03
	(0.017)	(0.005)		(0.075)	(0.041)
% Undergraduates White	-0.042	-0.018	% Vocational Degrees	-0.031	0.003
	(0.048)	(0.015)		(0.022)	(0.041)
% Undergraduates Female	0.027	-0.051*	% Associates Degrees	0.040	0.017
	(0.046)	(0.027)		(0.097)	(0.050)
% Undergraduates Over 25	0.068	-0.007	% Bachelors Degrees	-0.107	-0.000
	(0.066)	(0.013)		(0.098)	(0.000)
% Undergraduates Receiving Pell Grant	-0.110*	-0.001	% Certificates	0.025	-0.017
	(0.061)	(0.024)		(0.041)	(0.050)
% Undergraduates Receiving Federal Loans	0.006	0.023	Total Spending Per Student	131.87	-12.16
	(0.114)	(0.034)		(111.01)	(11.69)
% Undergraduates Sending FAFSA to 1 College	0.013	0.018	Instructional Spending Per Student	11.97	-4.59
	(0.059)	(0.015)		(11.49)	(5.24)
% Undergraduates Sending FAFSA to 2 Colleges	-0.019	-0.013	Research Spending Per Student	5.41	0.19
	(0.030)	(0.011)		(10.71)	(0.30)
% Undergraduates Sending FAFSA to 3 Colleges	-0.009	-0.001	Academic Support Spending Per Student	15.56	-0.79
	(0.010)	(0.003)		(24.77)	(2.61)
% Undergraduates Sending FAFSA to 4 Colleges	0.001	-0.002	Institutional Support Spending Per Student	-1.36	-1.12
	(0.008)	(0.001)		(6.36)	(0.90)
% Undergraduates Sending FAFSA to 5 Colleges	0.017	-0.003	Share of Spending: Instruction	-0.005	0.008
	(0.027)	(0.003)		(0.038)	(0.030)
% Parents with HS Diploma	-0.050	-0.003	Share of Spending: Research	-0.042	0.003
	(0.046)	(0.018)		(0.035)	(0.005)
% Parents with Middle School Diploma	0.000	-0.003	Share of Spending: Academic Support	-0.010	0.002
	(0.006)	(0.004)		(0.059)	(0.025)
% Parents with College Degree	-0.130**	0.005	Share of Spending: Institutional Support	-0.016	-0.020**
	(0.053)	(0.014)		(0.044)	(0.010)
Average Family Income	-93.22	7.43			
	(62.71)	(16.09)			
College Average SAT Score	-0.282	-0.290			
	(1.331)	(0.205)			

Each cell is a separate estimate of equation (4) and shows the coefficient on (For-profit)*(For-profit Supply)*$\hat{\eta}$ (α_6). 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 CBSA at the start of the sample period in 2000. All regressions include CBSA and state-year fixed effects but exclude CBSA-by-year observables. 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, 2000-2014

	4-Year Schools			
Independent Variable	(1)	(2)	(3)	(4)
(For-profit Supply)*$\hat{\eta}$	12.624	25.154	22.778	35.308
	(46.124)	(42.462)	(42.845)	(41.869)
(For-profit)*(For-profit Supply)*$\hat{\eta}$	-209.420***	-209.420***	-229.730***	-229.730***
	(58.760)	(59.639)	(71.427)	(72.499)
	[-3.564]	[-3.511]	[-3.216]	[-3.169]
Time Fixed Effects	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	Yes	Yes
Observations	23482	23482	23482	23482
R-squared	0.180	0.189	0.231	0.240
P-value	0.002	0.002	0.006	0.007

	2-Year Schools			
Independent Variable	(1)	(2)	(3)	(4)
(For-profit Supply)*$\hat{\eta}$	52.299***	50.074***	55.879***	53.654***
	(11.856)	(12.058)	(12.964)	(13.152)
(For-profit)*(For-profit Supply)*$\hat{\eta}$	-97.355***	-97.355***	-104.510***	-104.510***
	(21.025)	(21.366)	(22.740)	(23.110)
	[-4.630]	[-4.556]	[-4.596]	[-4.522]
Time Fixed Effects	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	Yes	Yes
Observations	21666	21666	21666	21666
R-squared	0.155	0.157	0.258	0.261
P-value	0.000	0.000	0.000	0.000

Authors estimates of equation (4) in the text using for-profit and public 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. The p-value of the triple interaction estimate is shown in square brackets. "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 CBSA in 2000. All regressions include the following CBSA-year level variables as controls: base-year $(t-3)$ 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 fixed effects, while even columns include 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 4: Instrumental Variables Estimates of the Effect of For-profit Attendance on the Number and Volume of Student Loan Originations

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.435***	0.490***	0.451**	0.502***	0.064	0.067	0.012	0.018
	(0.162)	(0.114)	(0.192)	(0.147)	(0.128)	(0.123)	(0.233)	(0.220)
Enroll*For-profit	1.087***	1.008***	1.171***	1.108***	1.148	1.131	0.839	0.821
	(0.305)	(0.255)	(0.327)	(0.286)	(0.727)	(0.747)	(1.364)	(1.381)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1401.50***	1823.20***	1492.10**	1893.80***	511.30	509.73	596.42	590.47
	(480.40)	(324.99)	(585.54)	(313.97)	(380.01)	(365.75)	(606.55)	(577.29)
Enroll*For-profit	3375.80***	2770.50**	3846.70***	3356.10***	5907.50***	5915.30***	6412.90*	6427.70*
	(1206.60)	(1108.80)	(1220.20)	(1034.70)	(2176.60)	(2252.00)	(3576.30)	(3654.10)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 5: Instrumental Variables Estimates of the Effect of For-profit Attendance on Student Borrowing, by Loan Type

	Panel A: Number Recipients: Direct Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.343**	0.329**	0.353**	0.336**	0.008	0.009	-0.035	-0.032
	(0.156)	(0.150)	(0.177)	(0.170)	(0.122)	(0.118)	(0.223)	(0.211)
Enroll*For-profit	0.614**	0.634**	0.671**	0.692**	0.163	0.157	-0.090	-0.099
	(0.293)	(0.282)	(0.305)	(0.303)	(0.721)	(0.742)	(1.329)	(1.349)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.296**	0.293***	0.310**	0.302**	0.020	0.021	-0.015	-0.012
	(0.126)	(0.111)	(0.147)	(0.134)	(0.106)	(0.102)	(0.190)	(0.180)
Enroll*For-profit	0.529**	0.534**	0.600**	0.610**	0.245	0.240	0.033	0.025
	(0.231)	(0.224)	(0.254)	(0.255)	(0.617)	(0.636)	(1.131)	(1.148)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel C: Number Recipients: FFEL Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.155**	0.168***	0.175**	0.186***	-0.027	-0.026	-0.093	-0.090
	(0.068)	(0.061)	(0.081)	(0.071)	(0.033)	(0.033)	(0.094)	(0.091)
Enroll*For-profit	0.457***	0.442***	0.487***	0.476***	0.007	0.000	-0.426	-0.436
	(0.117)	(0.114)	(0.134)	(0.134)	(0.213)	(0.215)	(0.625)	(0.631)
Time Fixed Effects	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	17290	17290	17290	17290	15946	15946	15946	15946
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.096*	0.110**	0.112*	0.125**	-0.004	-0.002	-0.043	-0.040
	(0.050)	(0.043)	(0.058)	(0.051)	(0.024)	(0.024)	(0.056)	(0.055)
Enroll*For-profit	0.414***	0.397***	0.438***	0.424***	0.162	0.154	-0.093	-0.102
	(0.074)	(0.074)	(0.086)	(0.087)	(0.152)	(0.153)	(0.366)	(0.368)
Time Fixed Effects	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	17290	17290	17290	17290	15946	15946	15946	15946

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 6: Instrumental Variables Estimates of the Effect of For-profit Attendance on Student Loan Defaults

	Dep. Var.: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.064*	0.058*	0.057*	0.051*	0.026	0.026	0.044	0.044
	(0.036)	(0.030)	(0.034)	(0.027)	(0.016)	(0.016)	(0.067)	(0.066)
Enroll*For-profit	0.108***	0.116***	0.101***	0.106***	0.214*	0.214*	0.342	0.342
	(0.038)	(0.040)	(0.038)	(0.040)	(0.117)	(0.119)	(0.482)	(0.483)
Time Fixed Effects	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	15347	15347	15347	15347	14500	14500	14500	14500

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 Estimates of the Effect of For-profit Attendance on Tuition

	Panel A: Total In-state Tuition							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	2475.50***	3191.60***	2599.60***	3291.40***	2884.10***	2851.10***	3319.20**	3256.80**
	(932.01)	(783.49)	(983.26)	(694.47)	(818.11)	(795.41)	(1681.50)	(1590.40)
Enroll*For-profit	6762.50***	5734.50**	7407.40***	6562.40***	9215.30**	9379.20**	11798.90	11953.90
	(2307.70)	(2295.70)	(2177.80)	(2057.30)	(4475.70)	(4646.90)	(9724.20)	(9849.10)
Time Fixed Effect 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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Average In-state Tuition							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.32	-0.86	0.05	-0.10	-0.085	-0.166	-0.088	-0.266
	(0.86)	(35.83)	(0.12)	(0.46)	(0.076)	(0.151)	(0.096)	(0.389)
Enroll*For-profit	-0.18	0.61	0.00	-0.01	-0.331	-1.039	-0.349	-1.770
	(0.41)	(29.51)	(0.10)	(0.29)	(0.561)	(1.208)	(0.670)	(3.003)
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	7509	7509	7509	7509	14493	14493	14493	14493

Authors estimates of equation (5) in the text using for-profit and public institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution 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. All regressions include the following CBSA-year level variables as controls: base-year ($t-3$) 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects. Panel A shows estimates using the sum of tution payments by in-state students as the dependent variable, while Panel B uses the average in-state tuition payment per in-state student as the dependent variable. 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 8: Instrumental Variables Estimates of the Effect of For-profit Attendance on Graduation, Employment, and Earnings

	Panel A: Total Graduated, assuming 150% Completion Time			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.411***	0.399***	0.173	0.179*
	(0.103)	(0.093)	(0.114)	(0.108)
Enroll*For-profit	-0.135	-0.122	0.298	0.281
	(0.257)	(0.266)	(0.768)	(0.780)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	18854	18854	17392	17392
	Panel B: Number Employed, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.859***	0.875***	0.800***	0.800***
	(0.029)	(0.022)	(0.099)	(0.099)
Enroll*For-profit	-0.093**	-0.110**	-0.358	-0.356
	(0.044)	(0.048)	(0.780)	(0.785)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710
	Panel C: Total Earnings, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	35981.20***	36887.60***	25879.60***	25865.20***
	(3396.50)	(2830.50)	(6618.60)	(6674.90)
Enroll*For-profit	-5138.10	-6107.60	-9718.70	-8541.20
	(6306.90)	(6624.40)	(52342.50)	(53038.90)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710
	Panel D: Total Students Making $25k, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.702***	0.713***	0.623***	0.623***
	(0.051)	(0.043)	(0.207)	(0.210)
Enroll*For-profit	-0.073	-0.085	0.379	0.419
	(0.100)	(0.108)	(1.607)	(1.632)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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. Even columns include state-for profit and year-for profit 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 Estimates Including Current-year and One-year Leads of Predicted Labor Demand Changes

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.453**	0.508***	0.466*	0.519***	0.002	0.009	-0.062	-0.050
	(0.205)	(0.142)	(0.241)	(0.181)	(0.129)	(0.123)	(0.219)	(0.205)
Enroll*For-profit	1.222***	1.142***	1.308***	1.246***	0.899	0.870	0.504	0.481
	(0.340)	(0.270)	(0.357)	(0.304)	(0.771)	(0.781)	(1.338)	(1.336)
Time Fixed Effects	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	20290	20290	20290	20290	18736	18736	18736	18736
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1451.10**	1860.80***	1535.20**	1927.20***	291.11	302.92	288.24	302.90
	(598.04)	(325.77)	(754.87)	(406.59)	(357.96)	(344.31)	(515.31)	(491.21)
Enroll*For-profit	3804.30***	3217.40***	4338.30***	3880.60***	5016.90**	4972.20**	4999.10	4972.00
	(1156.30)	(1032.30)	(1194.90)	(984.49)	(2121.40)	(2160.70)	(3115.90)	(3142.60)
Time Fixed Effects	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	20290	20290	20290	20290	18736	18736	18736	18736
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.064*	0.058*	0.057*	0.051*	0.026	0.026	0.044	0.044
	(0.036)	(0.030)	(0.034)	(0.027)	(0.016)	(0.016)	(0.068)	(0.067)
Enroll*For-profit	0.108***	0.117***	0.101***	0.107***	0.213*	0.213*	0.344	0.345
	(0.038)	(0.041)	(0.039)	(0.041)	(0.116)	(0.117)	(0.487)	(0.488)
Time Fixed Effects	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	15283	15283	15283	15283	14462	14462	14462	14462
	Panel D: Number Employed, 6 years after enrollment							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.857***	0.874***	0.856***	0.875***	0.592	0.590	0.791***	0.791***
	(0.027)	(0.019)	(0.029)	(0.021)	(1.213)	(1.238)	(0.099)	(0.099)
Enroll*For-profit	-0.092**	-0.112**	-0.092**	-0.111**	-1.966	-1.994	-0.427	-0.425
	(0.043)	(0.048)	(0.045)	(0.049)	(9.308)	(9.552)	(0.782)	(0.787)
Time Fixed Effects	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	9410	9410	9410	9410	8684	8684	8684	8684

Authors estimates of equation (5) in the text using for-profit and public institutions. We include controls for the current-year (year $\hat{\eta}_t$) and one-year lead ($\hat{\eta}_{t+1}$) predicted labor demand changes. 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-year level variables as controls: base-year ($t-3$) 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 10: Instrumental Variables Estimates of the Effect of For-profit Attendance, 4-year Schools by Different Categorizations

	Panel A: Number of Loans							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.372***	0.429***	0.390***	0.450***	0.392***	0.391***	0.393***	0.392***
	(0.124)	(0.098)	(0.141)	(0.108)	(0.104)	(0.097)	(0.108)	(0.102)
Enroll*For-profit	0.898***	0.847***	0.999***	0.943***	0.871***	0.872***	0.885***	0.887***
	(0.288)	(0.268)	(0.259)	(0.229)	(0.173)	(0.162)	(0.175)	(0.164)
Time Fixed Effect 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	23288	23288	23288	23288	21492	21492	21492	21492
	Panel B: Loan Origination Amount ($)							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	931.09*	1120.30**	1113.40***	1333.20***	1341.80***	1310.60***	1345.90***	1313.90***
	(481.22)	(549.39)	(397.25)	(345.16)	(491.30)	(401.96)	(508.31)	(419.29)
Enroll*For-profit	1404.80	1234.50	2434.80**	2226.80**	3171.70***	3203.40***	3230.80***	3262.90***
	(2112.60)	(2006.80)	(1207.40)	(1099.40)	(1020.60)	(937.01)	(985.02)	(892.79)
Time Fixed Effect 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	23288	23288	23288	23288	21492	21492	21492	21492
	Panel C: Number of Borrowers in Default (100%)							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.047	0.047	0.042	0.041	0.012***	0.013***	0.012***	0.012***
	(0.041)	(0.038)	(0.033)	(0.030)	(0.004)	(0.004)	(0.004)	(0.004)
Enroll*For-profit	0.107**	0.104**	0.099**	0.095**	0.069***	0.068***	0.069***	0.068***
	(0.054)	(0.051)	(0.042)	(0.040)	(0.011)	(0.011)	(0.011)	(0.010)
Time Fixed Effect 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	14922	14922	14922	14922	13729	13729	13729	13729
	Panel D: Number Employed, 6 years after enrollment							
	>25% Bachelors Degrees				>25th Percentile Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.851***	0.856***	0.852***	0.857***	0.826***	0.825***	0.828***	0.827***
	(0.028)	(0.025)	(0.027)	(0.024)	(0.032)	(0.025)	(0.034)	(0.027)
Enroll*For-profit	-0.130***	-0.137***	-0.128***	-0.134***	-0.144**	-0.142**	-0.140**	-0.138***
	(0.044)	(0.042)	(0.043)	(0.041)	(0.066)	(0.056)	(0.062)	(0.053)
Time Fixed Effect 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	9386	9386	9386	9386	8710	8710	8710	8710

Authors estimates of equation (5) in the text using for-profit and public institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA and institution-type (public or for-profit). Only four-year institutions are used in these estimates. In Columns (1)-(4), four-year institutions are those for which over 25% of all degrees awarded are Bachelors degrees. In Columns (5)-(8), four-year institutions are the top 75% of institutions in the distribution of the percent of degrees constituted by Bachelors degrees (among institutions that award any BA). 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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.

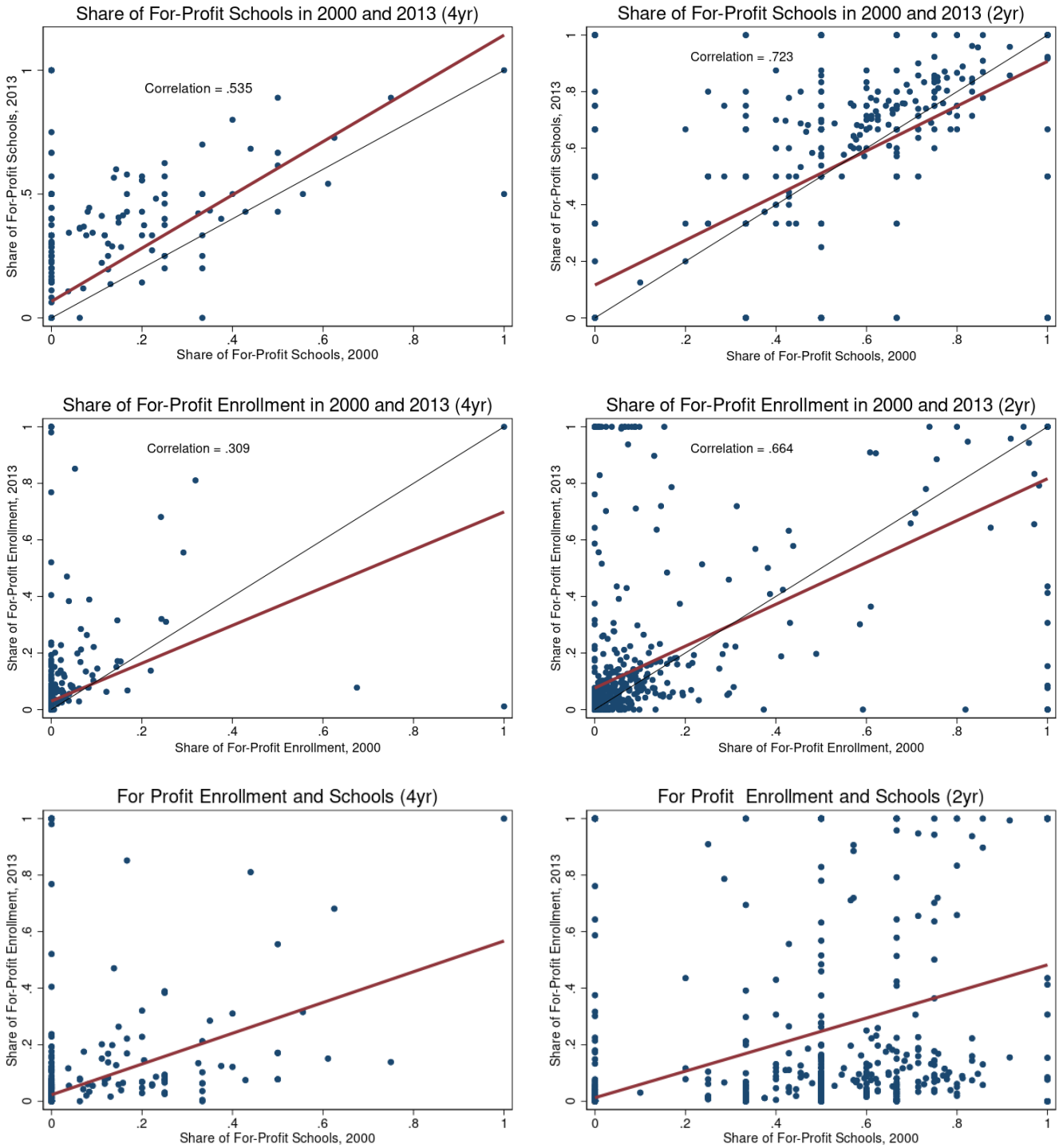
Table 11: Effects of Labor Demand Changes on Entry/Exit of Postsecondary Institutions, by Sector

Panel A: Average Effects	4 year schools				2 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.020***	-0.015*	-0.003	0.002	-0.023**	-0.002	0.003	0.001
	(0.006)	(0.008)	(0.003)	(0.003)	(0.011)	(0.013)	(0.004)	(0.005)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	11876	11876	11876	11876	10968	10968	10968	10968
R-squared	0.443	0.484	0.069	0.225	0.315	0.366	0.076	0.249
Mean of Dep. Var.	0.651	0.651	0.769	0.769	3.148	3.148	1.669	1.669
Panel B: Effects by FP Supply	4 year schools				2 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.020***	-0.014*	-0.002	0.003	-0.002	0.015	0.004	0.004
	(0.005)	(0.008)	(0.002)	(0.003)	(0.009)	(0.011)	(0.004)	(0.005)
(For-profit Supply)*($\hat{\eta}$)	-0.001	-0.001	-0.002	-0.001	-0.001*	-0.001*	-0.000	-0.000
	(0.004)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	11741	11741	11741	11741	10833	10833	10833	10833
R-squared	0.443	0.485	0.073	0.226	0.316	0.367	0.076	0.250
Mean of Dep. Var.	0.658	0.658	0.778	0.778	3.179	3.179	1.687	1.687

Authors estimates as described in the text using 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-year controls as described in the text as well as CBSA and year fixed effects. State-year fixed effects are included in even columns. 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: Not for Publication

Figure A-1: Correlation of For-profit Shares by CBSA, 2000 and 2013



Each panel in the figure shows a scatter plot of 2000 for-profit shares vs. 2013 for-profit shares. Each point is a CBSA. The top row shows institutional shares, while the middle row shows enrollment shares. The bottom row shows institutional shares in 2000 on the x-axis and enrollment shares in 2013 on the y-axis. The thin black line is the 45 degree line, while the thick red line is the linear fit.

Figure A-2a: Pre-2000 Trends by For-Profit Supply, Below and Above Median

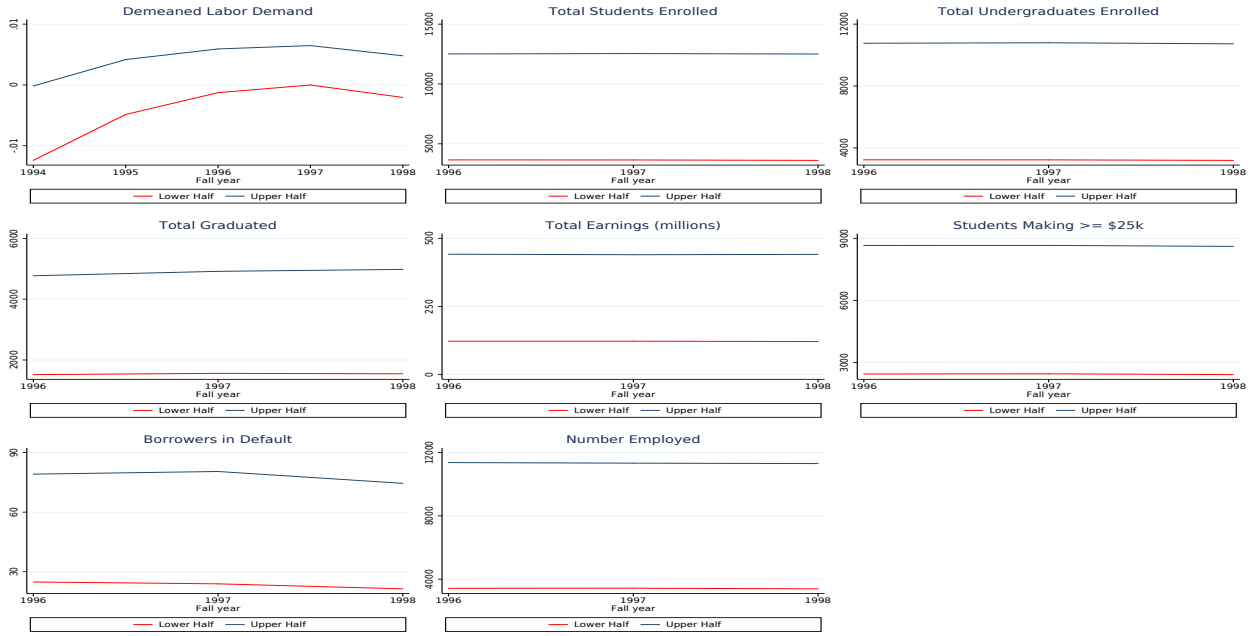
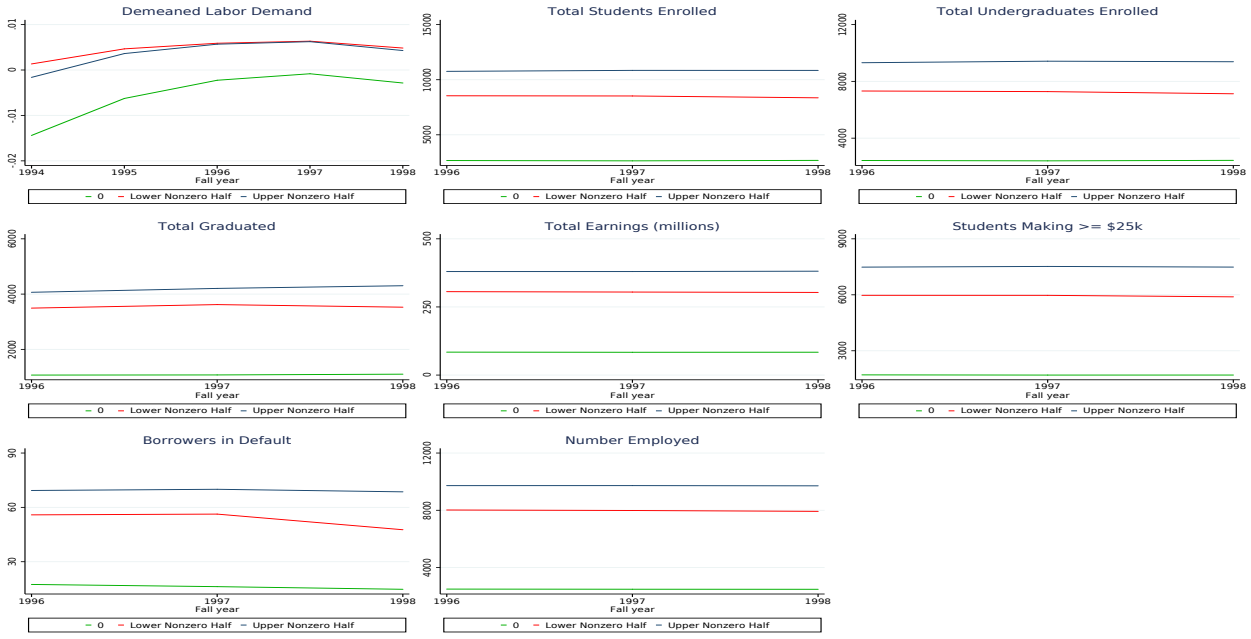


Figure A-2b: Pre-2000 Trends by For-Profit Supply, Three Groups



These figures show enrollment and outcome variables in the pre-2000 period for all institutions together without splitting into 2-year and 4-year schools. Panel A presents trends for CBSAs by whether they have a 2000 for-profit share that is below or above the median. Panel B presents trends for CBSAs by whether they have a zero, above median or below median for-profit share in 2000.

Figure A-3a: Pre-2000 Educational and Labor Market Outcome Trends by Labor Demand, Below and Above Median

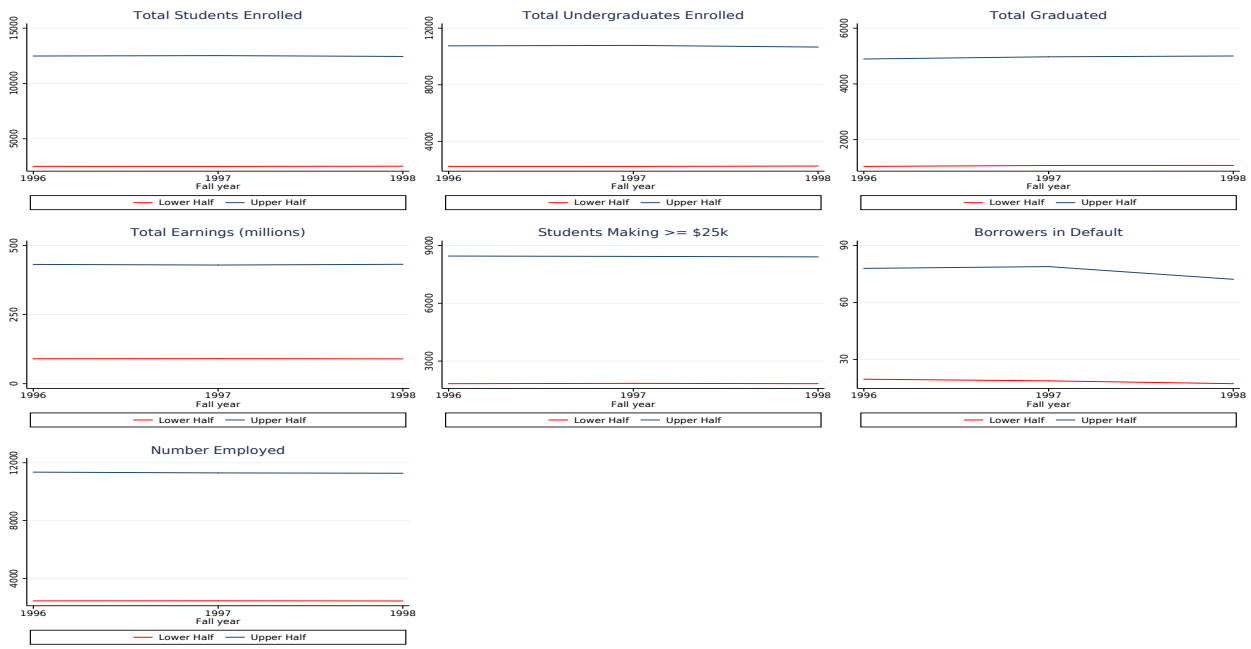
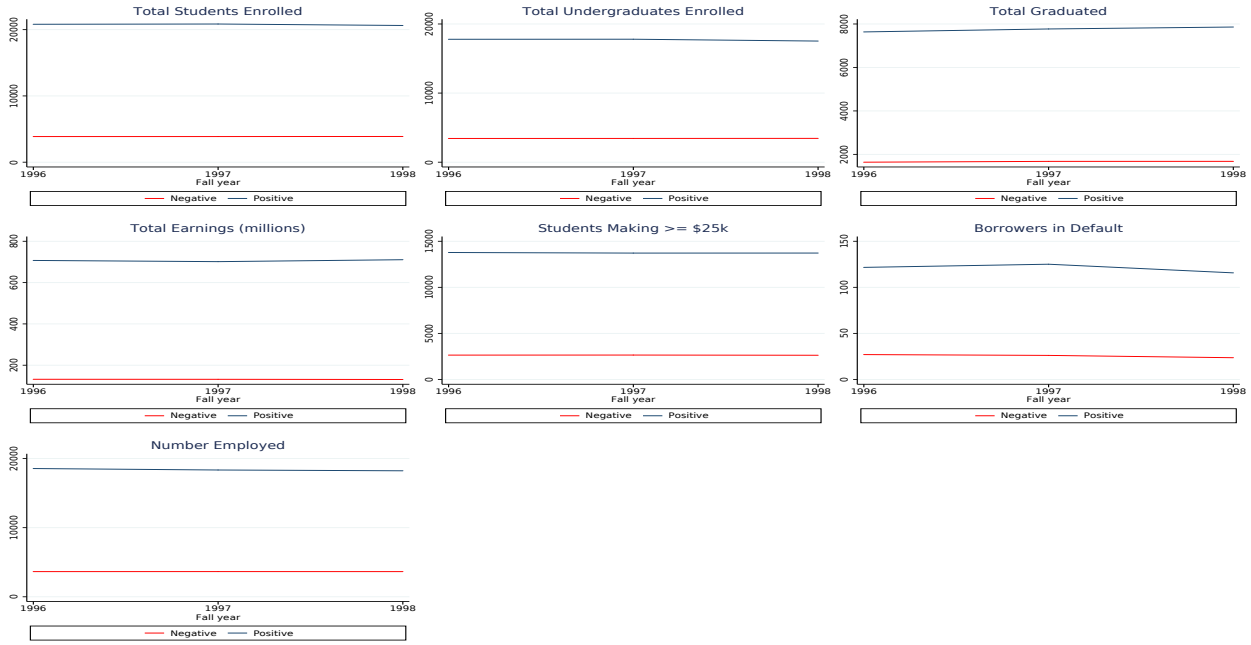
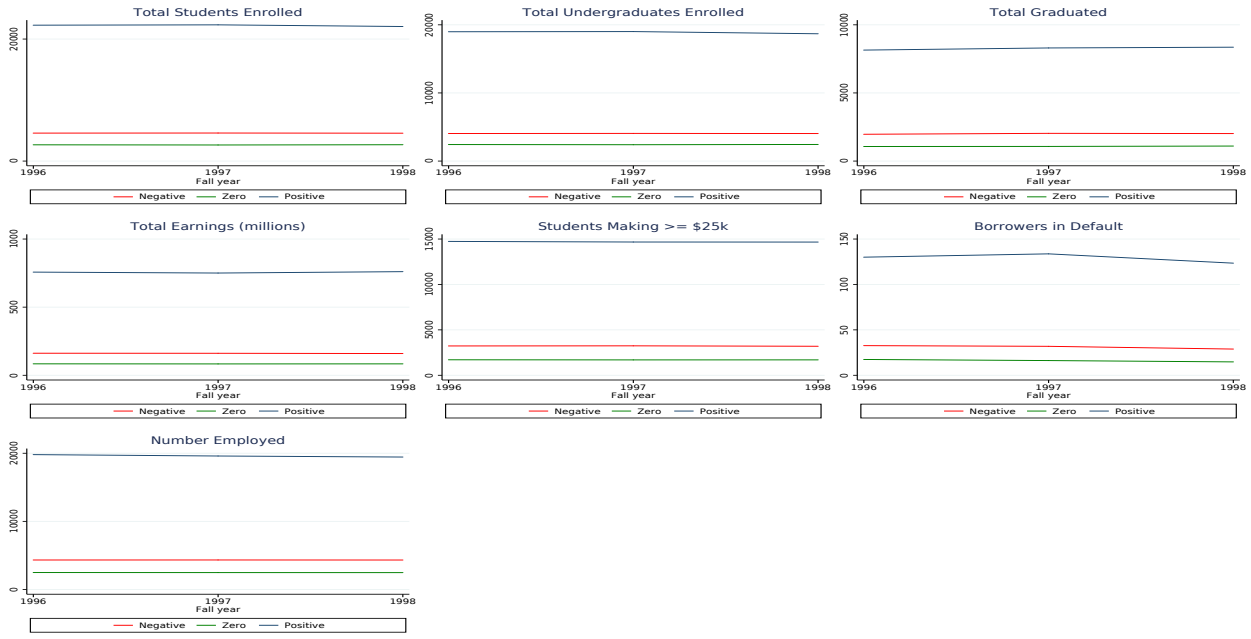


Figure A-3b: Pre-2000 Educational and Labor Market Outcome Trends by Labor Demand, Below and Above Zero



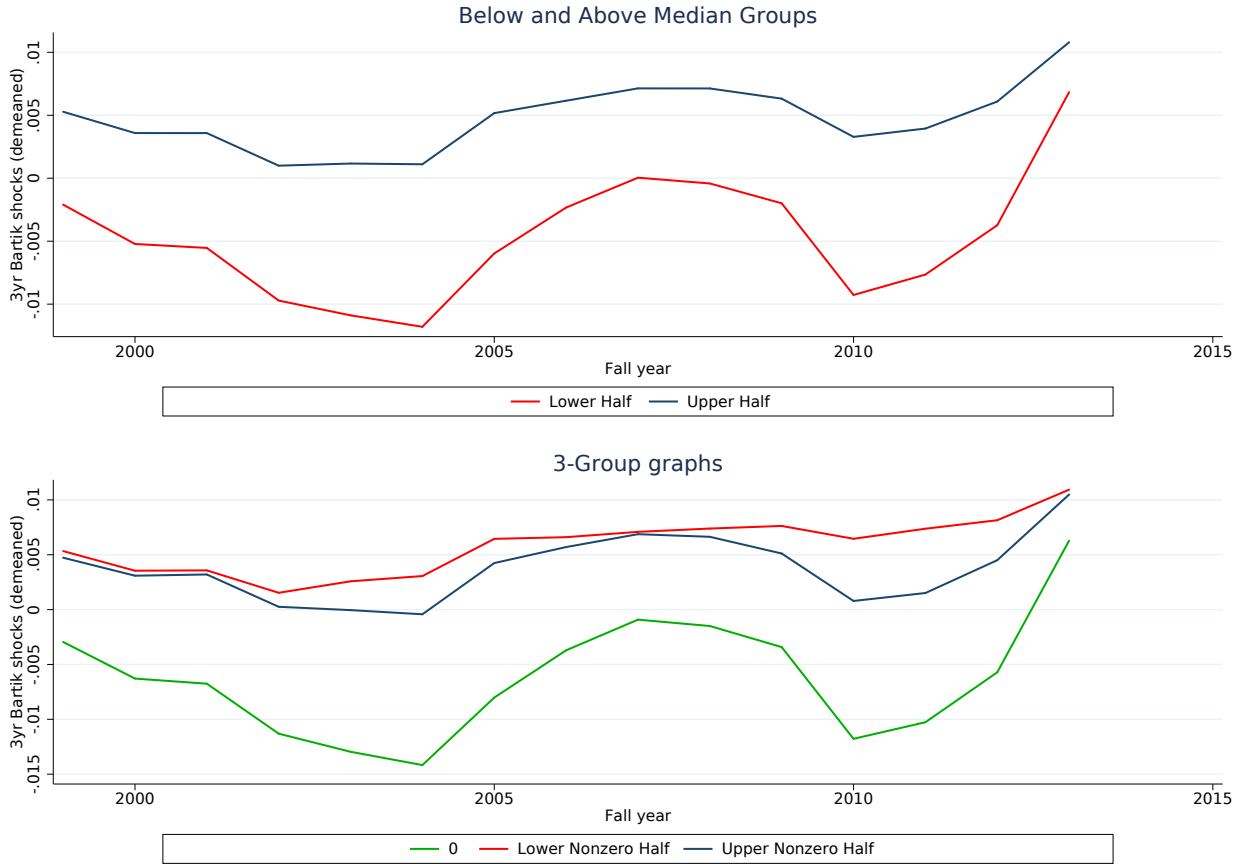
These figures show enrollment and outcome variables in the pre-2000 period for all institutions together without splitting into 2-year and 4-year schools. Panel A presents trends for CBSAs by whether they have a 2000 predicted labor demand shock that is below or above the median. Panel B presents trends for CBSAs by whether they have a negative, positive, or zero predicted labor demand shock in 2000.

Figure A-4: Pre-2000 Trends in Educational and Labor Market Outcomes by Labor Demand x For-Profit Supply, Below and Above Zero



The figure shows pre-2000 enrollment and outcome variable trends for all institutions together without splitting into 2-year and 4-year schools. The trends in each variable over time for CBSAs with negative and positive labor demand interacted with for-profit supply as measured in 2000 are shown.

Figure A-5: 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.

Table A-1: 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.

Table A-2: Summary Statistics: CBSA-level Variables (2000-2014)

Population Count (1000s)	344.531
	(1093.869)
Percentage Female	0.506
	(0.014)
Percentage White	0.850
	(0.140)
Percentage Black	0.100
	(0.131)
Percentage American Indian	0.015
	(0.045)
Percentage Asian	0.019
	(0.038)
Percentage Two or more races	0.017
	(0.019)
Percentage Hispanic	0.092
	(0.139)
Percentage Age 0-19 years	0.274
	(0.029)
Percentage Age 20-29 years	0.139
	(0.037)
Percentage Age 30-39 years	0.126
	(0.015)
Percentage Age 40+ years	0.460
	(0.056)
Poverty Rate	0.146
	(0.050)
Observations	47504

Means of variables reported in cells, with standard deviations in parentheses. Percentage and rate tabulations range from 0-1.

Table A-3: Descriptive Characteristics of CBSAs by For-Profit Supply Share

Characteristic	Zero For-Profit Schools	Below Median For-Profit Share	Above Median For-Profit Share
Percent Male	0.4931	0.4891	0.4941
	(0.0152)	(0.0091)	(0.0157)
Percent Female	0.5069	0.5109	0.5059
	(0.0152)	(0.0091)	(0.0157)
Percent White	0.8537	0.8551	0.8756
	(0.1612)	(0.1202)	(0.1118)
Percent Black	0.1031	0.1047	0.0810
	(0.1497)	(0.1181)	(0.1012)
Percent American Indian or Alaska Native	0.0176	0.0102	0.0112
	(0.0595)	(0.0268)	(0.0246)
Percent Asian or Pacific Islander	0.0133	0.0175	0.0191
	(0.0406)	(0.0265)	(0.0403)
Percent Two or more Races	0.0123	0.0125	0.0131
	(0.0222)	(0.0088)	(0.0139)
Percent Hispanic or Latino	0.0687	0.0625	0.1063
	(0.1184)	(0.1153)	(0.1613)
Age 0-19 years (Pct)	0.2868	0.2823	0.2896
	(0.0290)	(0.0242)	(0.0330)
Age 20-29 years (Pct)	0.1340	0.1393	0.1391
	(0.0370)	(0.0325)	(0.0361)
Age 30-39 years (Pct)	0.1362	0.1430	0.1424
	(0.0135)	(0.0129)	(0.0153)
Age 40 or more years (Pct)	0.4430	0.4354	0.4289
	(0.0509)	(0.0478)	(0.0564)
Poverty Rate	0.1312	0.1147	0.1208
	(0.0463)	(0.0355)	(0.0457)
Total Population (1000s)	68.341	491.205	561.192
	(54.156)	(1,465.857)	(1,265.283)

Note: Authors' tabulations from the 2000 US Census.

Table A-4: Regressions of FP supply on CBSA Demographics

	(1) Indicator of FP Supply	(2) % of FP Supply
Percent Female	4.959***	2.766***
	(3.81)	(3.79)
Percent Hispanic or Latino	0.345**	0.2526***
	(2.05)	(2.67)
Percent Black	-0.0485	-0.0465
	(-0.28)	(-0.48)
Percent American Indian or Alaska Native	-0.072	-0.0636
	(-0.19)	(-0.30)
Percent Asian or Pacific Islander	0.3721	0.1527
	(0.86)	(0.63)
Age 20-29 years (Pct)	0.4428	0.3867
	(0.50)	(0.78)
Age 30-39 years (Pct)	5.745***	3.134***
	(3.66)	(3.56)
Age 40 or more years (Pct)	0.2383	0.1703
	(0.37)	(0.47)
Poverty Rate	-0.6521	-0.3995
	(-1.16)	(-1.27)
Total Population	9.0e-08***	4.6e-08***
	(5.53)	(5.07)
Observations	792	792
R^2	0.128	0.120

Source: 2000 US Census. Each column is a separate regression.

t statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A-5: Summary Statistics: Student Composition (2000-2014)

	All	Four-year For-profit	Four-year Public	Two-year For-profit	Two-year Public
	(1)	(2)	(3)	(4)	(5)
% Undergraduates Black	13.490	19.343	10.974	16.457	11.803
	(16.706)	(17.679)	(16.044)	(19.418)	(14.007)
% Undergraduates Hispanic	8.098	8.456	7.172	9.423	7.661
	(14.115)	(11.260)	(13.132)	(16.378)	(13.374)
% Undergraduates White	68.239	51.820	69.623	67.019	71.457
	(22.663)	(23.101)	(20.893)	(25.625)	(19.761)
% Undergraduates Female	65.347	57.047	55.698	83.813	59.816
	(17.204)	(17.219)	(7.460)	(17.143)	(10.084)
% Undergraduates Over 25	31.296	50.109	20.531	29.500	35.692
	(20.408)	(25.874)	(13.567)	(21.564)	(17.742)
% Undergraduates Receiving Pell grant	48.013	62.454	34.988	64.438	40.776
	(20.046)	(16.829)	(12.354)	(17.053)	(16.169)
% Undergraduates Receiving Federal Loans	46.210	75.793	50.597	61.824	24.094
	(27.394)	(15.370)	(15.424)	(23.943)	(21.201)
% Undergraduates Sending FAFSA to 1 College	74.534	80.828	59.156	84.505	76.012
	(13.360)	(9.080)	(10.006)	(10.226)	(8.501)
% Undergraduates Sending FAFSA to 2 Colleges	15.930	13.263	20.996	12.620	15.550
	(7.663)	(7.228)	(4.911)	(9.503)	(5.895)
% Undergraduates Sending FAFSA to 3 Colleges	4.099	2.379	7.524	1.612	3.999
	(2.890)	(1.604)	(2.343)	(1.476)	(1.852)
% Undergraduates Sending FAFSA to 4 Colleges	2.049	1.162	4.403	0.495	1.830
	(1.973)	(1.159)	(2.105)	(0.675)	(1.073)
% Undergraduates Sending FAFSA to 5 Colleges	3.390	2.349	7.917	0.741	2.639
	(4.225)	(2.510)	(5.837)	(1.097)	(1.840)
% Parents with HS Diploma	36.661	16.318	33.439	34.213	44.652
	(17.849)	(16.174)	(12.423)	(20.300)	(14.417)
% Parents with MS	4.386	1.993	2.573	4.574	5.895
	(4.120)	(2.435)	(2.477)	(4.664)	(4.142)
% Parents with College Degree	39.978	17.895	56.763	28.500	42.080
	(21.278)	(17.736)	(22.197)	(16.824)	(14.115)
Average Family Income	37420.04	43060.09	59564.69	27826.09	29848.79
	(17741.83)	(1661.96)	(16787.21)	(10059.62)	(9365.87)
Average SAT score	1028.490	1010.418	1036.884	977.352	904.709
	(94.244)	(101.712)	(90.246)	(81.539)	(59.776)
% Arts degrees	36.157	40.771	60.632	8.339	39.950
	(26.645)	(26.099)	(13.915)	(15.870)	(20.451)
% STEM degrees	35.202	43.619	31.286	29.996	39.752
	(23.304)	(27.021)	(12.572)	(31.796)	(18.637)
% Vocational degrees	27.572	9.481	7.684	60.083	20.168
	(30.857)	(12.366)	(6.168)	(38.150)	(13.849)
% Associates Degrees	30.425	42.771	9.414	13.758	53.151
	(32.391)	(29.163)	(19.431)	(23.670)	(28.947)
% Bachelors Degrees	27.630	28.153	68.203	0.005	0.001
	(34.403)	(21.751)	(19.470)	(0.189)	(0.025)
% Certificates	44.126	10.531	1.872	86.237	46.848
	(39.358)	(16.136)	(5.978)	(23.680)	(28.947)
Observations	47504	11876	11876	11876	11876

Means of variables reported in cells, with standard deviations in parentheses. % tabulations range from 0-100.

Table A-6: First Stage Enrollment Estimates, Excluding Selective Institutions

Independent Variable	(1)	(2)	(3)	(4)
(For-profit Supply)*$\hat{\eta}$	13.746	26.385	24.566	37.205
	(45.594)	(42.005)	(42.704)	(41.812)
(For-profit)*(For-profit Supply)*$\hat{\eta}$	-210.380***	-210.380***	-232.020***	-232.020***
	(59.056)	(59.939)	(72.057)	(73.138)
	[-3.562]	[-3.510]	[-3.220]	[-3.172]
Time Fixed Effects	Year	State*Year	Year	State*Year
State*FP & Year*FP	No	No	Yes	Yes
Observations	23482	23482	23482	23482
R-squared	0.180	0.190	0.230	0.239
P-value[1]	0.002	0.002	0.006	0.007

Authors estimates of equation (4) in the text using 4-year for-profit and public institutions, excluding all institutions with a rating of "Highly Competitive" or higher in the 2001 Barron's rankings. 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. "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 4-year level in the CBSA in 2000. All regressions include the following CBSA-year 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 fixed effects, while estimates in the even columns include state-by-year fixed effects.

Table A-7: First Stage Enrollment Estimates - Horse Race Between Enrollment and Institutional For-Profit Measures

	4-year		2-year	
Independent Variable	(1)	(2)	(3)	(4)
(2000 Institutional FP Share)*$\hat{\eta}$	14.357	30.620	59.208***	60.368***
	(116.310)	(111.880)	(13.380)	(13.867)
(2000 Enrollment FP Share)*$\hat{\eta}$	-263.540	-270.040	-42.806***	-30.587***
	(350.450)	(319.720)	(10.568)	(10.404)
(For-profit)*(2000 Institutional FP Share)*$\hat{\eta}$	-25.449	-57.976	-113.010***	-115.340***
	(155.390)	(143.770)	(23.828)	(24.275)
(For-profit)*(2000 Enrollment FP Share)*$\hat{\eta}$	147.500	160.490	74.283***	49.847**
	(357.360)	(294.800)	(20.231)	(20.296)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	23178	23178	21292	21292

Authors estimates of a version of equation (4) in the text that uses both for-profit institutional shares and for-profit enrollment shares. 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. 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. All regressions include the following CBSA-year 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 state-year fixed effects.

Table A-8: OLS Estimates of the Effect of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.528***	0.530***	0.529***	0.530***	0.044***	0.056***	0.043***	0.054***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Enroll*For-profit	0.818***	0.821***	0.817***	0.820***	1.043***	1.097***	1.032***	1.086***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.010)	(0.010)	(0.010)	(0.010)
Time Fixed Effects	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	23752	23752	23752	23752	21936	21936	21936	21936
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	2664.60***	2680.00***	2672.70***	2688.70***	129.85***	166.81***	124.27***	160.92***
	(13.96)	(14.03)	(13.90)	(13.95)	(4.18)	(4.29)	(4.21)	(4.32)
Enroll*For-profit	3484.00***	3500.80***	3475.50***	3492.20***	3755.00***	3921.00***	3715.90***	3881.30***
	(20.18)	(20.34)	(20.19)	(20.33)	(31.47)	(31.23)	(31.66)	(31.41)
Time Fixed Effects	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	23752	23752	23752	23752	21936	21936	21936	21936
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.004***	0.003***	0.004***	0.003***	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Enroll*For-profit	0.023***	0.022***	0.024***	0.023***	0.041***	0.043***	0.040***	0.042***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Time Fixed Effects	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	15463	15463	15463	15463	14616	14616	14616	14616

Authors estimates of equation (5) in the text by OLS using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-9: OLS Estimates of the Effect of For-profit Attendance on Student Borrowing, by Loan Type

	Panel A: Number Recipients: Direct Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.168***	0.168***	0.168***	0.168***	0.026***	0.031***	0.025***	0.030***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Enroll*For-profit	0.102***	0.107***	0.101***	0.106***	0.277***	0.300***	0.273***	0.296***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)	(0.005)	(0.006)
Time Fixed Effect 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	23752	23752	23752	23752	21936	21936	21936	21936
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.149***	0.149***	0.149***	0.150***	0.021***	0.026***	0.021***	0.025***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Enroll*For-profit	0.131***	0.136***	0.131***	0.136***	0.270***	0.291***	0.266***	0.287***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.005)
Time Fixed Effects	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	23752	23752	23752	23752	21936	21936	21936	21936
	Panel C: Number Recipients: FFEL Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.127***	0.129***	0.127***	0.129***	0.003***	0.005***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Enroll*For-profit	0.388***	0.386***	0.388***	0.386***	0.264***	0.275***	0.261***	0.272***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)
Time Fixed Effects	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	17426	17426	17426	17426	16082	16082	16082	16082
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.100***	0.102***	0.100***	0.102***	0.000	0.002***	-0.000	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Enroll*For-profit	0.396***	0.395***	0.396***	0.395***	0.227***	0.239***	0.224***	0.236***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
Time Fixed Effects	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	17426	17426	17426	17426	16082	16082	16082	16082

Authors estimates of equation (5) in the text by OLS using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-10: OLS Estimates of the Effect of For-profit Attendance on Employment, Earnings, and Graduation

	Panel A: Total Graduated, assuming 150% Completion Time			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.329***	0.328***	0.274***	0.268***
	(0.003)	(0.003)	(0.001)	(0.001)
Enroll*For-profit	-0.246***	-0.250***	0.750***	0.726***
	(0.004)	(0.004)	(0.009)	(0.009)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	19022	19022	17560	17560
	Panel B: Number Employed, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.908***	0.908***	0.788***	0.791***
	(0.001)	(0.001)	(0.001)	(0.001)
Enroll*For-profit	-0.043***	-0.046***	-0.337***	-0.325***
	(0.002)	(0.002)	(0.007)	(0.007)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9516	9516	8774	8774
	Panel C: Total Earnings, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	37591.40***	37485.30***	23900.90***	24042.30***
	(66.97)	(66.74)	(47.55)	(50.62)
Enroll*For-profit	-23.15	-226.55	-11743.30***	-10920.40***
	(156.89)	(158.16)	(434.20)	(443.13)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9516	9516	8774	8774
	Panel D: Total Students Making $25k, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.710***	0.708***	0.505***	0.507***
	(0.001)	(0.001)	(0.001)	(0.001)
Enroll*For-profit	-0.024***	-0.027***	-0.229***	-0.212***
	(0.002)	(0.002)	(0.009)	(0.009)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9516	9516	8774	8774

Authors estimates of equation (5) in the text by OLS using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-11: Instrumental Variables Analysis Estimates of the Impact of For-profit Attendance on Outcomes by Percent of Bachelors Degrees Awarded

	Panel A: Number of Loans							
	25-50% Bachelors Degrees				75% or more Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.413	0.489	0.399	0.477	0.418***	0.487***	0.432***	0.504***
	(0.683)	(0.518)	(0.688)	(0.522)	(0.138)	(0.106)	(0.142)	(0.108)
Enroll*For-profit	1.290**	1.216***	1.315**	1.238***	0.781**	0.725**	0.889***	0.825***
	(0.603)	(0.436)	(0.610)	(0.444)	(0.368)	(0.357)	(0.295)	(0.269)
Time Fixed Effects	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	20642	20642	20642	20642	15418	15418	15418	15418
	Panel B: Loan Origination Amount ($)							
	25-50% Bachelors Degrees				75% or more Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	3867.20	3639.40	3797.20	3581.60	969.65	1132.20	1166.80**	1381.60**
	(3145.50)	(2417.70)	(3115.20)	(2392.50)	(913.79)	(1175.60)	(570.19)	(614.38)
Enroll*For-profit	2247.30	2469.20	2368.50	2578.70	-416.79	-550.02	1115.80	925.24
	(2733.30)	(2035.70)	(2712.90)	(2018.60)	(5591.30)	(5239.70)	(2596.50)	(2400.20)
Time Fixed Effects	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	20642	20642	20642	20642	15418	15418	15418	15418
	Panel C: Number of Borrowers in Default (100%)							
	25-50% Bachelors Degrees				75% or more Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.065	0.049	0.066	0.048	0.044	0.042	0.034	0.032
	(0.195)	(0.144)	(0.187)	(0.138)	(0.058)	(0.047)	(0.035)	(0.030)
Enroll*For-profit	0.063	0.077	0.057	0.070	0.145	0.129	0.118	0.108
	(0.118)	(0.078)	(0.112)	(0.074)	(0.133)	(0.103)	(0.076)	(0.066)
Time Fixed Effects	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	13856	13856	13856	13856	9654	9654	9654	9654
	Panel D: Number Employed, 6 years after enrollment							
	25-50% Bachelors Degrees				75% or more Bachelors Degrees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1.054***	1.022***	1.060***	1.024***	0.843***	0.852***	0.837***	0.846***
	(0.234)	(0.157)	(0.226)	(0.151)	(0.039)	(0.033)	(0.045)	(0.037)
Enroll*For-profit	-0.149	-0.119	-0.171	-0.138	-0.184	-0.196*	-0.203	-0.215*
	(0.331)	(0.257)	(0.295)	(0.223)	(0.113)	(0.103)	(0.126)	(0.118)
Time Fixed Effects	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	8382	8382	8382	8382	6182	6182	6182	6182

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.

Table A-12: Instrumental Variables Estimates of the Effect 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							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.441***	0.468***	0.453***	0.477***	0.103	0.124	0.083	0.120
	(0.138)	(0.112)	(0.157)	(0.138)	(0.119)	(0.147)	(0.197)	(0.300)
Enroll*For-profit	1.016***	0.983***	1.078***	1.056***	1.391**	1.518*	1.276	1.491
	(0.265)	(0.252)	(0.276)	(0.267)	(0.674)	(0.909)	(1.136)	(1.901)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1438.80***	1700.70***	1502.70***	1754.20***	617.12	759.67	782.95	1211.30
	(411.06)	(381.13)	(432.22)	(320.68)	(376.36)	(480.96)	(616.15)	(1279.10)
Enroll*For-profit	2954.20**	2631.70**	3291.30***	3065.00***	6566.70***	7609.70**	7552.10**	10524.90
	(1175.60)	(1151.90)	(1107.60)	(1050.10)	(2198.60)	(3164.20)	(3720.70)	(8599.30)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.066*	0.062**	0.060*	0.055*	0.026*	0.044	0.040	0.201
	(0.036)	(0.031)	(0.036)	(0.030)	(0.014)	(0.042)	(0.047)	(1.275)
Enroll*For-profit	0.109***	0.114***	0.104***	0.106***	0.211**	0.350	0.311	1.485
	(0.037)	(0.039)	(0.039)	(0.040)	(0.101)	(0.314)	(0.338)	(9.290)
Time Fixed Effects	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	15347	15347	15347	15347	14500	14500	14500	14500

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects. All 2-year estimates include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all 4-year estimates include %Bachelors Degrees awarded in the baseline year interacted with year dummies. 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-13: Instrumental Variables Estimates of the Effect of For-profit Attendance on Student Borrowing, by Loan Type Including Baseline Degree Composition X Year Controls

	Panel A: Number Recipients: Direct Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.343**	0.329**	0.353**	0.338*	0.011	0.006	-0.024	-0.056
	(0.154)	(0.158)	(0.177)	(0.182)	(0.121)	(0.155)	(0.209)	(0.371)
Enroll*For-profit	0.613**	0.634**	0.673**	0.696**	0.183	0.139	-0.025	-0.258
	(0.276)	(0.275)	(0.282)	(0.290)	(0.714)	(0.995)	(1.244)	(2.409)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Number Recipients: Direct Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.297**	0.290**	0.310**	0.301**	0.020	0.020	-0.013	-0.031
	(0.122)	(0.116)	(0.144)	(0.141)	(0.106)	(0.134)	(0.184)	(0.316)
Enroll*For-profit	0.519**	0.532**	0.590**	0.608**	0.241	0.233	0.048	-0.096
	(0.215)	(0.220)	(0.234)	(0.244)	(0.623)	(0.853)	(1.093)	(2.047)
Time Fixed Effects	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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel C: Number Recipients: FFEL Subsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.155**	0.165**	0.174**	0.184**	-0.012	-0.012	-0.065	-0.105
	(0.069)	(0.065)	(0.084)	(0.078)	(0.031)	(0.044)	(0.070)	(0.163)
Enroll*For-profit	0.457***	0.444***	0.485***	0.476***	0.115	0.103	-0.237	-0.541
	(0.117)	(0.111)	(0.135)	(0.132)	(0.196)	(0.287)	(0.458)	(1.134)
Time Fixed Effects	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	17290	17290	17290	17290	15946	15946	15946	15946
	Panel D: Number Recipients: FFEL Unsubsidized Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.095*	0.106**	0.109*	0.120**	0.007	0.015	-0.023	-0.032
	(0.050)	(0.046)	(0.060)	(0.055)	(0.024)	(0.034)	(0.044)	(0.085)
Enroll*For-profit	0.414***	0.401***	0.435***	0.425***	0.241	0.275	0.041	-0.045
	(0.074)	(0.072)	(0.085)	(0.085)	(0.149)	(0.225)	(0.280)	(0.575)
Time Fixed Effects	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	17290	17290	17290	17290	15946	15946	15946	15946

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects. All 2-year estimates include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all 4-year estimates include %Bachelors Degrees awarded in the baseline year interacted with year dummies. 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-14: Instrumental Variables Estimates of the Effect of For-profit Attendance on Employment, Earnings, and Graduation, Including Baseline Degree Composition X Year Controls

	Panel A: Total Graduated, assuming 150% Completion Time			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.418***	0.417***	0.164	0.144
	(0.112)	(0.106)	(0.119)	(0.161)
Enroll*For-profit	-0.099	-0.107	0.236	0.034
	(0.276)	(0.279)	(0.816)	(1.171)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	18854	18854	17392	17392
	Panel B: Number Employed, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.858***	0.872***	0.803***	0.803***
	(0.030)	(0.024)	(0.096)	(0.102)
Enroll*For-profit	-0.096**	-0.110**	-0.336	-0.329
	(0.046)	(0.048)	(0.753)	(0.809)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710
	Panel C: Total Earnings, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	36024.90***	37247.40***	25876.80***	25965.80***
	(3392.30)	(3014.90)	(6390.90)	(6899.90)
Enroll*For-profit	-4976.50	-6182.40	-9741.60	-7630.60
	(6280.90)	(6600.20)	(50441.00)	(55001.50)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710
	Panel D: Total Students Making $25k, 6 years after enrollment			
	4-year Schools		2-year Schools	
	(1)	(2)	(3)	(4)
Enroll	0.702***	0.719***	0.620***	0.626***
	(0.051)	(0.045)	(0.197)	(0.221)
Enroll*For-profit	-0.070	-0.086	0.355	0.452
	(0.100)	(0.108)	(1.526)	(1.720)
Time Fixed Effects	State*Year	State*Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	9452	9452	8710	8710

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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. Even columns include state-for profit and year-for profit fixed effects. All 2-year estimates include %Certificates Degrees awarded in the baseline year interacted with year dummies, while all 4-year estimates include %Bachelors Degrees awarded in the baseline year interacted with year dummies. 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-15: Instrumental Variables Estimates of the Effect of For-profit Attendance on Outcomes, Including Opposite-Level Supply

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.300	0.411*	0.312	0.423	0.121	0.114	0.124	0.115
	(0.360)	(0.241)	(0.399)	(0.288)	(0.123)	(0.114)	(0.191)	(0.174)
Enroll*For-profit	1.308**	1.148***	1.366**	1.230***	1.351**	1.369**	1.364	1.373
	(0.550)	(0.386)	(0.545)	(0.414)	(0.554)	(0.584)	(0.899)	(0.926)
Time Fixed Effects	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	21666	21666	21666	21666
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	403.87	1081.40	462.88	1140.20	688.35*	641.45*	944.31	862.62
	(1531.70)	(847.34)	(1707.90)	(1019.60)	(399.20)	(369.06)	(690.25)	(615.32)
Enroll*For-profit	5101.00**	4132.40***	5386.10**	4552.40***	6208.00***	6323.80***	7476.00**	7558.90**
	(2405.60)	(1565.40)	(2374.60)	(1610.00)	(1854.00)	(1952.40)	(3393.80)	(3426.60)
Time Fixed Effects	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	21666	21666	21666	21666
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.088*	0.080*	0.075	0.067*	0.027*	0.026*	0.054	0.051
	(0.051)	(0.043)	(0.048)	(0.040)	(0.016)	(0.015)	(0.101)	(0.091)
Enroll*For-profit	0.069*	0.080**	0.065*	0.074**	0.195**	0.199**	0.356	0.358
	(0.040)	(0.035)	(0.034)	(0.032)	(0.096)	(0.098)	(0.606)	(0.593)
Time Fixed Effects	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	14158	14158	14158	14158	14500	14500	14500	14500
	Panel D: Number Employed, 6 years after enrollment							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.822***	0.849***	0.820***	0.848***	0.790***	0.790***	0.827***	0.826***
	(0.055)	(0.038)	(0.062)	(0.044)	(0.185)	(0.188)	(0.083)	(0.082)
Enroll*For-profit	-0.042	-0.074	-0.043	-0.074	-0.442	-0.429	-0.195	-0.174
	(0.058)	(0.046)	(0.059)	(0.049)	(1.228)	(1.335)	(0.548)	(0.577)
Time Fixed Effects	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	8710	8710	8710	8710	8710	8710	8710

Authors estimates of equation (5) in the text using for-profit and public 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects. Four-year estimates include controls for two-year for-profit supply fully interacted with FP and $\hat{\eta}$, while two-year estimates control for four-year for-profit supply fully interacted with FP and $\hat{\eta}$. 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-16: Instrumental Variables Estimates of the Impact of For-profit Attendance on Outcomes, Excluding Online Institutions

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.467***	0.518***	0.511***	0.568***	0.064	0.067	0.012	0.019
	(0.150)	(0.120)	(0.172)	(0.137)	(0.127)	(0.122)	(0.231)	(0.218)
Enroll*For-profit	1.482***	1.358***	1.658***	1.565***	1.147	1.130	0.841	0.824
	(0.452)	(0.402)	(0.452)	(0.409)	(0.724)	(0.744)	(1.351)	(1.367)
Time Fixed Effect 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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1243.80	1560.50**	1524.00*	1870.70***	509.94	508.40	593.62	587.79
	(774.05)	(679.42)	(780.48)	(612.70)	(377.97)	(363.75)	(600.59)	(571.58)
Enroll*For-profit	3885.10	3127.50	4985.90**	4416.60**	5898.20***	5905.80***	6396.00*	6410.40*
	(2609.30)	(2447.30)	(2272.00)	(2062.30)	(2168.60)	(2243.30)	(3546.70)	(3623.20)
Time Fixed Effect 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	23482	23482	23482	23482	21666	21666	21666	21666
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.034*	0.032*	0.032	0.030	0.026	0.026	0.043	0.043
	(0.019)	(0.019)	(0.022)	(0.020)	(0.016)	(0.016)	(0.064)	(0.063)
Enroll*For-profit	0.102**	0.104**	0.098*	0.098*	0.214*	0.214*	0.336	0.336
	(0.050)	(0.051)	(0.055)	(0.055)	(0.116)	(0.117)	(0.462)	(0.463)
Time Fixed Effect 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	15347	15347	15347	15347	14500	14500	14500	14500
	Panel D: Number Employed, 6 years after enrollment							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.820***	0.835***	0.816***	0.832***	0.605	0.603	0.800***	0.800***
	(0.066)	(0.058)	(0.079)	(0.069)	(1.099)	(1.120)	(0.099)	(0.099)
Enroll*For-profit	-0.247	-0.270	-0.258	-0.278	-1.872	-1.897	-0.359	-0.356
	(0.152)	(0.169)	(0.186)	(0.198)	(8.442)	(8.664)	(0.778)	(0.783)
Time Fixed Effect 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	9452	9452	9452	9452	8710	8710	8710	8710

Authors estimates of equation (5) in the text using for-profit and public institutions, excluding institutions classified as online at the start of the sample period in 1999-2000. We use the IPEDS definition of a fully online institution (where all programs are offered via distance education), which it began reporting in 2011. We exclude from the entire analysis sample the 60 institutions that were listed as fully online between 2011 and 2014. Using the method in Deming, Goldin and Katz (2012) that employs a threshold for out-of-state enrollment share yields similar results. IPEDS also reports an alternative measure that identifies schools that offer only some programs or courses (but not all) via distance education. We do not use this measure as a large part of the enrollment of these "some online" schools comes from local enrollment. Excluding these schools would lead us to exclude much of our relevant local enrollment variation. 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-17: Instrumental Variables Estimates of the Impact of For-profit Attendance on Outcomes Excluding Post-2006 Cohorts

	Panel A: Number of Loans							
	Year < 2006				Year ≤ 2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.513***	0.527***	0.546***	0.555***	0.467***	0.494***	0.495***	0.518***
	(0.133)	(0.124)	(0.165)	(0.146)	(0.137)	(0.127)	(0.157)	(0.139)
Enroll*For-profit	1.132***	1.116***	1.206***	1.197***	1.069***	1.039***	1.125***	1.101***
	(0.399)	(0.386)	(0.459)	(0.455)	(0.363)	(0.353)	(0.386)	(0.381)
Time Fixed Effect 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	11044	11044	11044	11044	12606	12606	12606	12606
	Panel B: Loan Origination Amount ($)							
	Year < 2006				Year ≤ 2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	2179.70***	2231.90***	2359.00***	2383.80***	1952.50***	2075.80***	2098.80***	2201.60***
	(593.38)	(509.36)	(737.96)	(624.75)	(585.25)	(495.90)	(684.12)	(567.20)
Enroll*For-profit	5151.90***	5091.60***	5556.60***	5529.80***	4753.10***	4618.70***	5040.20***	4934.20***
	(1657.00)	(1597.30)	(1993.00)	(1969.40)	(1453.10)	(1403.00)	(1612.30)	(1582.20)
Time Fixed Effect 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	11044	11044	11044	11044	12606	12606	12606	12606
	Panel C: Number of Borrowers in Default (100%)							
	Year < 2006				Year ≤ 2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.023***	0.021***	0.023***	0.022***	0.017***	0.017***	0.017**	0.016***
	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Enroll*For-profit	0.088***	0.089***	0.088***	0.090***	0.077***	0.078***	0.076***	0.076***
	(0.012)	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)	(0.011)	(0.012)
Time Fixed Effect 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	11044	11044	11044	11044	12606	12606	12606	12606
	Panel D: Number Employed, 6 years after enrollment							
	Year < 2006				Year ≤ 2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.859***	0.875***	0.859***	0.875***	0.859***	0.875***	0.859***	0.875***
	(0.027)	(0.020)	(0.029)	(0.022)	(0.027)	(0.020)	(0.029)	(0.022)
Enroll*For-profit	-0.093**	-0.112**	-0.093**	-0.110**	-0.093**	-0.112**	-0.093**	-0.110**
	(0.042)	(0.047)	(0.044)	(0.048)	(0.042)	(0.047)	(0.044)	(0.048)
Time Fixed Effect 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	9452	9452	9452	9452	9452	9452	9452	9452

Authors estimates of equation (5) in the text using for-profit and public institutions and cohorts that enrolled after 2006. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit). 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects.

Table A-18: Instrumental Variables Estimates of the Impact of For-profit Attendance on Outcomes, Excluding Chains

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.450***	0.497***	0.483**	0.514***	0.102	0.091	0.051	0.057
	(0.177)	(0.142)	(0.190)	(0.166)	(0.110)	(0.115)	(0.172)	(0.165)
Enroll*For-profit	2.296	2.165*	2.462*	2.382*	1.249	1.022	0.582	0.558
	(1.406)	(1.220)	(1.323)	(1.282)	(1.333)	(1.476)	(2.193)	(2.204)
Time Fixed Effect 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	22872	22872	22872	22872	20944	20944	20944	20944
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1607.20**	1912.00***	1817.50**	2002.10***	550.99	519.09	513.37	517.37
	(728.28)	(551.24)	(735.38)	(577.08)	(338.57)	(341.10)	(441.94)	(428.40)
Enroll*For-profit	5736.10	5576.90	7255.80	6770.50	7530.00*	7042.50	7035.00	7019.20
	(6695.80)	(6069.80)	(5807.80)	(5694.40)	(4163.50)	(4334.40)	(5515.60)	(5562.10)
Time Fixed Effect 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	22594	22594	22594	22594	20944	20944	20944	20944
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.007	0.007	0.006	0.007	0.027	0.027	0.035	0.035
	(0.006)	(0.006)	(0.005)	(0.005)	(0.024)	(0.023)	(0.055)	(0.054)
Enroll*For-profit	0.065	0.062	0.057	0.396	0.390	0.214*	0.519	0.519
	(0.042)	(0.039)	(0.038)	(0.037)	(0.389)	(0.384)	(0.866)	(0.870)
Time Fixed Effect 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	14735	14735	14735	14735	14028	14028	14028	14028

Authors estimates of equation (5) in the text using for-profit and public institutions, excluding institutions classified as chains. 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-19: Instrumental Variables Estimates of the Effect of For-profit Attendance on the Number and Volume of Student Loan Originations and Defaults, Excluding Selective Institutions

	Panel A: Number of Loans			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.449***	0.501***	0.467**	0.515***
	(0.167)	(0.124)	(0.201)	(0.164)
Enroll*For-profit	1.120***	1.044***	1.214***	1.156***
	(0.311)	(0.257)	(0.339)	(0.297)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	23432	23432	23432	23432
	Panel B: Loan Origination Amount ($)			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	1531.30***	1906.80***	1626.80***	1979.70***
	(465.52)	(290.02)	(590.00)	(338.95)
Enroll*For-profit	3591.40***	3045.20***	4083.70***	3646.00***
	(1141.40)	(1008.80)	(1191.00)	(988.24)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	23432	23432	23432	23432
	Panel C: Number of Borrowers in Default (100%)			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.065*	0.059*	0.057*	0.052*
	(0.037)	(0.031)	(0.034)	(0.028)
Enroll*For-profit	0.108***	0.116***	0.100***	0.105***
	(0.039)	(0.042)	(0.038)	(0.041)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	15326	15326	15326	15326

Authors estimates of equation (5) in the text using for-profit and public institutions, excluding all institutions with a rating of "Highly Competitive" or higher in the 2001 Barron's rankings. 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-20: Instrumental Variables Estimates of the Effect of For-profit Attendance on Borrowing, by Loan Type Excluding Selective Institutions

	Panel A: Number Recipients: Direct Subsidized Loans			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.352**	0.340**	0.363**	0.347*
	(0.162)	(0.157)	(0.183)	(0.177)
Enroll*For-profit	0.608**	0.625**	0.664**	0.684**
	(0.303)	(0.294)	(0.314)	(0.315)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	23432	23432	23432	23432
	Panel B: Number Recipients: Direct Unsubsidized Loans			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.308**	0.305**	0.322**	0.314**
	(0.133)	(0.120)	(0.155)	(0.144)
Enroll*For-profit	0.536**	0.541**	0.606**	0.616**
	(0.242)	(0.236)	(0.266)	(0.267)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	23432	23432	23432	23432
	Panel C: Number Recipients: FFEL Subsidized Loans			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.154**	0.163***	0.172**	0.179**
	(0.068)	(0.061)	(0.080)	(0.070)
Enroll*For-profit	0.461***	0.450***	0.487***	0.479***
	(0.116)	(0.113)	(0.130)	(0.130)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	17256	17256	17256	17256
	Panel D: Number Recipients: FFEL Unsubsidized Loans			
	4-year Schools			
	(1)	(2)	(3)	(4)
Enroll	0.099**	0.111**	0.113*	0.123**
	(0.050)	(0.044)	(0.058)	(0.050)
Enroll*For-profit	0.419***	0.405***	0.440***	0.428***
	(0.076)	(0.076)	(0.086)	(0.087)
Time Fixed Effects	Year	Year	State*Year	State*Year
State*FP & Year*FP	No	Yes	No	Yes
Observations	17256	17256	17256	17256

Authors estimates of equation (5) in the text using for-profit and public institutions, excluding all institutions with a rating of "Highly Competitive" or higher in the 2001 Barron's rankings. 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include 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 A-21: Instrumental Variables Estimates of the Effect of For-profit Attendance on Graduation, Employment, and Earnings, Excluding Selective Institutions

	Panel A: Total Graduated, assuming 150% Completion Time	
	4-year Schools	
	(1)	(2)
Enroll	0.368***	0.363***
	(0.082)	(0.070)
Enroll*For-profit	-0.215	-0.211
	(0.215)	(0.226)
Time Fixed Effects	State*Year	State*Year
State*FP & Year*FP	No	Yes
Observations	18816	18816
	Panel B: Number Employed, 6 years after enrollment	
	4-year Schools	
	(1)	(2)
Enroll	0.858***	0.873***
	(0.029)	(0.022)
Enroll*For-profit	-0.095**	-0.112**
	(0.044)	(0.048)
Time Fixed Effects	State*Year	State*Year
State*FP & Year*FP	No	Yes
Observations	9436	9436
	Panel C: Total Earnings, 6 years after enrollment	
	4-year Schools	
	(1)	(2)
Enroll	35379.70***	36156.00***
	(3399.90)	(2866.50)
Enroll*For-profit	-5657.80	-6486.70
	(6402.00)	(6665.10)
Time Fixed Effects	State*Year	State*Year
State*FP & Year*FP	No	Yes
Observations	9436	9436
	Panel D: Total Students Making $25k, 6 years after enrollment	
	4-year Schools	
	(1)	(2)
Enroll	0.694***	0.705***
	(0.051)	(0.043)
Enroll*For-profit	-0.084	-0.095
	(0.099)	(0.107)
Time Fixed Effect Type	State*Year	State*Year
State*FP & Year*FP	No	Yes
Observations	9436	9436

Authors estimates of equation (5) in the text using for-profit and public institutions, excluding all institutions with a rating of "Highly Competitive" or higher in the 2001 Barron's rankings. 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-year level variables as controls: base-year $(t-3)$ 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. Even columns include state-for profit and year-for profit 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-22: Instrumental Variables Estimates of the Impact of For-profit Attendance on Outcomes, Including Not-for-profit Institutions

	Panel A: Number of Loans							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.491***	0.570***	0.516***	0.588***	0.083	0.086	0.012	0.019
	(0.141)	(0.136)	(0.127)	(0.097)	(0.174)	(0.169)	(0.362)	(0.345)
Enroll*For-profit	0.612**	0.501	0.697**	0.609**	1.254***	1.238***	1.073	1.054
	(0.303)	(0.325)	(0.288)	(0.288)	(0.420)	(0.443)	(0.897)	(0.928)
Time Fixed Effect 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	35223	35223	35223	35223	32499	32499	32499	32499
	Panel B: Loan Origination Amount ($)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	1377.80	2303.90	1571.80	2449.30	718.16	715.67	939.07	926.71
	(2301.40)	(2765.60)	(1935.40)	(2279.40)	(519.22)	(503.26)	(993.27)	(947.49)
Enroll*For-profit	-1686.60	-2995.20	-1027.60	-2109.10	5437.00***	5449.80***	5992.60**	6025.90**
	(3745.60)	(4097.70)	(3080.60)	(3382.90)	(1320.70)	(1406.70)	(2577.00)	(2696.70)
Time Fixed Effect 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	35223	35223	35223	35223	32499	32499	32499	32499
	Panel C: Number of Borrowers in Default (100%)							
	4-year Schools				2-year Schools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enroll	0.085*	0.073**	0.075*	0.064**	0.038	0.038	0.075	0.075
	(0.045)	(0.035)	(0.043)	(0.033)	(0.028)	(0.027)	(0.147)	(0.144)
Enroll*For-profit	0.064**	0.082***	0.060**	0.075***	0.163*	0.163*	0.276	0.278
	(0.032)	(0.027)	(0.028)	(0.025)	(0.088)	(0.090)	(0.458)	(0.462)
Time Fixed Effect 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	23210	23210	23210	23210	21750	21750	21750	21750

Authors estimates of equation (5) in the text using for-profit, public, and not-for-profit institutions. The enrollment measure is 12-month total enrollment. Enrollment and outcomes are aggregated by CBSA, institution-type (public or for-profit). 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-year level variables as controls: base-year $(t-3)$ 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 fixed effects. Even columns include state-for profit and year-for profit fixed effects, while columns (3)-(4) and (7)-(8) include state-by-year fixed effects.

Table A-23: Effects of Labor Demand Changes and For-Profit Supply on Entry/Exit of Postsecondary Institutions, by Sector and Period

Panel A: 2000-2006	4 year schools				2 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	-0.006	0.001	0.003	0.001	-0.011	0.016	-0.003	-0.011*
	(0.005)	(0.007)	(0.002)	(0.003)	(0.009)	(0.016)	(0.005)	(0.006)
(For-profit Supply)*($\hat{\eta}$)	0.003	0.002	-0.000	-0.000	0.000	0.000	-0.000	-0.001*
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5522	5522	5522	5522	5086	5086	5086	5086
R-squared	0.215	0.271	0.025	0.101	0.066	0.143	0.041	0.259
Mean of Dep. Var.	0.438	0.438	0.746	0.746	2.899	2.899	1.751	1.751
Panel B: 2008-2014	4 year schools				2 year schools			
	For-Profit		Public		For-Profit		Public	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted Labor Demand Change ($\hat{\eta}$)	0.003	0.002	-0.000	0.003	0.022***	0.014	0.005	0.007*
	(0.004)	(0.005)	(0.002)	(0.002)	(0.007)	(0.010)	(0.003)	(0.004)
(For-profit Supply)*($\hat{\eta}$)	-0.002	-0.002	-0.000	0.000	-0.001***	-0.001***	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Fixed Effects	Year	State*Year	Year	State*Year	Year	State*Year	Year	State*Year
Observations	5477	5477	5477	5477	5065	5065	5065	5065
R-squared	0.502	0.537	0.048	0.197	0.344	0.390	0.050	0.203
Mean of Dep. Var.	0.882	0.882	0.807	0.807	3.456	3.456	1.609	1.609

Authors estimates as described in the text using 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-year controls as described in the text as well as CBSA and year fixed effects. State-year fixed effects are included in even columns. 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.