Can Public Colleges Cater to For-Profit Students?

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Abstract

For-profit colleges yield lower returns than public alternatives but may attract students who would otherwise forgo higher education. I estimate for-profit students' second choices from their revealed preferences following abrupt for-profit chain closures. Between 30-50% of for-profit students substitute to public colleges, yet those who do downshift to part-time enrollment, slowing their degree progression. Public diversion is significantly larger where for-profit and public colleges specialize in similar programs. New public programs introduced under a large community-college grant program sap for-profit enrollment only when their fields of study overlap, reinforcing the evidence that limited substitution stems from for-profits' distinctive vocational programs.

JEL Codes: I23, I24, I28, J24.

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1 Introduction

For-profit colleges offer lower average returns and charge significantly higher prices than open-access public colleges (Lovenheim and Smith 2023). In an environment with extensive for-profit advertising and limited information about college quality, curtailing public support for for-profit colleges is a commonly proposed policy lever for encouraging better college choices (Cellini and Koedel 2017). Whether such policies will steer students toward higher-return public colleges or lead them to forgo higher education is an empirical question about the sources of for-profit college demand: where would for-profit students enroll, if anywhere, were their preferred for-profit unavailable?

This question is also central to understanding how the for-profit sector shapes inequality in higher education. Even compared to their peers at community colleges (CCs), for-profit students come from relatively low-income families and are more likely to enter college at an older age, without a high-school diploma, or as a single parent. Whether the for-profit sector has expanded college mobility – opening up new pathways for groups historically excluded from higher education – hinges on whether these students would have enrolled at all, but for the expansion of the for-profit sector. Answering this question can provide a new angle with which to view recent work on the college mobility pipeline, as in Bleemer and Quincy (2025).[1]

Especially among two-year colleges with low graduation rates, initial enrollment choices only capture part of the picture – subsequent persistence and degree attainment shapes the total return to college. While for-profit students mostly enroll full-time in accelerated degree programs, community colleges feature more complex degree pathways and lower graduation rates. And in light of their socioeconomic disadvantages, those for-profit students who do substitute to public colleges may face unique challenges which prevent them from reaping the full benefits of a public education. Since enrollment intensity is a strong predictor of subsequent attainment, whether for-profit students can maintain their full-time pace at CCs speaks to the salience of barriers to public degree progression.

These facts motivate an overarching question: Can public colleges effectively cater to for-profit students? In particular, do for-profit students find public colleges to be viable substitutes, as evidenced by their revealed preferences following for-profit closures? When they do substitute, are they able to maintain their full-time pace to their desired degrees? What

[1] Even if for-profit colleges expand college access, they may not improve economic outcomes, and evidence on the net returns to for-profit enrollment relative to no college is conflicted. Using nationwide data, Cellini and Turner (2019) find that for-profit certificate programs may not pay off on average. By contrast, recent evidence from Jepsen et al. (2025) shows relatively large net returns based on the near-universe of for-profit degree programs in Missouri. And college-level estimates from Armona and Cao (2024) show significant heterogeneity, with negative returns concentrated among cosmetology programs.

institutional characteristics enable community colleges to accommodate for-profit student demand? And can community colleges make investments to better cater to the interests of for-profit students?

To evaluate whether for-profit students substitute to public colleges – and, if so, whether public schools slow their degree progression –, I study a large shock to for-profit college supply: the nationwide closures of 10 for-profit chains spanning the U.S. These chains collectively operated more than 400 campuses, which shuttered abruptly between 2014-2018 due primarily to heightened regulatory pressure. I leverage the across-cohort choice-set variation induced by these closures to infer the extent of substitution from closing schools to other sectors. I propose a shift-share estimator, where abrupt college exit shifts residual demand for other colleges, but to different degrees in different local markets based on campuses' pre-exit market shares.

To guide interpretation, I derive the shift-share estimating equations as the reduced form of a generic discrete-choice college demand model. Building on work by Conlon and Mortimer (2021), I show that the average rates of substitution from for-profit colleges to other sectors are identified from market share data before and after a set of college closures, provided they satisfy a suitable exogeneity assumption. Appealing to institutional evidence and a variety of empirical tests, I argue that the chain closures I identify satisfy this assumption. I also show this same college-closure variation identifies school effects on enrollment intensity, given data on market shares split by full-time/part-time status.

I find evidence of modest substitution from for-profit to public colleges: 30-50% of students who would have attended closing for-profit colleges instead enroll in public colleges. Yet nearly all of those who do downshift from full- to part-time enrollment, implying substantial school effects on enrollment intensity. Diversion to other local colleges is minimal – as a result, total local enrollment declines by about 40-60% of the size of the for-profit closure. These core findings are robust to a variety of alternative specifications and market definitions.

I next explore whether some community colleges are better able to accommodate for-profit students than others. Whereas public colleges offer a range of academic and vocational majors, for-profit colleges specialize in a small set of vocational fields of study which are often unavailable at nearby community colleges. Using three empirical approaches, I show that these differences in program offerings strongly influence enrollment choices and constitute an important reason for incomplete substitution. First, I show that program offerings matter to students according to their stated preferences: for-profit students rank programs of study as the most influential factor in their college decisions, even above location and affordability. Consistent with this survey evidence, I find that estimated substitution rates to the public

sector are significantly higher in the minority of counties where public colleges specialize in programs similar to those offered by the closing chains.

To further validate this mechanism, I next show that new public-college programs significantly reduce for-profit demand, but only when neighboring for-profits offer programs in closely related fields. To do so, I use variation in public program offerings induced by the Trade Adjustment Assistance Community College and Career Training (TAACCCT) grant program. This $2 billion effort expanded community college programs of study in many fields offered by neighboring for-profit colleges. I show that for-profits offering the same field as that of a newly introduced CC program suffer persistent enrollment losses of more than 30%, while those offering programs in different fields face negligible enrollment effects. Altogether, these results provide strong evidence that program offerings contribute to for-profit college differentiation and are a key source of the incomplete substitution between for-profit and public colleges.

Related Literature My results contribute to three main strands of the literature. First, I add to our understanding of how the for-profit sector shapes college access. Most studies conclude that public colleges are close substitutes for for-profits, such that for-profits mainly divert students from higher-return options (Cellini 2021). Early studies used tuition and spending variation to document substitution patterns on the margin (Cellini 2009; Chung 2012; Goodman and Volz 2020). While these LATEs help characterize the marginal effects of policies like reducing CC tuition, they do not identify the share of for-profit students diverted from other colleges. Cellini, Darolia and Turner (2020) overcome this limitation, showing that Department of Education (DoE) sanctions in the 1990s against for-profits induced 60% diversion to public colleges. On the other extreme, Soliz (2018) shows that CC enrollment did not dip following the entry of many for-profit colleges in the 2000s. And Armona and Cao (2024) estimate a national college demand model, finding similarly low diversion ratios.

My average diversion estimates fall between these findings. While up to half of for-profit students substitute to public colleges, about half would not have enrolled anywhere but for their chosen for-profit. In addition, I highlight that diversion ratios depend significantly on market features, particularly the types of degree programs offered across sectors. The inconsistency of prior estimates may therefore reflect heterogeneity across widely differing contexts, as the for-profit sector has evolved rapidly and experienced significant churn over the last thirty years. My results characterize the substitution patterns of more recent cohorts (from 2014-2018) attending modern for-profit chains. I show that these for-profits are broadly similar to the rest of the two-year for-profit sector in Appendix E, concluding that diversion ratios among other for-profits may, if anything, lie below those I estimate for the for-profit chains.

Secondly, I provide the first evidence of a for-profit school effect on degree progression. While for-profits are touted for their accelerated degree programs, measuring their program features has been challenging, as for-profit transcript data is not readily available. In addition, for-profits' high rates of full-time enrollment and on-time graduation could simply reflect student sorting on factors like motivation. In reality, I show that for-profits' relatively high rates of full-time enrollment reflect large school effects on enrollment intensity. Though I cannot directly estimate school effects on downstream outcomes like graduation, initial full-time enrollment is highly predictive of long-term graduation rates.[2] In addition, encouraging full-time enrollment is a centerpiece of the most successful efforts to raise CC graduation rates thus far, as in the Accelerated Study in Associate Programs (ASAP) interventions (Weiss et al. 2019).

My results therefore provide novel suggestive evidence for one potential driver of low community-college graduation rates: students who would otherwise enroll full-time downshift to part-time enrollment at CCs, thereby slowing their credit accumulation. While I cannot characterize the underlying reasons for the part-time shift, this finding is consistent with several ways in which CCs are thought to slow degree progression. These include implicit capacity constraints and limited scheduling flexibility, onerous course requirements, and more complex degree pathways combined with limited advising resources.[3] All of these hurdles might be especially consequential for the kinds of non-traditional students who typically enroll in for-profits.

Finally, my results improve our understanding of what drives for-profit demand and differentiates for-profits from public alternatives. Much existing work provides suggestive evidence for different channels by documenting differences in for-profit and public college characteristics. These include for-profits' unique program offerings, reduced course requirements, extensive advertising, and enhanced flexibility (e.g., continuous academic calendars; Deming, Goldin and Katz 2012, 2013; Cellini and Chaudhary 2020). However, causal evidence on the impact of these distinguishing features is scarce. Differentiation is a matter

[2]Controlling for academic and socioeconomic background characteristics, Attewell, Heil and Reisel (2012) show that CC students initially enrolling part-time are between 8-13.2 percentage points less likely to graduate within 9 years (representing between 26-43% of the mean).

[3]Prior work documents the extent of these and other constraints at community colleges, which have become more binding as public support for CCs has fallen (Bound, Lovenheim and Turner 2010). For instance, course shutouts are common at CCs and have been shown to disrupt students' progress (Robles, Gross and Fairlie 2021). CCs vary in their scheduling flexibility, which is associated with greater enrollments (Grosz, Kurlaender and Stevens 2022). CCs have also been criticized for their "cafeteria model," which leaves students with many options but limited information about viable degree pathways and insufficient guidance to navigate them (Bailey, Jaggars and Jenkins 2015). The significant benefits of supplemental college advising programs suggest that these information frictions are large (Bettinger and Baker 2014; Weiss et al. 2019).

of both product characteristics and student preferences, and measuring preferences can be challenging.

To this end, I offer some of the first causal evidence on the drivers of for-profit college demand. Not only do for-profits specialize in unique vocational programs, but I show that these differences have large effects on enrollment behavior. In a similar vein, Armona and Cao (2024) also offer causal evidence on this question, showing that for-profit enrollment is highly sensitive to advertising. Their estimated demand model supports the importance of for-profits' distinctive program offerings as well.

Along the way, I provide the first quasi-experimental evidence on the effects of the large national for-profit shutdowns which impacted hundreds of thousands of students across the U.S. over the last decade. My work complements two recent studies describing how these closures impacted displaced students (Burns et al. 2022, 2023). Finally, I build on recent work in empirical IO showing how the diversion ratios of structural demand models can be recovered from different kinds of experimental interventions (Conlon and Mortimer 2021).

The remainder of the paper proceeds as follows. I begin by reviewing the data and institutional setting in Section 2, including the policy environment governing for-profit colleges, the pattern of for-profit closures, and the characteristics that distinguish for-profit and public colleges. Section 3 then lays out a general college-choice model, derives reduced-form estimating equations, and describes how they are identified and estimated from for-profit chain closures. Section 4 presents estimated diversion ratios and tests of part-time downshifting, followed by evidence in Section 5 on the importance of program-of-study differentiation as a mechanism for limited substitution. I then conclude with a discussion in Section 6.

2 Data and Institutional Setting

2.1 Data Sources

I combine institution-level data from two sources, both maintained by the National Center for Education Statistics (NCES). The first is a complete enrollment panel of Title-IV-eligible colleges spanning 2010 to 2019, taken from the DoE's Integrated Postsecondary Education Data System (IPEDS; National Center for Education Statistics 2005–2020). The second is a complete list of Title-IV-eligible college campus closures between 2010 and 2019, from the Postsecondary Education Participants System (PEPS; Office of Federal Student Aid 2010–2019).

I match campus closure data to the enrollment panel at the UNITID level, identifying more than 1,500 closures between 2010 and 2019. Although closures are reported at the

OPEID level, which can be more granular than UNITID identifiers, all of the chain campuses which I identify link each campus to a single UNITID identifier, so that I can observe campus-specific enrollment. To identify the campuses of closing chains, I use media reporting from various online news publications, combined with IPEDS data about college ownership and the listed institution names of different campuses, as well as the campus-level first-year enrollment series.

Beyond enrollment, I take several other key variables from IPEDS, including each campus' county, sector, and admissions rate (for defining the non-selective sample). For my analysis, I need annual data on *new* enrollment, as my research design compares college choices of successive cohorts of incoming college students facing different choice sets. For this purpose, I use counts of first-time, first-year, degree-/certificate-seeking undergraduates, split by full-/part-time status.[4]

For evaluating the for-profit enrollment effects of the TAACCCT grant program, I also use data on CC grantees' new programs of study from CareerOneStop, a website offering career resources sponsored by the Department of Labor (DoL). This data contains the fields of study in which CCs expanded their program offerings, using the Classification of Instructional Programs (CIP) taxonomy, along with other grant information (CareerOneStop 2017). I then relate this to IPEDS data on nearby for-profits' existing 2-digit CIP offerings.

Finally, I use nationally representative individual-level data from the 2016 National Postsecondary Student Aid Study (NPSAS) for statistics on distance between home and college, as well as other descriptive evidence on for-profit and community-college students (National Center for Education Statistics 2018). I also use the earlier 2004/2009 Beginning Postsecondary Students survey (BPS; National Center for Education Statistics 2011) for survey data on the factors which influenced students' college attendance choices.

2.2 Institutional Features

The private for-profit sector accounts for a small slice of U.S. undergraduate enrollment, enrolling just 5% of U.S. college students in 2021. This is down from 9% in 2010, partly due to the intensity of for-profit college closures over the course of the decade.[5] Despite their small share, for-profit colleges still serve nearly 800,000 students each year (National Center for Education Statistics 2017-2021).

Though small, the undergraduate for-profit sector comprises a diverse set of colleges which, broadly speaking, cluster into two models: large four-year colleges offering an array

[4]Full-time enrollment is defined as taking at least 12 credit hours per semester/quarter, or 24 or more contact hours per week.

[5]Appendix Figure A3 depicts these trends in college supply and enrollment.

of applied Bachelor's degree programs which are often conducted online, and smaller two-year colleges specializing in short, career-oriented technical training programs. Most of these two-year for-profits offer a small handful of degree programs – in some cases, just one –, and rarely provide fully online options. And while both have a professional focus, two-year for-profits prepare students for a distinctive set of occupations, primarily in blue-collar and services trades like automobile repair and cosmetology. Outcomes in these two corners of the for-profit market also differ markedly: four-year for-profit graduation rates are quite low, while two-year for-profits graduate students at significantly higher rates than other two-year colleges.

I focus on the two-year market. For one, this is where the majority of closures occurred, and the predominance of in-person programs also makes possible a spatial exposure-based research design. More substantively, it is also a part of the sector where seemingly close public alternatives abound: community colleges are affordable, open-admission, and usually locate relatively close to two-year for-profits. And while it has been home to some of the sector's worst offenders in terms of false advertising and fraud, the two-year for-profit sector also excels at providing concrete occupation-specific skills through short programs with high graduation rates and a clear connection to in-demand occupations. Indeed, Deming, Goldin and Katz (2013) conclude that "for-profits appear to be at their best with well-defined programs of short duration that prepare students for a specific occupation." In line with this view, Gilpin, Saunders and Stoddard (2015) show that two-year for-profits' entry and program-offering patterns are highly responsive to changes in local labor demand.

Two-year colleges in all sectors are almost entirely non-selective, with 96% admitting at least 80% of students (the admission-rate threshold I choose for defining non-selectivity).[6] Another important fact about the two-year college market is that search is highly localized, motivating my use of counties for geographic markets. Over half of two-year for-profit and public college students live within ten miles of their chosen school, and about 80% live within 25 miles, as seen in Figure 1.[7]

Though some of the best-known for-profits specialize in fully remote programs, these are concentrated in the four-year sector – in fact, only 4.8% of two-year for-profit students were enrolled fully online as of 2015. This figure rises to 12.1% among public two-year students.[8] Yet even those two-year students who enroll fully online remain overwhelmingly likely to choose a nearby college (Figure 1). For public students, this is related to in-district tuition

[6] Author's calculation based on 2014 IPEDS admissions data, for the universe of Title-IV-eligible, primarily sub-baccalaureate colleges. 89% have a fully open-admission policy.

[7] Author's calculation using 2016 National Postsecondary Student Aid Study (NPSAS) via PowerStats (https://nces.ed.gov/datalab/powerstats/default.aspx).

[8] See Table 311.15 of the NCES Digest (National Center for Education Statistics 2017-2021).

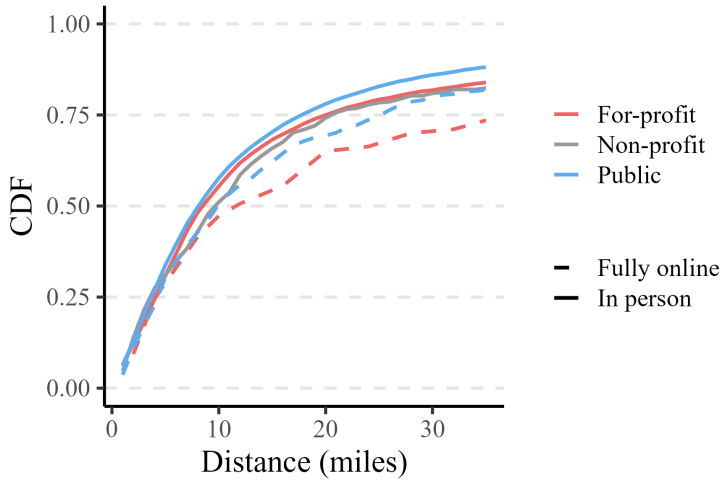


Figure 1: Distance to Chosen College

Notes: Non-selective two-year colleges only. Data from 2016 NPSAS (National Center for Education Statistics 2018).

discounts.

High rates of in-person study in this sector likely reflect the kind of hands-on, technical instruction required in many vocational programs. Among the for-profit chains I study, very few programs were even offered in a fully online capacity. To evaluate whether students could divert to fully online versions of these programs elsewhere, I check what share of all other colleges offering the same program allow a fully online option. The median chain for-profit program is only offered fully online by 3% of all the colleges offering that program. 90% are offered fully online less than 15% of the time.

Overall these facts suggest strong location preferences and/or limited consideration sets, likely due to the extra costs of attending school beyond a reasonable commuting distance from home. And even in the infrequent cases where students opt for remote schooling, they still tend to choose nearby schools. Therefore, students displaced from their first-choice college will be most likely to consider alternatives in its vicinity. This is important for my research design, as I rely on spatial variation in college closures to identify the attendance spillovers of for-profit exit. Later, I also show that allowing for substitution to four-year colleges and outside counties within larger Commuting Zones does not significantly alter my results.

2.2.1 Policy Environment and the History of For-Profit Chains

Most for-profit students pay their tuition using federally funded student loans under Title IV of the Higher Education Act of 1965. As these loans are offered at below-market rates

and default is relatively common among for-profit students, the federal government effectively subsidizes the for-profit sector. To remain eligible for these subsidies, for-profits must maintain accreditation and prepare students for "gainful employment in a recognized occupation." DoE can therefore moderate for-profits' access to federal revenue sources through its interpretation of the "gainful employment" (GE) standard.

The Obama administration promulgated rules to enforce this provision, which ultimately went into effect in 2014.[9] GE rules were part of a broader DoE effort under the Obama administration to scrutinize for-profit colleges' compliance with laws and DoE regulations. This heightened federal scrutiny ultimately led to the abrupt closure of many for-profit college campuses. When the DoE flagged a school for poor financial health or, as in the case of ITT Tech, its accreditor announced a financial audit, DoE would require the school to post a large surety and limit its access to federal aid disbursements. As for-profit colleges take up to 90% of their revenues from federal aid, losing access to this revenue stream can quickly lead to financial insolvency. Such financial pressures led ITT Tech to abruptly shutter all its 132 campuses in 2016 (Douglas-Gabriel 2016). In this and other cases, federal and state governments also sued and imposed fines for fraud and misleading job-placement claims. Corinthian Colleges was a leading example, closing all 28 campuses in 2015 after multiple lawsuits (Johnson 2015).

Apart from these biggest failed brands in the profit-sector, multiple other lesser-known chains of for-profit campuses have abruptly closed nationwide since 2013. Drawing on the college-closure database, I identify 10 primarily sub-baccalaureate for-profit chain closures satisfying several criteria: each chain (1) operated at least ten campuses across the U.S. prior to closing, (2) operated in more than one state, (3) closed all campuses in the same year and without advanced warning,[10] and (4) permanently stopped admitting new students.[11] In most cases, closures occurred as a direct result of financial troubles caused by threatened or actual suspension of federal financial aid eligibility. Appendix B includes further details on the timing and causes of each chain closure. These features of the chain closures will be important for the credibility of the identifying assumptions described in Section 3.1.

Finally, these chain for-profits offered almost entirely in-person instruction, and while some offered Bachelor's degrees, these made up a minority of awards at the vast majority of

[9]These rules would deny federal funding to for-profits whose graduates exceeded a maximum debt-to-earnings threshold. The provision was tied up in the courts and did not itself lead to any disqualifications (Weiss 2023), though schools have lost eligibility for other reasons.

[10]In some cases, the chain had a handful of campus closures several years before. In the case of Corinthian Colleges, the closures came in two large waves, with one in 2014 and another in 2015, and 56 other campuses were sold to Zenith Education Group. My results are not sensitive to the exclusion of this chain.

[11]In some cases, the chain simultaneously stopped all new enrollment without shutting down for existing students.

campuses. To ensure I capture the substitution patterns of students in two-year, in-person programs, I will restrict the analysis sample to those campuses which enrolled fewer than 10% of students fully online and awarded mostly certificates or Associate's degrees each year since 2010.

2.2.2 Geography of College Exit

The for-profit chain closures represent only a fraction of total for-profit exit over this period. From 2010 to 2019, more than 1,500 for-profit college campuses have permanently shut down, disrupting hundreds of thousands of active students and reducing the set of available colleges for prospective students. Some private non-profit colleges have also exited, though much less frequently than for-profit schools, which account for about 90% of closures. For-profit college exit is fairly geographically dispersed and occurred in approximately 40% of counties with any local college from 2010 to 2019, as described in Figure 2.

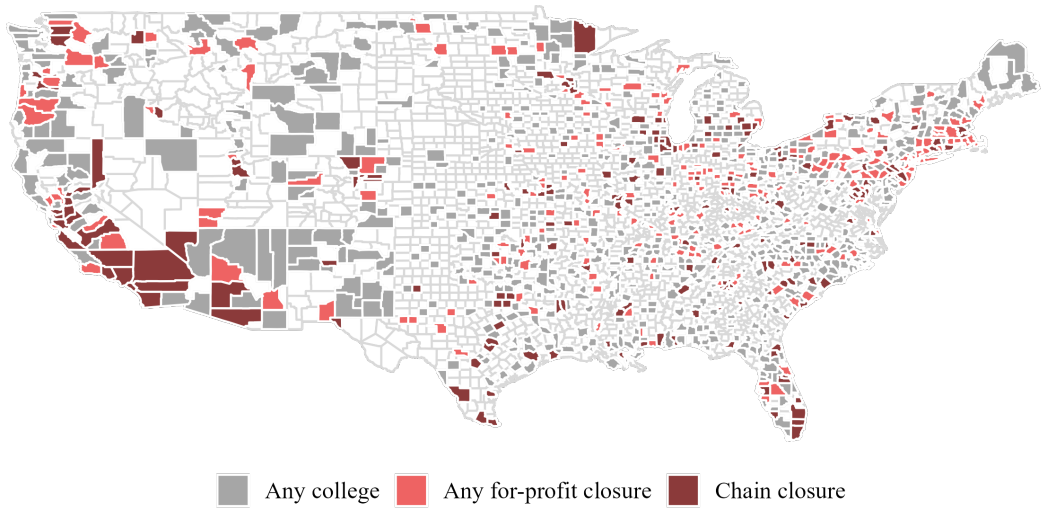


Figure 2: For-Profit Closures

Notes: Counties in red were the site of one of the 10 chain closures in my main sample. Counties in pink experienced some other two-year for-profit closure between 2010-2019, while counties in grey have a non-selective two-year college present but did not experience any for-profit closure over this period.

30% of these campus closures belonged to one of the 10 chains I identified. And as of 2010, these chains enrolled 42,600 *new* students, accounting for a whole 37% of first-time enrollment at closing colleges, as well as one-seventh of the entire sector's new enrollments in that year. Chain campus closures often overlap geographically: the 10 large chains collectively

shuttered 448 campuses, all of which operated within 198 counties. Still, each of these chains had operations across multiple states, and in some cases across coasts. There is substantial variation within chains in their local market penetration, described in Appendix Figure A1. This variation is critical for my research design, as I will compare local enrollment trends across counties with different pre-closure chain market shares, which serve as a measure of exposure.

Most of these local markets feature multiple college options: 86% of counties where for-profits have shut down also have a non-selective public college present through the whole period, and 94% feature a surviving for-profit. In my analysis, I will restrict attention to the 166 counties with available public options. Otherwise, estimated spillovers would be attenuated by markets where substitution is mechanically zero due to a lack of local alternatives. Non-profit colleges make up a very small portion of the two-year sector – only 24% of counties feature a non-profit college –, so estimated substitution to non-profits will reflect these mechanical zeroes.[12]

2.2.3 Potential Sources of Differentiation

Although I will not estimate a complete model of student preferences and beliefs, here I review evidence that for-profits look systematically different from public alternatives in key dimensions.[13] These differences provide suggestive evidence about sources of differentiation and help rationalize imperfect substitution across sectors.

For-profit colleges are substantially more expensive than non-selective publics, and available evidence shows that they yield lower earnings premia (Appendix Figure A4).[14] For-profit students may be relatively price-inelastic due to the availability of subsidized federal student loans (Armona and Cao 2024). In addition, students may lack information about prices or returns, resulting in undue optimism about their ability to repay large debt burdens. This is especially worrisome given the intensity of for-profit advertising – which has large enrollment effects – and repeated instances of misleading claims and fraud by for-profit recruiters

[12] Whether conditional or unconditional substitution rates are more relevant depends on the policy question of interest. Another motive for excluding counties without local alternatives is to make the assumption of localized substitution more plausible.

[13] I keep this review brief, as prior work already documents many of these distinctions. See, e.g., Deming, Goldin and Katz (2012), Deming, Goldin and Katz (2013), Cellini and Darolia (2016), Armona, Chakrabarti and Lovenheim (2022), and Armona and Cao (2024).

[14] Whether for-profits yield lower net returns is uncontested, though recent work shows that some for-profit programs yield larger gross returns than community colleges (Jepsen et al. 2025). However, existing estimates of college returns by sector only identify ATT effects for distinct populations and may not be informative about relative returns under essential heterogeneity. As discussed in Mountjoy and Hickman (2021), however, these ATT contrasts can still be informative about average relative returns under additional restrictions on essential heterogeneity like symmetric sorting gains.

(Cellini and Darolia 2016; Armona and Cao 2024).

What other features of for-profit colleges might support such high prices? Most for-profit colleges reside within a few miles of an open-access public college, so college availability does not readily explain for-profit enrollment.[15] For-profits do differ substantially from public colleges in their program offerings, specializing in a handful of pre-professional fields including healthcare, "personal services" like cosmetology and culinary arts, "handy trades" like construction and automobile repair, and information technology (Figure 3).

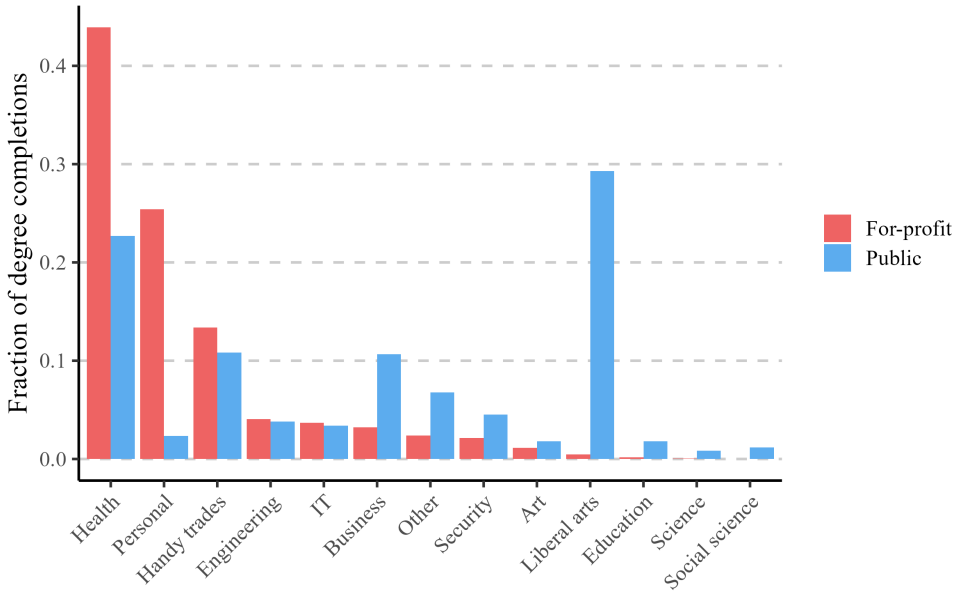


Figure 3: Programs of Study: For-Profit vs. Public

Notes: Degree completions by program taken from 2011-2015 IPEDS files. Restricting to non-selective, primarily sub-baccalaureate colleges which are not primarily online. Programs of study correspond to 2-digit CIP codes, sometimes grouped: "Handy trades" groups 46-49; "Engineering" groups 14-15; "Liberal arts" groups 23, 24, 30, 38, 54; and "Science" groups 26, 27, 40.

Though these aggregate distributions overlap somewhat, specific for-profit degree programs are often not available at any other local colleges. At the finer 6-digit CIP classification, only 39% of for-profit degree programs are even available at a public college in the county.[16] No single field drives this lack of overlap, though the rate of common offerings does vary somewhat across fields of study: public colleges are even less likely to offer the same cosmetology or security-services programs, and they are somewhat more likely to of-

[15] That said, community colleges may face implicit capacity constraints due to funding limitations and course enrollment caps, which may in practice limit public-college accessibility (Robles, Gross and Fairlie 2021).

[16] These estimates use 2013 program-offering data and restrict to counties where a two-year public college is present.

fer the same health, business, and legal-services programs (Appendix Table A3). At the college-level, 87% of for-profits offer at least one (locally) unique degree program. And half of for-profits have less than one third of their degree programs in common with another local college.

Despite their lackluster returns, two-year for-profit colleges graduate their students at much higher rates than community colleges – graduation rates are more than 20 percentage points higher overall.[17] To the extent that higher for-profit graduation rates reflect lower academic standards, this may be another important source of differentiation for students who doubt whether they could meet the graduation requirements of public schools. Others may simply want a more direct path to their degree, as for-profits typically don't require the remedial and core coursework needed at many CCs. Along with limited course requirements, for-profits are known for other forms of flexibility like continuous academic calendars and accelerated programs. Perhaps as evidence of accelerated program offerings, for-profit students are much more likely to enroll full-time. These and other differences are documented in Appendix Table A2.

Note that the available data on colleges' enrollment intensity, degree progression, and ultimately graduation rates may reflect a mix of student and college characteristics. For instance, a given student may choose to enroll full-time at any of their college options, and dispersion in graduation rates may simply reflect student sorting on initial preparedness or motivation. In that case, for-profits' higher graduation rates could not explain for-profit demand unless students wrongly believed their graduation outcomes would differ across schools. One contribution of this paper is to show that community colleges' slower rate of degree progression, and likely therefore also graduation, does reflect a substantial school effect. Differences in the ease of full-time enrollment and credit accumulation may therefore help to explain for-profit demand. This evidence also helps ground in data some of the qualitative evidence pointing to accelerated for-profit pathways, as in Bailey, Badway and Gumport (2001).

For-profit students also differ from their peers at non-selective publics. They come from much poorer families and take less traditional paths to college – they are more likely to have military experience, to delay post-secondary entry, to lack a high-school diploma, and to have caregiving responsibilities. Such students may have particularly strong preferences for for-profits' unique flexibility and degree paths, amenities without which they might not enroll in college. Appendix Table A4 elaborates on these differences.

[17] Though these statistics count transfer students as not graduating, accounting for higher rates of CC transfer only modestly shrinks this gap.

3 Empirical Model

Rather than specify and estimate a complete demand model, I now derive reduced-form estimating equations from a generic college-choice model. This exercise helps to clarify the appropriate functional form, to understand my estimator's complier group, to precisely characterize the counterfactual I identify, and to develop testable predictions from a plain logit model. None of the few modeling assumptions made here are necessary if we wish to take the reduced-form equations in Section 3.2 as given and interpret them as shift-share estimators justified under an "exogenous shares" assumption. An analogy can also be made to methods for estimating the mix of compliers coming from different alternatives in settings with multiple unordered treatments, as in Kline and Walters (2016).

Consider a standard random-utility model of discrete-choice demand, where each student $i \in I$ facing choice set $\mathcal{C}_i$ chooses college $j \in \mathcal{C}_i$ if $u_{ij} \geq u_{ik} \forall k \in \mathcal{C}_i$. Let d_{ij} be an indicator variable equal to one if i chooses j. College utilities are a function of student i's preferences (β_i, ϵ_{ij}) and the college's and student's characteristics, (X_j, Y_i): $u_{ij} = f(X_j, Y_i; \beta_i) + \epsilon_{ij}$, where $\epsilon_{ij} \overset{\text{iid}}{\sim} F$ is a continuously distributed taste shock. The model nests all commonly used discrete-choice demand models, from the simplest plain logit model (where $f(X_j, Y_i; \beta_i) = \delta_j$), to random-coefficients nested logit (RCNL) formulations. It also accommodates some forms of imperfect information via distortions of preference parameters.

Integrating out the $\boldsymbol{\epsilon}_i$ taste shocks yields individual choice probabilities

$$s_{ij}(\boldsymbol{X}) = \int \mathbb{1}\{u_{ij} \geq u_{ik} \forall k \in \mathcal{C}_i\} dF(\boldsymbol{\epsilon}_i)$$

so that $\sum_{j \in \mathcal{C}_i} s_{ij}(\boldsymbol{X}) = 1$. Aggregating individual choice probabilities yields market shares $s_j(\boldsymbol{X}) = \int s_{ij}(\boldsymbol{X}) dG(\beta_i, Y_i)$, where G is the joint distribution of student characteristics and preferences. Note that $\boldsymbol{X}$ includes all colleges' characteristics. For the remainder of the paper, I also subsume in $\boldsymbol{X}$ the distributions of student characteristics and preferences.

Given the demand system, individual diversion ratios are defined as

$$D_{jk,i}(\boldsymbol{X}) = \left| \frac{\partial s_{ik}(p_j, \boldsymbol{X})}{\partial p_j} \Big/ \frac{\partial s_{ij}(p_j, \boldsymbol{X})}{\partial p_j} \right| \in [0, 1]$$

where I split college j's price p_j out of $\boldsymbol{X}$.[18] The diversion ratio measures the extent to which colleges j and k are substitutable for student i. That is, when a marginal price increase at college j induces i to leave, $D_{jk,i}$ is the chance they divert to college k. Product-level diversion $D_{jk}(p_j, \boldsymbol{X}) \in [0, 1]$ is defined analogously and represents the fraction of students switching

[18] Note that individual i's diversion ratios do not depend on p_j since product j's characteristics are excluded from u_{ik}, $k \neq j$ (Conlon and Mortimer 2021).

to college k, out of those induced to leave j due to a small price change. Importantly, the diversion ratio represents the effect of an exogenous price change, holding the rest of the market characteristics $\boldsymbol{X}$ fixed.

For larger price changes, the total diversion ratio due to a price change $p_j \to p_j'$ is

$$D_{jk}(p_j', p_j, \boldsymbol{X}) = \left| \frac{s_k(p_j', \boldsymbol{X}) - s_k(p_j, \boldsymbol{X})}{s_j(p_j', \boldsymbol{X}) - s_j(p_j, \boldsymbol{X})} \right| \in [0, 1]$$

Letting $\bar{p}_j$ be the choke price for college j, i.e. the price such that $s_j(\bar{p}_j, \boldsymbol{X}) = 0$, we can represent a college closure as a large price increase, with the total diversion from product exit being

$$\bar{D}_{jk}(p_j, \boldsymbol{X}) = \left| \frac{s_k(\bar{p}_j, \boldsymbol{X}) - s_k(p_j, \boldsymbol{X})}{s_j(\bar{p}_j, \boldsymbol{X}) - s_j(p_j, \boldsymbol{X})} \right| = \frac{s_k(\bar{p}_j, \boldsymbol{X}) - s_k(p_j, \boldsymbol{X})}{s_j(p_j, \boldsymbol{X})}$$

These total diversion ratios are weighted averages of the incremental diversion ratios $D_{jk}(p_j', \boldsymbol{X})$ over $p_j' \in [p_j, \bar{p}_j]$. As the price increases beyond p_j, inframarginal buyers of j are also induced to leave. Conlon and Mortimer (2021) show that under weak regularity conditions this total diversion ratio can also be represented as a weighted average of individual diversion ratios:

$$\bar{D}_{jk}(p_j, \boldsymbol{X}) = \mathbb{E}[D_{jk,i}(\boldsymbol{X}) \mid d_{ij}(p_j, \boldsymbol{X}) = 1]$$

That is, the total diversion from a college closure is the average diversion ratio among those who would have enrolled in j under $(p_j, \boldsymbol{X})$ (the "compliers").

In this way, the $\bar{D}_{jk}$ provide second-choice data on the subsample of students enrolling in college j.[19] Raising p_j to the choke price is equivalent to an exogenous college closure; choices made under $(\bar{p}_j, \boldsymbol{X})$ reveal the second choices of students originally attending j, who must now reallocate to some alternative $k \in \mathcal{C}_i \setminus \{j\}$. This connection illuminates how college closures can identify the average diversion ratios of the choice model in reduced form.

Thus far I have suppressed market indices, but a reasonable choice of market boundaries will be important. This includes both the geographic- and product-market choices described in Section 2.2, as well as a choice of student population. I restrict attention to first-time college students in order to cleanly identify the choices of adjacent pre- and post-closure cohorts. Within these cohorts, IPEDS separately tracks full- and part-time enrollment. Although almost all for-profit students enroll full-time, a student's enrollment intensity may depend on their chosen college. For this reason, I will partition choice sets into sector-by-full-time/part-time combinations, allowing for substitution between full- and part-time status.

[19] See Appendix A for a proof.

This more granular representation of choice sets will also allow for tests of whether diversion occurs primarily within or across different levels of enrollment intensity – that is, whether or not college choice impacts students' attempted credits.

Though I infer substitution patterns without parameterizing and estimating the demand model above, it will be instructive to benchmark estimated diversion against the predictions of a simple demand model. In general, the diversion ratios depend on all features of the market – the characteristics of all products, as well as the distributions of student characteristics and preferences. Yet in the plain logit model, diversion ratios are simply proportional to market shares. In particular, when $u_{ij} = \delta_j + \epsilon_{ij}$ and $\epsilon_{ij} \overset{\text{iid}}{\sim} \text{T1EV}$,

$$\bar{D}_{jk}(p_j, \boldsymbol{X}) = \frac{s_k(p_j, \boldsymbol{X})}{1 - s_j(p_j, \boldsymbol{X})}$$

Diversion ratios in this case are identified directly from pre-closure market shares, and preference heterogeneity is trivial, only arising from iid taste shocks. As a result, I can test for the relevance of richer preference heterogeneity by comparing the plain logit predictions to the estimated diversion ratios.

3.1 Identification

Suppose we observe market shares over a distribution of markets m in which for-profit college $j(m)$ closes between $t = 0$ and $t = 1$. Let $(p_{j(m)}, \bar{p}_{j(m)})$ be the pre-closure and choke prices of college $j(m)$. In practice, I am interested in diversion across sectors, rather than between particular colleges. To that end, let $K(m)$ be the set of colleges in market m belonging to sector $K \in \{\text{public, non-profit, for-profit}\} \times \{\text{FT, PT}\}$, also split by full-time vs. part-time enrollment, and let $s_K(p_{j(m)}, \boldsymbol{X}_{mt}) = \sum_{k \in K(m)} s_k(p_{j(m)}, \boldsymbol{X}_{mt})$. Denote the total diversion from for-profit $j(m)$ to sector K under $(p_{j(m)}, \boldsymbol{X}_{m0})$ as

$$\bar{D}_m^K = \sum_{k \in K(m)} \bar{D}_{j(m),k}(p_{j(m)}, \boldsymbol{X}_{m0})$$

and the analogous individual diversion ratios as

$$D_i^K = \sum_{k \in K(m)} D_{j(m),k,i}(\boldsymbol{X}_{m0}).$$

3.1.1 Distribution of Diversion Ratios

Consider identification of the whole distribution of total diversion ratios from closing college $j(m)$ to another sector $K(m)$. In each market m, we observe market shares of local colleges

$J(m)$ before and after the closure, $\{s_{j0}, s_{j1}\}_{j \in J(m)}$. From these we can construct a simple Wald estimator for the total diversion from $j(m)$ to sector $K(m)$:

$$\begin{aligned}\hat{D}_m^K = \sum_{k \in K(m)} \frac{(s_{k1} - s_{k0}) - \gamma_K}{s_{j(m),0}} &= \sum_{k \in K(m)} \frac{[s_k(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_k(p_{j(m)}, \boldsymbol{X}_{m0})] - \gamma_K}{s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})} \\ &= \bar{D}_m^K + \frac{[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0})] - \gamma_K}{s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})} \\ &\equiv \bar{D}_m^K + u_m\end{aligned}$$

The γ_K represent an estimate of average counterfactual share growth in sector K. The identification challenge is that we do not observe $s_k(\bar{p}_{j(m)}, \boldsymbol{X}_{m0})$, the counterfactual school-k share had school $j(m)$ not been available one year earlier. Setting $\gamma_K = 0$, $\hat{D}_m^K$ identifies the total diversion ratio $\bar{D}_m^K$ provided that $\boldsymbol{X}_{m0} = \boldsymbol{X}_{m1}$, i.e. college characteristics and distributions of student preferences and characteristics remain fixed. This is the assumption maintained, e.g., in Raval, Rosenbaum and Wilson (2022), who estimate diversion ratios from disaster-induced hospital closures. Alternatively, we can directly assume that, but for the for-profit closure, sector-K market shares would have remained the same, or grown at a common rate γ_K: $s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) = \gamma_K$. This assumption implicitly restricts how $\boldsymbol{X}_{mt}$ changes following the closure. Note that to the extent other market characteristics change immediately after the closure, the Wald estimator's bias will be most severe in counties with the smallest closing schools. This will motivate restricting attention to markets with sufficiently large closures when estimating diversion in this way.

3.1.2 Average Diversion from Closing For-Profits

These assumptions are restrictive, as many other features of the choice set may change from year to year, as can the characteristics and preferences of adjacent cohorts of entering students. Given my primary interest in the overall rates of for-profit-to-public diversion, I now show that average total diversion rates are identified under weaker (and in my setting, more plausible) assumptions. A simple reduced-form estimating equation identifies average diversion from closing for-profits to other sectors, under a variant of the parallel-trends assumption.

By manipulating the definition of the total diversion ratios, we arrive at a linear estimating equation – the first-differences (FD) estimator:

$$s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(p_{j(m)}, \boldsymbol{X}_{m0}) = \alpha_K + \beta_K \times s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) + \epsilon_m$$

where

$$\alpha_K = \mathbb{E}[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0})]$$

$$\beta_K = \mathbb{E}[\bar{D}^K_m]$$

$$\epsilon_m = \overbrace{\big(s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0})\big) - \alpha_K}^{\text{excess trend due to } \boldsymbol{X}_{m0} \to \boldsymbol{X}_{m1}} + \underbrace{\big(\bar{D}^K_m - \mathbb{E}[\bar{D}^K_m]\big) s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})}_{\text{excess diversion}}$$

The error terms consists of (1) deviations of the target sector's enrollment trend from the overall trend, and (2) a factor due to heterogeneous market-specific diversion ratios.

Regression will identify $\beta_K = \mathbb{E}[\bar{D}^K_m]$ under two mean-independence conditions. First, the trend in sector K's enrollment needs to be unrelated to the size of the closing for-profit, but for the effect of the closure – a variant of the parallel-trends assumption:

$$\mathbb{E}\big[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0}) \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})\big] = \alpha_K$$

Notice that average diversion is identified only from intensive-margin variation across closures of different sizes, making comparisons to never-treated markets unnecessary. If diversion ratios differ across markets, the first-differences estimator also requires that the strength of diversion is unrelated to the size of the closing for-profit:

$$\mathbb{E}\big[\bar{D}^K_m \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})\big] = \beta_K$$

In other words, for least-squares regression to identify the unconditional average diversion ratio, heterogeneity in diversion ratios (effectively, random coefficients on the closing-school share) cannot be correlated with the independent variable.

A simple average of the Wald estimators, $W_K \equiv \frac{1}{|M|} \sum_{m \in M} \hat{D}^K_m$, identifies the average diversion $\mathbb{E}[\bar{D}^K_m]$ without this restriction on the heterogeneity in $\bar{D}^K_m$, provided that $\mathbb{E}[u_m] = 0$. Applying iterated expectations, this is satisfied when

$$\mathbb{E}\big[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0}) \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})\big] = \gamma_K$$

This condition is stronger than necessary, but it is identical to the first-differences assumption above, after replacing the regression intercept with γ_K. I identify γ_K using the average share

growth of treated markets with near-zero exposure:

$$\begin{aligned}\gamma_K(c) &\equiv \mathbb{E}[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(p_{j(m)}, \boldsymbol{X}_{m0}) \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) \leq c] \\ &= \mathbb{E}[s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m1}) - s_K(\bar{p}_{j(m)}, \boldsymbol{X}_{m0}) - \bar{D}_m^K s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) \leq c] \\ &= \gamma_K - \mathbb{E}[\bar{D}_m^K s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) \mid s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0}) \leq c]\end{aligned}$$

The second line is by the definition of $\bar{D}_m^K$, and the third comes from the above assumption. As the bandwidth shrinks to zero, we achieve $\lim_{c \to 0} \gamma_K(c) = \gamma_K$, as desired, since $\bar{D}_m^K \in [0, 1]$.

By comparison, the regression estimator may be more efficient, but it is also more sensitive to outlier closing-chain market shares and requires restricting the heterogeneity in $\bar{D}_m^K$. The Wald estimator also has the advantage that we can easily identify alternative weighted averages of market-level diversion estimates. In particular, I will weight by closing-campus enrollment in order to identify student-level average diversion, which may be more policy-relevant than market-level averages. The estimand in this case is $\mathbb{E}[\omega_m \bar{D}_m^K]$, where $\omega_m \equiv N_m / \mathbb{E}[N_m]$ are weights proportional to closing-chain enrollment N_m. I also consider enrollment-weighted regression estimators, yet these require stronger assumptions on the design variance in order to interpret them as student-level averages (Solon, Haider and Wooldridge 2015).

3.1.3 Across-Sector Diversion

When students choosing closing for-profits substitute to other for-profits, we observe their second-choice college, but not their second-choice sector. Second-choice sector data may be more pertinent for evaluating sector-level policy interventions, so I will also use for-profit closures to instrument for sector-wide for-profit enrollment. In particular, the change in closing-school share instruments for the change in the overall for-profit share, which then enters the second-stage model of public or non-profit enrollment share growth. The equation from Section 3.1.2 for diversion from *closing* for-profits to other sectors is then the reduced-form of this model. The complier group will still only consist of students at closing for-profit chains, but it will restrict attention to those induced to leave the sector – those for whom it's as if the entire for-profit sector became unavailable.

Beyond the exogeneity condition also assumed in identifying the reduced-form diversion ratios in Section 3.1.2, we now require three additional assumptions for identification of these LATEs. First, for-profit closures only impact other-sector enrollment by reducing for-profit sector enrollment (exclusion). This requirement is an implication of the college-choice model.[20] Instrument relevance requires that diversion from closing for-profits to other for-

[20] When the set of available colleges changes from J_0 to J_1 with $J_1 \subset J_0$, $j^* \equiv \arg\max_{j \in J_0} u_{ij} \in J_1$ implies

profits is less than complete. And for a LATE interpretation, we further require monotonicity: the probability of for-profit enrollment is non-increasing in the size of for-profit closures. This is once again implied by the choice model.[21]

3.1.4 Evaluating the Identifying Assumptions

Rather than use all for-profit closures over this period, I restrict attention to the nationwide closures of the 10 for-profit chains described in Section 2.2. These chain closures plausibly satisfy my identifying assumptions due to their abruptness, unexpectedness, their situation across dozens of geographic markets, and the fact that almost all chain campuses closed at once due to heightened regulatory scrutiny. Here I elaborate how these facts support the identifying assumptions, taking guidance from the underlying choice model.

First, these closures were generally induced by federal regulatory changes rather than changes in local demand conditions. In general, college exit decisions depend on profitability, which is a function of (unobserved) local college demand shocks – and these shocks independently influence enrollment in other sectors. For instance, persistent local labor demand shocks may drive overall college enrollment by shifting the outside-good utility, while also influencing college profitability and exit behavior. Chains operating across dozens of markets dispersed across different states are not nearly as exposed to local college demand fluctuations. Adding to this the fact that these chains closed in response to federal regulatory changes and/or an overall decline in the for-profit sector's reputation, we can reasonably conclude that these closures were not related to local demand conditions.

A second important feature of these closures is that the chains permanently shut down all their remaining campuses, rather than selectively closing the worst-performing locations. We might be concerned if chains selectively closed those campuses experiencing declining demand or tougher competition from local public colleges – say, due to public capacity investments or free-tuition programs. The fact that chains closed all their campuses allays these concerns.

Finally, these chains closed abruptly, in some cases even surprising the students in attendance at the time of the closure according to contemporaneous media reports. Therefore, these closures are unlikely to have been anticipated in advance by competitor colleges, leaving them unable to change their characteristics $\boldsymbol{X}_{-j(m)}$ in response, at least for the immediately following academic year. As a result, closures are unlikely to come with other changes in students' choice sets which could influence demand. Still, since competitor colleges may

$j^* = \arg\max_{j \in J_1} u_{ij}$. That is, for-profit closures do not affect other colleges' enrollment except through changes in for-profit enrollment (ruling out network effects in demand).

[21] Again, closures do not change choices for students originally choosing other colleges, and within-sector diversion is capped at one.

optimally adjust tuition, program offerings, and other characteristics in response to the closure, I restrict attention to the year immediately after closure and assume that any college responses occur with a lag.

As a robustness check, I will test whether observable college characteristics change right after the closure, and if so whether these changes are correlated with exposure. Specifically, I test whether changes in components of $\boldsymbol{X}_{mt}$ are correlated with the pre-exit chain share, $s_{j(m)}(p_{j(m)}, \boldsymbol{X}_{m0})$. These tests are informative about the parallel-trends assumption provided that the shares $s_K(\bar{p}_{j(m)}, \boldsymbol{X})$ are approximately linear on the support of the year-to-year variation in $\boldsymbol{X}$ (i.e., for small enough variation in $\boldsymbol{X}$).

Supposing the evidence is in favor of the parallel-trends assumption, the first-differences estimator still requires that heterogeneity in diversion ratios be unrelated to the closing chain's market share prior to closure. This condition would be violated, for instance, if larger chain campuses are large because of a weak public sector, which could induce low diversion (leading to downward bias). On the other hand, the kinds of students who choose a very small for-profit over a relatively strong public option (evidenced by market shares) may have lower public diversion rates (leading to upward bias). If diversion ratios are in fact correlated with closure sizes, this will generate a non-linearity in the relationship between sector-K share growth and closure size. To evaluate this concern, I will compare the linear estimator to a kernel smoother and plot the raw data. In any case, the Wald estimator is robust to violations of this condition.

The additional assumptions required for IV estimates of across-sector diversion are relatively innocuous. The closure instrument turns out to be quite strong, and the exclusion and monotonicity assumptions are implied by the simple mechanics of the discrete-choice model. However, one data issue calls into question the exclusion restriction. Apart from the for-profit colleges I observe in IPEDS, some small for-profit schools without access to Title-IV federal financial aid operate outside the IPEDS data universe (Cellini and Goldin 2014). Since this slice of for-profit enrollment is unobserved, exclusion may be violated if for-profit closures induce substitution to these non-Title-IV for-profits, and if students at these colleges would then substitute to public colleges under a for-profit ban. Though data limitations make it difficult to evaluate the extent of this issue, substitution to these small, unobserved for-profits may be limited by the fact that they specialize in non-degree programs and cannot offer federally subsidized student loans, as can the Title-IV-eligible closing for-profits. In addition, substitution to the Title-IV-eligible for-profits I do observe is near zero, suggesting that diversion to for-profits without the same loan-granting abilities may be similarly low. Moreover, this issue does not threaten the validity of estimated diversion from the closing for-profits to other sectors.

Finally, even if these closures satisfy the identifying assumptions, the diversion ratios I estimate represent diversion from the subsample of closing chain for-profits, which is not a random subsample of the for-profit sector. Although closing chains did not selectively close particular campuses, the chains which closed as a result of worsening regulatory conditions may have been particularly unprofitable and under-performing relative to the portion of the sector which remained open. Though this does not bias my ATT estimates, I take up the question of external validity by describing the observable differences between closing chains and other for-profits in Appendix E.

3.2 Estimation

In practice, the chain closures span four years between 2014 and 2018. To accommodate evolving target-sector enrollment trends, I fit calendar-year intercepts in the first-differences regressions. Where multiple chain campuses close in the same market and year, I sum their market shares in defining the closing-college market share. In cases where a county experiences closures in more than one year, I include all closure events and define the market index m based on all county/closure-year combinations.

Letting $t \in T = \{2014, 2015, 2016, 2018\}$ index the calendar year, the first-differences estimating equation is

$$\Delta s_{K(m),t} = \sum_{s \in T} \delta_s \mathbb{1}\{t = s\} + \beta_K \times s_{j(m),t-1} + \epsilon_{mt}$$

The outcomes $\Delta s_{K(m),t}$ include the change in public, non-profit, and other for-profit market shares, split by full- and part-time status. The sum of the β_K coefficients is the total rate of substitution from the closing for-profits to local colleges, equal to one under full substitution.

Since I do not observe the market size – that is, the number of "potential students" choosing not to attend college in each year – the public-college share is the fraction of total local college enrollment accounted for by local public colleges (i.e., it is the share of the "inside goods"). This means that the share's denominator is not exogenous as is often reasonable to assume when shares are defined as fractions of a market population determined by age and/or geography; instead, it depends on the utility of the inside goods relative to the outside good (no college), which is directly affected by for-profit closures. Therefore, I peg the share denominator at the pre-closure level of total county enrollment, so that changes in sector shares do not reflect changes in the inside-good share.[22] This does not change the

[22] A simple example makes this point clear. Suppose in a market of 100 potential students that 50 choose a public college, 10 chose the closing for-profit, and suppose the true diversion to the public sector is zero, with all market characteristics fixed but the closure. If we allow the denominator of the inside-good share to

interpretation of the coefficient as a diversion ratio, provided that changes in market size are uncorrelated with the closing for-profit share.

When estimating the average of Wald estimators, I trim closures of campuses which accounted for less than 2% of market share, since the Wald estimators for such small closures are particularly high-variance. In addition, I estimate the counterfactual share growth γ_K using closures with less than 1% market share (choosing $c = 1$ for $\hat{\gamma}_K(c)$). Letting $N_{\geq 2}$, $N_{\leq 1}$ be the number of market/years with exposure in that range, the Wald estimator is then

$$\hat{W}_K \equiv \frac{1}{N_{\geq 2}} \sum_{s_{j(m),t-1} \geq 2} \frac{\Delta s_{K(m),t} - \hat{\gamma}_K(1)}{s_{j(m),t-1}}$$

with $\hat{\gamma}_K(1) \equiv \frac{1}{N_{\leq 1}} \sum_{s_{j(m),t-1} \leq 1} \Delta s_{K(m),t}$. Finally, I present the main results weighted by closing-campus enrollment, with $(\tilde{\beta}_K, \tilde{W}_K)$ representing the enrollment-weighted estimators, and I include unweighted versions in Appendix Table A5.

4 Diversion Ratio Estimates

First, Figure 4 plots the data on changes in the public, non-profit, and for-profit market shares $\Delta s_{K(m),t}$, relative to the size of the closing chain $s_{j(m),t-1}$.[23] For public and non-profit diversion, the $y = x$ line represents complete substitution to the target sector, while the $y = 0$ line represents no substitution. The change in *total* for-profit share should track the $y = 0$ line if diversion to other for-profits is complete, and the $y = -x$ line if there is no within-sector substitution. The red fitted curve represents the predicted diversion under a plain logit model with no substitution to the outside good – under this simple model, substitution rates are proportional to market shares. The plain logit predictions further assume that full-time/part-time status is fixed; since 90% of for-profit students enroll full-time, the predicted diversion to full-time public enrollment is substantially higher than to part-time public enrollment. The blue fitted curve represents the diversion from the closures, estimated with a local quadratic regression of changes in sector-K shares on the closing-school share.

As for the first-differences (FD) estimator's assumption that diversion-ratio heterogeneity is unrelated to exposure, the fitted relationships do not deviate substantially from a linear approximation. Yet particularly for part-time public enrollment, a handful of markets with

update, the public-college share will increase by $50/50 - 50/60 \approx 16\%$, despite there being no substitution. Holding the denominator fixed gives the correct answer: $50/60 - 50/60 = 0\%$. Although in reality market size may change, we simply require that these changes not be systematically related to the closing-school share, as with other market characteristics.

[23] Changes in market shares are uniformly translated up or down to force a y-intercept of zero.

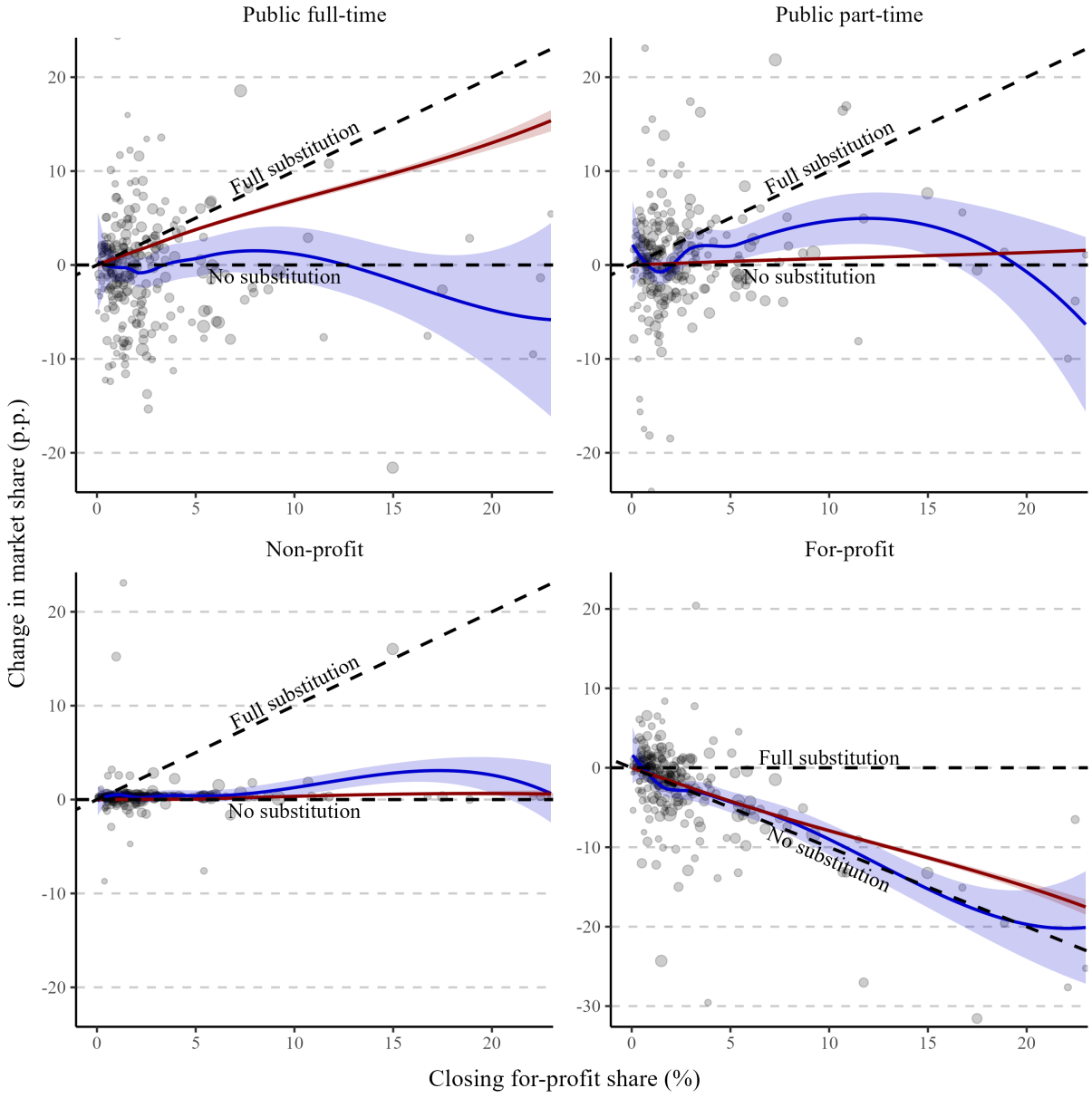


Figure 4: Diversion Estimates on Unadjusted Data

Notes: Red curves represent predicted diversion under the plain logit with no substitution to the outside good and fixed full-time/part-time status; blue curves represent estimated diversion. Shaded regions are 95% pointwise confidence bands for the predicted mean of a loess regression. Changes in market shares are uniformly translated up or down to have y-intercept equal to zero. Dot size proportional to closing-campus enrollment.

closing-chain market shares exceeding 20% exhibit lower diversion than the rest of markets. While these markets may be particularly informative about substitution patterns, they also exert a disproportionate influence on estimated diversion. As a robustness check, I will consider FD estimators which omit markets where exposure exceeds 20%. In addition, the Wald estimator relaxes the linearity assumption and is not as sensitive to these high-exposure markets.

The raw data show that diversion to full-time public enrollment is near zero, falling well below the predictions of the plain logit model. On the other hand, part-time public diversion exceeds predicted diversion based on for-profit students' full-time rates – indicating a switch from full- to part-time status among diverted students. Diversion to the non-profit sector appears near zero; this is consistent with the fact that non-profit college shares are small, and in some markets diversion is mechanically zero due to the absence of a local non-profit option. Diversion to the for-profit sector appears near zero as well, with roughly one-for-one declines in for-profit enrollment with the size of the chain for-profit closure. Altogether, enrollment losses at closing for-profit chains do not appear to be offset by gains at other local colleges.

Next, Table 1 shows estimated diversion from closing for-profits to other sectors, using both the first-differences regressions and the average Wald estimators weighted by closing-campus enrollment. Estimated diversion is split by initial enrollment intensity, with Panel A (B) showing full-time (part-time) diversion. The first column provides the average predicted diversion from the plain logit demand model with no substitution to the outside good (and fixed full-time status). Again, low predicted diversion to part-time enrollment reflects for-profits' high full-time rates. The high predicted public-sector diversion, totaling 82%, reflects the fact that community colleges have dominant market share in most markets. Predicted diversion to the non-profit sector is 2%, and about 16% to the rest of the for-profit sector.

Columns (2)-(4) show estimated diversion ratios. Public-sector diversion is estimated to be modest across specifications and concentrated in part-time enrollment. Both regression and Wald estimates show zero full-time public diversion, though standard errors indicate some imprecision. Part-time public diversion is 30% in the main FD specification, rising to 44% when removing outlier chain shares, and reaching 47% under the Wald estimator. For the FD estimator excluding outliers, 95% confidence intervals for public full- and part-time diversion are $[0, 0.57]$ and $[0.11, 0.77]$, respectively.[24] Notice that these confidence intervals exclude the predictions of the plain logit model with fixed part-time status, suggesting that for-profit students downshift to part-time enrollment at public colleges. I test this hypothesis formally in Section 4.1. Diversion to non-profits is estimated to be between 3-21% depending

[24]Diversion is bounded from below by zero.

Table 1: Diversion Ratios from Closing Chains to Other Sectors

	Logit	First Differences		Wald
	(1)	(2)	(3)	(4)
Panel A: Full-time				
Public	0.75	−0.03	−0.005	0.06
	(0.01)	(0.28)	(0.35)	(0.26)
Non-profit	0.02	0.17	0.21	0.03
	(0.002)	(0.15)	(0.18)	(0.07)
Other for-profit	0.15	−0.04	−0.04	−0.09
	(0.001)	(0.06)	(0.04)	(0.14)
Total	0.00	−0.85	−0.78	−0.76
	(0.00)	(0.22)	(0.27)	(0.32)
Panel B: Part-time				
Public	0.08	0.30	0.44	0.47
	(0.001)	(0.15)	(0.17)	(0.25)
Non-profit	0.00	−0.00	−0.00	0.00
	(0.00)	(0.004)	(0.004)	(0.01)
Other for-profit	0.01	0.04	−0.03	−0.01
	(0.001)	(0.06)	(0.04)	(0.04)
Total	0.00	0.28	0.35	0.39
	(0.00)	(0.15)	(0.18)	(0.26)
$\geq 2\%$ closure share				✓
$\leq 20\%$ closure share			✓	
Calendar-year FE		✓	✓	
Observations	251	251	248	181

Notes: Column (1) presents the mean diversion ratios predicted by a plain logit demand model with no substitution to the outside good, and with fixed full-time/part-time status. Columns (2)-(3) present the first-differences regressions with year FE. Column (4) presents the mean of closure-specific Wald estimators $\tilde{W}_K$. All estimates weighted by closing-campus enrollment. Heteroskedasticity-robust standard errors in parentheses.

on specification. Estimates of diversion to other for-profits are similarly small, with point estimates falling slightly below zero in most cases. As a result, IV estimates of across-sector diversion are nearly identical to the reduced-form estimates in Table 1 and are presented in Appendix Table A7.

Altogether, total local full-time enrollment declines by about 80% of the closure size, while part-time enrollment increases by about 35%. For the FD estimator excluding outliers, two-sided tests reject both that there are no full-time enrollment losses and that there are no part-time enrollment gains ($p = 0.004$ and $p = 0.050$, respectively).[25] Precision worsens somewhat when pooling full- and part-time enrollment, and whether we can reject full substitution to local colleges at conventional significance levels depends on the specification. The total enrollment losses are 56%, 42%, and 37%, across the three specifications in Columns (2)-(4), and a two-sided test can only reject full-substitution at a 10% significance level for the FD estimator in Column (2), where $p = 0.081$.

In addition, we cannot rule out diversion to colleges outside the observed choice set – these include fully online colleges and non-Title-IV-eligible schools (almost all of which are for-profit), as well as four-year and selective colleges and colleges located outside the county. However, since the programs in which the chains specialized are generally not conducted online, if program preferences are indeed important elements of for-profit students' college demand – as suggested by evidence to come in Section 5 –, diversion to fully online colleges is unlikely. Regardless, if online diversion takes place within the two-year sector, it is likely to be observed locally (Figure 1). As a robustness check, in Section 4.2 I will expand the market definition to the Commuting Zone (CZ) level and allow for substitution to four-year and selective colleges as well, finding very similar rates of substitution.

It may seem odd that diversion of for-profit students to other nearby schools in the same sector is near zero. However, during this time the for-profit sector overall faced a number of scandals and federal investigations related to misleading students and misreporting their outcome statistics – several of which implicated the very chains I study. These scandals were accompanied by extensive negative media coverage of the sector as a whole. Against this backdrop of the for-profit sector's deteriorating reputation, low diversion rates are not so hard to rationalize; Cellini, Darolia and Turner (2020) similarly find low or even negative effects on other for-profit enrollment during a similarly fraught period for the for-profit sector.

On the whole, these results provide strong evidence that for-profit students substitute to part-time public enrollment, but there is no evidence for substitution on other margins,

[25] Note that since the change in the closing-college share is exactly equal to the (negative) pre-exit closing-school share (as its share drops to zero), the total college enrollment effect is the sum of diversion ratios to public, non-profit, and other for-profit colleges, minus one (the contribution of the closing school). That is, $\beta_{pub} + \beta_{np} + \beta_{fp} - 1 = \beta_{total}$.

whether to full-time public enrollment or to other local private colleges. There is strong support for the conclusion that for-profit closures induce full-time enrollment losses, and there is somewhat weaker evidence for total enrollment declines. Next, I turn to the question of whether for-profit students reduce their enrollment intensity when they do substitute to public colleges – as suggested by the gains in part-time public enrollment.

4.1 School Effects on Enrollment Intensity

Though 90% of for-profit students enroll full-time, diversion to public colleges is concentrated in part-time enrollment. These point estimates imply that most for-profit students who substitute to CCs downshift to part-time status and take fewer credits than they otherwise would have. I now formally test for school effects on enrollment intensity by comparing the rates of part-time enrollment among for-profit and diverted public students. First, I show that college-closure variation identifies the counterfactual part-time enrollment rates of for-profit students when they enroll in public colleges, which can then be contrasted with their observed part-time rate at their chosen for-profits. I next form a test of school effects on full-time status by comparing both the relative and absolute rates of part-time enrollment between for-profit and diverted public students.

Let $PT_i^j \in \{0,1\}$ denote potential outcomes indicating whether student i would enroll part-time in sector $j \in \{FP, CC\}$, with $FT_i^j = 1 - PT_i^j$. Define $G_{FP} = \{i \in I : d_{i,j(m)}(p_{j(m)}, \boldsymbol{X}) = 1\}$ and $G_{FP \to K} = \{i \in G_{FP} : d_{i,K}(\bar{p}_{j(m)}, \boldsymbol{X}) = 1\}$ as the sets of students at closing for-profits and the subset of those who divert to community colleges, whether part- or full-time ($K \in \{CC, CC/PT, CC/FT\}$). There are no school effects on enrollment intensity, on average among compliers, if $\mathbb{E}[PT_i^{FP} - PT_i^{CC} \mid G_{FP \to CC}] = 0$. We observe empirical analogues of these quantities through the quasi-experiment of for-profit college closures.

In particular, the pre-closure part-time share at closing for-profits gives an estimate of the potential probability of part-time enrollment at for-profits, among for-profit students, $\mathbb{E}[PT_i^{FP} \mid G_{FP}]$. And the potential part-time probability at community colleges, among

those for-profit students diverted there, is equal to

$$\begin{aligned}
\mathbb{E}[PT_i^{CC} \mid G_{FP\to CC}] &= \frac{\mathbb{E}[PT_i^{CC} d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}]}{\mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}]} \\
&= \frac{\mathbb{E}[d_{i,CC/PT}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}]}{\mathbb{E}[d_{i,CC/FT}(\bar{p}_{j(m)}, \boldsymbol{X}) + d_{i,CC/PT}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}]} \\
&= \frac{\mathbb{E}[D_i^{CC/PT} \mid G_{FP}]}{\mathbb{E}[D_i^{CC/FT} + D_i^{CC/PT} \mid G_{FP}]} \\
&= \frac{\mathbb{E}[\omega_m \bar{D}_m^{CC/PT}]}{\mathbb{E}[\omega_m (\bar{D}_m^{CC/FT} + \bar{D}_m^{CC/PT})]}.
\end{aligned}$$

The second line is by definition, while the third line results from the relationship between diversion ratios and second-choice market shares (Appendix A). The resulting parameter can be consistently estimated from the diversion estimators as $\frac{\tilde{W}_{CC/PT}}{\tilde{W}_{CC/FT}+\tilde{W}_{CC/PT}}$.

We cannot immediately contrast these potential outcomes since the part-time probabilities of for-profit students may differ from those of *diverted* students. It is challenging to separately estimate diversion ratios of part-time and full-time for-profit students due to low part-time for-profit shares. I therefore consider a range of bounds on the relative diversion of full- and part-time students. If full-time for-profit students substitute to public colleges at $\kappa \geq 0$ times the rate of part-time students, then the counterfactual part-time share of diverted for-profit students is

$$\mathbb{E}[PT_i^{FP} \mid G_{FP\to CC}] = \frac{\mathbb{E}[PT_i^{FP} \mid G_{FP}]}{\kappa \mathbb{E}[FT_i^{FP} \mid G_{FP}] + \mathbb{E}[PT_i^{FP} \mid G_{FP}]}.$$

This follows from Bayes' Rule.[26] For a choice of κ, then, differencing these mean potential outcomes identifies the desired school effect on enrollment intensity (among compliers), $\mathbb{E}[PT_i^{FP} - PT_i^{CC} \mid G_{FP\to CC}]$. We might expect that the 10% of for-profit students who only enroll part-time are even more likely to be on the margin of non-enrollment – in which case we may assume that their public substitution rates are no greater than those of full-time students, i.e., $\kappa \geq 1$. But given any lower bound $\kappa \geq \kappa_0$, the null hypothesis of no school

[26] Assuming that full-time for-profit students substitute at κ times the rate of part-time students, i.e. $\mathbb{E}[D_i^{CC} \mid G_{FP}, FT_i^{FP} = 1] = \kappa \mathbb{E}[D_i^{CC} \mid G_{FP}, PT_i^{FP} = 1]$, Bayes' Rule yields:

$$\begin{aligned}
\mathbb{E}[PT_i^{FP} \mid G_{FP\to CC}] &= \frac{\mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}, PT_i^{FP} = 1]\mathbb{E}[PT_i^{FP} \mid G_{FP}]}{\mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}]} \\
&= \frac{\mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}, PT_i^{FP} = 1]\mathbb{E}[PT_i^{FP} \mid G_{FP}]}{\kappa \mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}, PT_i^{FP} = 1]\mathbb{E}[FT_i^{FP} \mid G_{FP}] + \mathbb{E}[d_{i,CC}(\bar{p}_{j(m)}, \boldsymbol{X}) \mid G_{FP}, PT_i^{FP} = 1]\mathbb{E}[PT_i^{FP} \mid G_{FP}]} \\
&= \frac{\mathbb{E}[PT_i^{FP} \mid G_{FP}]}{\kappa \mathbb{E}[FT_i^{FP} \mid G_{FP}] + \mathbb{E}[PT_i^{FP} \mid G_{FP}]}
\end{aligned}$$

effect on full-time status implies

$$\frac{\mathbb{E}[PT_i^{FP} \mid G_{FP}]}{\kappa_0 \mathbb{E}[FT_i^{FP} \mid G_{FP}] + \mathbb{E}[PT_i^{FP} \mid G_{FP}]} \geq \frac{\mathbb{E}[D_i^{CC/PT} \mid G_{FP}]}{\mathbb{E}[D_i^{CC/FT} + D_i^{CC/PT} \mid G_{FP}]}.$$

In practice, I test the implied linear restriction,

$$g_1(\kappa_0) \equiv \kappa_0 \mathbb{E}[FT_i^{FP} \mid G_{FP}]\mathbb{E}[D_i^{CC/PT} \mid G_{FP}] - \mathbb{E}[PT_i^{FP} \mid G_{FP}]\mathbb{E}[D_i^{CC/FT} \mid G_{FP}] \leq 0.$$

Without any assumption on κ, the hypothesis of no school effects on full-time status also implies that the absolute substitution rate to part-time enrollment cannot exceed the part-time share among for-profit students, since $D_{jk,i} \leq 1$:

$$g_2 \equiv \mathbb{E}[D_i^{CC/PT} \mid G_{FP}] - \mathbb{E}[PT_i^{FP} \mid G_{FP}] \leq 0.$$

This gives an additional restriction on the estimated diversion ratios which I combine with the restriction on relative enrollment rates in a joint Wald test of $H_0 : \boldsymbol{g}(\kappa_0) \leq 0$ versus the complement $H_1 : \{g_1(\kappa_0) > 0\} \cup \{g_2 > 0\}$. Since the assumption of no school effects implies both inequalities in H_0, rejecting the intersection null implies that CCs cause for-profit students to downshift to part-time enrollment.[27]

In practice, I take $\kappa_0 = 1$ as a neutral baseline and consider robustness to $\kappa_0 \in [0, 1]$, with $\kappa_0 = 0$ the least favorable case for rejection. I implement a one-sided test of the least favorable point null $g_1(\kappa_0) = g_2 = 0$, where the null inequalities bind. When using the FD regression estimators, I apply a standard Wald test, estimating the full covariance matrix of the parameters using a system of seemingly unrelated regressions. When using the Wald estimators, I estimate the covariance matrix of the restrictions Σ_g by bootstrap sampling and apply a re-centered bootstrap hypothesis test.

Table 2 displays the estimated functions $\boldsymbol{g}$ along with the p-values for tests of no school effects on enrollment intensity, for different levels of κ_0. In the neutral baseline where full-time and part-time for-profit students substitute to public colleges at equal rates ($\kappa_0 = 1$), there is strong evidence of part-time downshifting under both estimators ($p < 0.001$). Even if part-time for-profit students are twice as likely to substitute to public colleges, we still reject in both cases ($p < 0.02$). And in the extreme case where all diverted for-profit students would have enrolled part-time at their first-choice for-profit ($\kappa_0 = 0$), the absolute level of public part-time diversion is large enough to support part-time downshifting at or near conventional

[27] Rejection could instead indicate a violation of the maintained hypothesis $\kappa \geq \kappa_0$, which is why I consider values up to the worst-case $\kappa_0 = 0$. Still, $\kappa < 1$ would be counterintuitive, given that, if anything, full-time students should be more committed to re-enrolling elsewhere.

significance levels ($p = 0.078$ and $p = 0.143$ for FD and Wald estimators, respectively).

Table 2: Tests of School Effects on Enrollment Intensity

	First Differences			Wald		
	$\kappa_0 = 1$	$\kappa_0 = 0.5$	$\kappa_0 = 0$	$\kappa_0 = 1$	$\kappa_0 = 0.5$	$\kappa_0 = 0$
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{g}_1$	0.403	0.201	0.0004	0.422	0.208	−0.006
	(0.142)	(0.071)	(0.032)	(0.221)	(0.108)	(0.024)
$\hat{g}_2$	0.351	0.351	0.351	0.379	0.379	0.379
	(0.164)	(0.164)	(0.164)	(0.251)	(0.251)	(0.251)
p-value	0.0002	0.011	0.078	0.0004	0.017	0.143
$\geq 2\%$ closure share				✓	✓	✓
$\leq 20\%$ closure share	✓	✓	✓			
Observations	248	248	248	181	181	181

Notes: Columns (1)-(3) present tests of school effects on full-time status, using the enrollment-weighted regression estimators, trimming to $\leq 20\%$ closure share. Columns (4)-(6) present the same tests, but using enrollment-weighted Wald estimators of diversion. Results are shown for different values of κ_0, the lower bound on the relative substitution rate of full-time for-profit students. The reported p-value is for the one-sided joint Wald test of $H_0 : g_1 \leq 0,\ g_2 \leq 0$. Also shown are the estimated values of $\boldsymbol{g}$, $\hat{g}_1$ and $\hat{g}_2$, which capture the excess relative and absolute part-time diversion, respectively. In parentheses are heteroskedasticity-robust standard errors for Columns (1)-(3) and bootstrap standard errors for Columns (4)-(6).

In summary, while almost all for-profit students enroll full-time, the estimated full-time share of diverted students is near zero, implying that CCs significantly reduce for-profit students' enrollment intensities. While I cannot directly estimate school effects on downstream outcomes, initial enrollment intensity mechanically affects potential credit accumulation and is among the strongest predictors of persistence and long-term degree attainment (Attewell, Heil and Reisel 2012). Still, selection-on-observables assumptions in this context are not particularly satisfying, as full-time students are likely to differ in motivation and other predictors of graduation which are hard to observe. As complementary suggestive evidence, one of the most emphasized features of the ASAP interventions is their full-time enrollment requirement (Weiss et al. 2019). Though these programs bundle full-time requirements with other treatments, their sizable effects on credit accumulation and graduation likely owe in part to heightened enrollment intensity.

In any case, the fact that for-profit students opt for full-time enrollment but reduce their course load at public colleges strongly suggests that they encounter some institutional barrier

to enrolling in all their desired courses. This could reflect constraints on course capacity or flexibility, a lack of relevant course/program offerings, pre-requisite remedial requirements, and/or greater academic difficulty leading students to throttle back their course-taking. My results do not identify which mechanisms are at play. While a lack of open or relevant courses could harm students, other plausible channels may benefit students: remedial courses, for instance, might prime them for later success, and more challenging courses might be the reason for the greater returns to public colleges.

That said, for-profit students primarily enroll in college to earn certificates and degrees, and CCs appear to make achieving this goal harder for them. Indeed, BPS survey evidence suggests that for-profit students disproportionately value accelerated degree programs with high graduation rates. I find that for-profit students are nearly twice as likely as CC students to consider graduation rates when deciding where to enroll (44.1% vs. 23.4%), a gap which holds constant when conditioning on a rich set of pre-college student characteristics.[28] This indicates that the ability to earn a credential matters particularly to for-profit students, who may face higher psychic costs of schooling or simply prefer short, streamlined programs. Part-time downshifting and generally low CC graduation rates therefore reflect one way in which CC degree pathways may be unsuited to for-profit students' goals.

4.2 Robustness Checks

4.2.1 Concurrent Changes in Other Market Characteristics

Many drivers of college enrollment are observable, which allows for several natural robustness checks. If chain-closure exposure is in fact unrelated to the counterfactual growth of other college sectors, we would expect it to be uncorrelated with changes in the college characteristics of other sectors relevant for demand. In addition, it should be uncorrelated with changes in variables like the unemployment rate, which may impact overall college enrollment.

Table 3 presents balance tests for both the regression and Wald estimators, where I replace the outcomes $\Delta s_{K(m),t}$ with changes in a variety of placebo outcomes. These include the changes in tuition, revenues, online options, program offerings, and net entry of the local colleges in each sector. These are all characteristics which should not be directly impacted by diverted student flows but which may impact college demand and should not be trending differentially across markets with greater exposure to chain closures.

For the FD estimator, just two of the twenty-four tested coefficients are statistically significantly different from zero at a 5% significance level. These are public colleges' non-

[28] Calculation performed using the 2004/2009 BPS via PowerStats (https://nces.ed.gov/datalab/powerstats/default.aspx). Control variables are those listed in the notes to Table 5. This result is robust to restricting to students enrolling in Associate's degree programs.

Table 3: Balance Tests

	First Differences			Wald		
	Public	Non-Profit	For-Profit	Public	Non-Profit	For-Profit
Log tuition	0.001	−0.00003	0.001	0.002	−0.0001	0.002
	(0.004)	(0.002)	(0.001)	(0.004)	(0.002)	(0.002)
Log non-tuition revenue	−0.007	−0.085	−0.029	0.012	−0.042	−0.055
	(0.002)	(0.106)	(0.031)	(0.007)	(0.086)	(0.029)
Net entry	0.0003	0.002	0.0003	−0.001	−0.018	−0.002
	(0.001)	(0.007)	(0.002)	(0.003)	(0.014)	(0.004)
Offers fully online option	−0.009	0.001	0.003	0.004	−0.002	−0.001
	(0.009)	(0.006)	(0.002)	(0.012)	(0.007)	(0.002)
Log(# programs + 1)	−0.005	−0.001	−0.001	0.0002	−0.004	−0.004
	(0.002)	(0.002)	(0.001)	(0.003)	(0.004)	(0.003)
Log(# online programs + 1)	−0.017	0.012	0.006	−0.039	−0.081	−0.003
	(0.046)	(0.028)	(0.008)	(0.129)	(0.100)	(0.009)
Log(# vocational programs + 1)	−0.005	−0.001	−0.001	0.00003	−0.001	−0.004
	(0.002)	(0.002)	(0.001)	(0.003)	(0.004)	(0.003)
Log(# certificate programs + 1)	−0.005	0.005	−0.001	−0.001	0.004	−0.006
	(0.002)	(0.006)	(0.002)	(0.003)	(0.006)	(0.004)
Joint Wald Test p-value	0.084	0.758	0.449	0.713	0.327	0.526
Observations	775	323	3,601	775	323	3,601

Notes: First-difference regressions and Wald estimators applied to eight placebo outcomes: the concurrent changes in characteristics of nearby colleges, split by sector. For each sector and estimator, I report the p-value of a joint Wald test that none of the eight outcomes are correlated with exposure to chain closures. All estimates weighted by closing-campus enrollment. Standard errors clustered by county reported in parentheses; those for the FD estimator are heteroskedasticity-robust asymptotic standard errors, while those for the Wald estimator are bootstrapped with 10,000 bootstrap samples.

tuition revenues and number of offered programs, which decline by about 1.38% and 1.00% on average across the interquartile range of exposure, respectively.[29] These are relatively small fluctuations, especially compared to typical variation in these characteristics: the median absolute year-over-year change in public non-tuition revenues is 6.2%, while that of public program counts is 3.4%. For most other characteristics, I estimate precise null relationships with exposure. To account for multiple hypothesis testing, I also present joint Wald tests that none of these outcomes are predicted by exposure. Only public-sector characteristics under the FD estimator are marginally significant ($p = 0.084$).

Balance tests for the Wald estimator indicate no statistically significant relationships with exposure at a 5% significance level, though standard errors are somewhat larger in most cases. And for the Wald estimator, joint Wald tests of any relationship with exposure yield p-values well above conventional significance levels. Overall, changes in demand-relevant market characteristics are relatively well-balanced.

In addition, I regress contemporaneous changes in the county unemployment rate on chain-closure exposure.[30] Unemployment rates increase by just 0.016 percentage points for every unit of closing-chain market share (s.e. 0.011). This figure is -0.079 under the Wald estimator (s.e. 0.035). Though the Wald estimate is statistically significant, these changes are again economically small, especially compared to the overall 0.57 percentage-point average decline in the unemployment rate over this period. This suggests that local labor market conditions are not evolving much differently across exposure levels. Altogether, these checks provide evidence that the size of chain closures may be as-good-as-random with respect to changes in some of the important market characteristics likely to influence enrollment behavior.

4.2.2 Alternative Market Definitions

In the main analysis, markets were defined as all the non-selective two-year colleges in a county, with inside-good market shares defined accordingly. The outside good therefore included not only the choice of no college, but also the choice of any selective or four-year college, or any college outside the county. Substitution on these margins is unlikely given the descriptive evidence about the strength of these students' location and program preferences and their below-average academic preparation, but here I relax these restrictions to be sure that the main estimates do not miss legitimate substitution toward such options.

Table 4 presents results from the first-differences diversion estimator under alternative market definitions, expanding either the choice set or the geographic market definition.

[29] The interquartile range of closing-chain market shares is 2.13 percentage points.

[30] Unemployment rate data is from Bureau of Labor Statistics (2005–2019).

Table 4: FD Diversion Estimates: Alternative Market Definitions

	Market definition:		
	Adding all local colleges	Adding out-of-county publics	Expanding to CZ markets
	(1)	(2)	(3)
Public			
Full-time	−0.35	−0.05	−0.65
	(0.42)	(0.50)	(0.45)
Part-time	0.18	0.46	0.36
	(0.12)	(0.20)	(0.18)
Four-year	0.02	0.11	–
	(0.15)	(0.07)	–
Non-profit	0.37	0.43	0.51
	(0.18)	(0.19)	(0.30)
Other for-profit	−0.21	−0.04	0.13
	(0.19)	(0.14)	(0.11)
Total enrollment			
Full-time	−0.97	−0.50	−1.00
	(0.26)	(0.36)	(0.27)
Part-time	−0.05	0.30	0.36
	(0.16)	(0.22)	(0.18)
Core geographic market	County	County	CZ
Including selective colleges	✓		
Including four-year colleges	✓	✓	
Including nearest public four-year		✓	
Observations	251	251	212

Notes: Mean diversion ratios from first-differences regressions with year FE. The first column adds to the original county-level choice sets both selective colleges and four-year colleges. The second column adds to the original choice sets only non-selective four-year colleges and, for counties without a non-selective public four-year, the nearest such out-of-county college. The third column expands the geographic market to encompass Commuting Zones, restricting to the original set of non-selective two-year colleges. All estimates weighted by closing-campus enrollment. Heteroskedasticity-robust standard errors in parentheses.

Column (1) admits all local colleges – adding in selective and four-year colleges –, while Column (3) expands the geographic market definition to the Commuting Zone to allow for substitution outside of counties within CZs. Column (2) blends these, including some out-of-county public four-years but otherwise restricting to in-county non-selective colleges. Results are quite similar to the main estimates overall. Across market definitions, we again find modest public substitution concentrated in part-time enrollment, though estimates of full-time public diversion are now quite imprecise. Estimates of non-profit diversion rise, though these results are driven by a handful of outlier high-exposure markets (see Appendix Figure A6), and diversion to other for-profits remains negligible. Total full-time enrollment declines remain substantial, though harder to distinguish from zero in Column (2), and we continue to find overall part-time enrollment increases when admitting out-of-county colleges.

Since the main analysis already restricted to counties where a local community college is present – and since community-college students almost always choose their nearest location even when enrolling online –, the primary suspect for omitted public substitution is non-selective four-years. Reassuringly, when adding in all local colleges (Column 1), estimated diversion to public four-years is just 2% (s.e. 15%), a relatively precise null. Non-selective public four-years are only present in 25% of counties, however, so Column (2) further expands the choice set to include the nearest such out-of-county college, in cases where a local option does not exist. There the estimated four-year public diversion remains low and insignificantly different from zero. And Appendix Figure A5 shows that four-year public diversion hugs the no-substitution benchmark across the distribution of exposure. Thus, we can be reasonably confident that the main specifications do not omit other significant channels of public substitution. It is harder to guard against missing substitution to other private colleges since some small for-profits do not report enrollment.

5 Mechanisms

Several commentaries on what generates for-profit demand have suggested that distinctive programs of study may be an important source of differentiation (Deming, Goldin and Katz 2012; Gurantz, Sakoda and Sarkar 2021), yet little direct evidence exists for this hypothesis. Although it is difficult to evaluate the reasons for limited substitution without estimating a college demand model, here I offer three empirical strategies for evaluating the role of differentiated field-of-study offerings in driving limited for-profit-to-public substitution.

5.1 Evidence from Stated Preferences

Before turning to revealed-preference evidence, I first consider survey data on students' stated preferences for college characteristics. The Beginning Postsecondary Students 2004/2009 survey elicited students' self-reported reasons for enrolling in their chosen college during their first year of enrollment, asking which of six factors influenced their attendance decision. These included affordability, location, reputation, programs of study, personal or family reasons, and other reasons.

Table 5 shows that for-profit students disproportionately care about programs of study, both in absolute terms and relative to other college characteristics. Among two-year for-profit students, programs of study are the single most commonly cited factor in college decision-making, an important influence for 76% of students. Programs of study are only the third most commonly cited factor among community-college students, considered important by some 43% of enrollees. For-profit students are 77% more likely than CC students to enroll due to programs of study. When controlling for a set of pre-college student characteristics including academic and socioeconomic background variables, this gap narrows only slightly to 67% (29 percentage points).

Table 5: Stated Reasons for Attending Chosen College

	Programs of study	Location	Reputation	Personal & family	Affordable	Other
Proportions						
For-profit	0.76	0.63	0.51	0.38	0.34	0.16
Public	0.43	0.84	0.35	0.37	0.67	0.11
Differences						
Unadjusted	0.32	−0.21	0.16	0.01	−0.33	0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
With controls	0.29	−0.21	0.17	0.00	−0.31	0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)
With controls, only shared programs	0.32	−0.11	0.21	0.03	−0.25	0.03
	(0.05)	(0.07)	(0.07)	(0.07)	(0.08)	(0.04)

Notes: Data from BPS 2004/2009. Outcomes are the proportion of students in each sector reporting that their college choice was influenced by each factor. Sample includes first-time college students at two-year colleges. Raw differences are shown alongside selection-corrected differences with controls for HS GPA, HS graduate status, highest level of HS math, years of HS math, years of HS science, mother's and father's highest education level, whether parents own investments in excess of $10k, income percentile rank, dependency status, binned income by dependency status, parents' marital status, age, race, gender, single parent status, family size, and an indicator for dropout risk. The final row of differences restricts to students initially enrolling in degree programs which commonly overlap between public and for-profit colleges, namely Associate's degree programs in vocation-related majors (which include IT, personal services, engineering and engineering technologies, legal professions, science technologies, security services, construction, mechanic/repair technologies, precision production, transportation and materials, visal arts, health professions, and business). Standard errors in parentheses.

For-profits do specialize in shorter vocational programs, and students enrolling in these degree programs may have stronger program preferences. Yet when restricting to those degree programs which commonly overlap between sectors – vocation-oriented majors in Associate's degree programs –, for-profit students remain substantially more likely to report enrolling due to the college's program offerings.

These results suggest that for-profit students have a strong sense of their desired programs of study and find for-profits well-suited to their educational interests. Still, the survey data do not distinguish which specific aspects of for-profit programs matter to students, which could include distinctive fields of study but also the accelerated and streamlined structure of for-profit programs. Next I turn to two revealed-preference analyses to isolate the role of for-profits' unique fields of study in driving program preferences and explaining limited substitution.

5.2 Heterogeneity by Program Similarity

To the extent that for-profit students care about for-profits' unique vocation-oriented fields of study, diversion ratios should be larger where public colleges specialize in similar fields. To explore this possibility, I now estimate heterogeneity in diversion ratios by the similarity of nearby public programs with those of closing chains.

To summarize the similarity of the closing for-profit campus' program offerings with those of other sectors, I take the vectors of historical degree completions by 2-digit CIP code for the closing chain campus, $\boldsymbol{C}_{j(m)}$, and for the local colleges in sector K, $\boldsymbol{C}_{K(m)}$. In each market, I then compute the cosine similarity between these degree-completion vectors as an index of program similarity (PS):

$$PS_m^K = \frac{\boldsymbol{C}_{K(m)} \cdot \boldsymbol{C}_{j(m)}}{\|\boldsymbol{C}_{K(m)}\| \|\boldsymbol{C}_{j(m)}\|} \in [0, 1]$$

As expected, closing chains are least similar to public colleges in terms of program offerings, with just one-quarter of local public colleges having more than 50% program similarity. The share of markets with non-profit similarity exceeding 50% is low too, at just 36%, and the share with other for-profit similarity exceeding 50% is nearly half. Figure 5 shows the distribution of closing chain program similarities across markets. These distributions are approximately bimodal, clustering above and below 50% similarity.

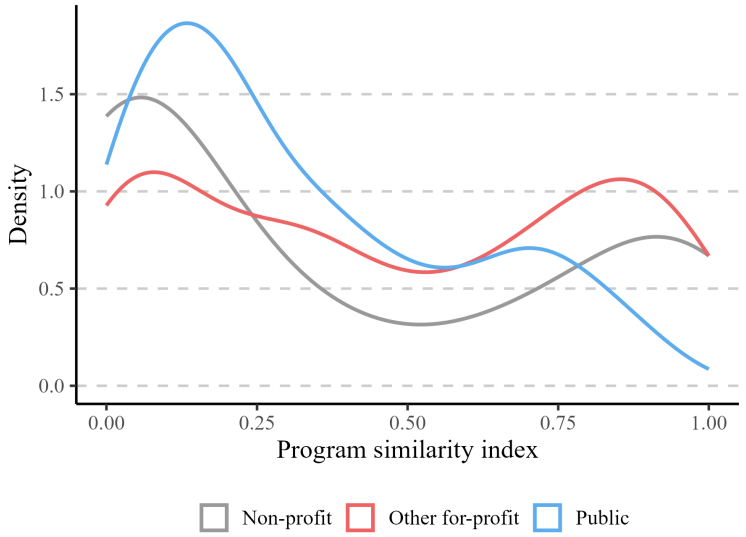


Figure 5: Program similarity distributions

Notes: Plotted are the distributions of PS_m^K, the program-similarity indices of closing chains in market m with local colleges in sector K.

For simplicity, I estimate diversion separately to high- and low-similarity public colleges, where $HighPS_m^{pub} = \mathbb{1}\{PS_m^{pub} > 0.5\}$. I interact $HighPS_m^{pub}$ with exposure in the FD regression and take conditional means of Wald estimators above and below $PS_m^{pub} = 0.5$. I present estimates of these conditional mean diversion ratios in Table 6, along with the estimated difference between high- and low-similarity diversion.

Estimated diversion to low-similarity colleges is near zero for both estimators and across enrollment intensities, though estimates are rather imprecise. Estimated diversion to public colleges is indeed higher when receiving-sector programs are highly similar to those of the closing chain, though the FD and Wald estimators of the difference are only marginally statistically significant ($p = 0.10$ and $p = 0.05$, respectively). Point estimates for diversion to high-similarity publics suggest nearly full substitution, though only the Wald estimate of 130% diversion can be distinguished from zero at conventional significance levels ($p = 0.028$).

5.3 Evidence from the TAACCCT Grant Program

Given the imprecision of these estimates, I now test for this same mechanism using an alternative source of variation which directly shifted community colleges' program offerings. This analysis helps to reinforce the diversion heterogeneity analysis, which could in principle be driven by other college characteristics correlated with program similarity, and it further shows whether public colleges can indeed become more attractive to for-profit students by expanding their vocational program offerings.

Table 6: Heterogeneity by Program Similarity

	First Differences			Wald		
	$PS_m^{pub} \leq 0.5$	$PS_m^{pub} > 0.5$	Difference	$PS_m^{pub} \leq 0.5$	$PS_m^{pub} > 0.5$	Difference
Total public	−0.29	0.73	1.02	0.02	1.30	1.28
	(0.31)	(0.54)	(0.62)	(0.48)	(0.59)	(0.66)
Full-time public	−0.45	0.45	0.90	−0.19	0.40	0.59
	(0.33)	(0.32)	(0.46)	(0.31)	(0.36)	(0.45)
Part-time public	0.15	0.28	0.12	0.21	0.90	0.68
	(0.17)	(0.26)	(0.31)	(0.28)	(0.30)	(0.30)
$\geq 2\%$ closure share				✓	✓	✓
Calendar-year FE	✓	✓	✓			
Observations		250			180	

Notes: Estimates of conditional mean diversion to public colleges using the FD and Wald estimators, splitting markets according to the similarity of closing-chain programs with those of local publics. Program similarity PS_m^{pub} is defined as the cosine similarity of the vectors of degree completions at closing chains and public colleges. All estimates weighted by closing-campus enrollment. Heteroskedasticity-robust standard errors in parentheses.

5.3.1 Institutional Background

The Trade Adjustment Assistance Community College and Career Training grant program was authorized in 2009 as part of the American Recovery and Reinvestment Act in the wake of the Great Recession. Across four waves from 2011-2014, the Department of Labor approved grants in excess of $1.9 billion designed to improve the quality of vocational education at community colleges across the country. Grantees used these funds to create new or redesigned degree programs targeted toward the needs of displaced workers and other adult learners seeking employment in growing local industries, primarily healthcare, manufacturing, and IT.

As part of their implementation of new and redesigned "career pathways," grantees were encouraged to partner with local employers, allow for online and technology-enabled learning, and develop structured program sequences awarding professional certifications and certificates at different stages. Other grant-funded activities could include recruitment of adult learners, academic support in the form of remediation services and career coaching, and work-based learning opportunities like sponsored internships. Nevertheless, the TAACCCT's primary aim and metric of success was capacity-building at community colleges through the development of career and technical education (CTE) programs, deemed necessary to address the country's workforce needs following the Great Recession and a period of stagnating CC funding (Durham et al. 2017).

Grantees were chosen through a competitive application process administered by the DoL.

Colleges offering degrees of two years or less were eligible to submit individual applications, or to apply as a consortium with other eligible colleges. Individual applicants typically received $2.5 million, while consortia could receive up to $25 million. In all, a total of 256 grants funded 729 individual colleges across all 50 U.S. states, including more than 60% of all community colleges in the U.S. All grantees submitted quarterly progress updates to the DoL, and dozens of commissioned reports have documented colleges' compliance with their initial program-building targets. As of 2018, 96% of all planned programs of study had been implemented, totalling some 2,691 new or redesigned programs. And to that point, TAACCCT programs had awarded some 350,000 credentials among a total of 500,000 enrolled students (Employment & Training Administration 2018). Soliz and Ecton (2023) also document the TAACCCT's effects on CC grant recipients by comparison to CCs which never receive grants, finding that the program boosted enrollment and substantially increased completions in targeted CTE fields.

5.3.2 For-Profit Spillovers: Asymmetric Effects by Program Similarity

Given that CTE programs constitute the bulk of for-profit offerings, this grant program also provides an excellent environment for testing whether differentiation on program offerings contributes to limited rates of substitution between for-profit and public colleges. To do so, I leverage differences in the similarity of new TAA-funded CC programs with those offered by neighboring for-profit colleges. Given that my diversion estimates are substantially higher when for-profit and public colleges specialize in similar programs of study, grants which expand CCs' CTE offerings may divert students from for-profit colleges, despite modest average substitution rates – and especially so for those grantees expanding offerings in fields of study closely related to those of neighboring for-profits.

Without grant-induced changes in CC program offerings, it would be challenging to identify whether for-profits divert students from CCs due to distinctive program offerings. For-profit entry and program-offering choices could be informative, but differences in local labor markets could confound this analysis – for instance, a region with a large hospital system may feature more health programs at both public and for-profit colleges, even if program offerings are in fact strategic substitutes. The rapid development of new CC programs in fields typically offered by for-profits allows an analysis of the demand effects of new programs, uncontaminated by for-profit supply responses and time-invariant location effects.

Of course, grants are not randomly assigned, and the CCs which win grants may be making other kinds of investments to attract students from the private sector. The evidence in Soliz and Ecton (2023) of limited effects on other college inputs assuages this concern to some degree. But we cannot rule out grantee selection for other reasons related to trends

in local college demand which could themselves impact for-profit enrollment. For testing the importance of program-of-study differentiation, however, variation in the composition of new TAA programs and in for-profits' initial program specialties allows a comparison *within* TAA-exposed schools, across for-profits facing new TAA programs in more or less similar fields. By comparing for-profits which were all exposed to nearby TAA programs, we limit concerns related to grantee selection.

Specifically, I split for-profits into three groups: those located in counties without any TAACCCT grantees (the never-treated), those located near TAACCCT grantees introducing programs in 2-digit CIP fields *not* offered by the for-profit, and those located near a TAACCCT grantee introducing a program in a field offered by the for-profit. I begin by estimating differences-in-differences regressions comparing changes in for-profit enrollment across these groups before and after new CC programs were introduced. Table 7 presents results for three different functional forms: I consider outcome equations in levels of enrollment, log enrollment, and an exponential-mean specification (Poisson regression).[31] I also allow trends to vary non-parametrically with for-profits' field offerings.[32] Across specifications, I find negligible enrollment effects on for-profits exposed only to a dissimilar program, while those exposed to similar programs suffer large enrollment losses.[33]

Even if TAA exposure is correlated with other factors driving for-profit enrollment trends, like additional public investments, directly comparing programs exposed to similar and dissimilar programs can still identify the importance of differentiation on program offerings. Columns (4)-(6) show that, relative to for-profits exposed only to TAA programs in different fields, for-profits facing new, similar programs see enrollment declines of about 30%.

I next evaluate treatment effect dynamics by estimating TWFE models comparing the evolution of log for-profit enrollment in each treatment group to that of the never-treated for-profits. I normalize the 2010 coefficients to zero, the year before the first TAACCCT wave. Figure 6 shows that as new TAACCCT programs were rolled out from 2011-2014, enrollment at for-profits located near CCs introducing similar programs steadily declined, while it remained relatively steady among those exposed to dissimilar programs. Finally, I refine the specification by aligning event timing and estimating effects by year relative to first

[31] Since enrollment is occasionally zero, I in fact use $log(max\{1, Enroll_{jt}\})$, which calibrates extensive-margin changes from 0 to 1 enrollee to have zero weight. The exponential-mean specification avoids this calibration (Chen and Roth 2024).

[32] Specifically, I fit FE for year × (top 2-digit CIP field) × (number of 2-digit CIP offerings). This precludes comparisons of for-profits with very different program offerings in terms of composition and scale.

[33] These results are robust to including non-parametric trends for different states and local industry employment composition, and when omitting the closing for-profit chains (Appendix Table A10). Though omitted for brevity, alternative specifications using degree completions by field of study as the outcome variable show substantially similar results.

Table 7: For-Profit Enrollment Effects of New TAA Programs: Diff-in-Diff Estimates

	Including never-treated schools			Excluding never-treated schools		
	Linear	Log-linear	Poisson	Linear	Log-linear	Poisson
	(1)	(2)	(3)	(4)	(5)	(6)
Dissimilar program	−2.4	−0.12	−0.06	–	–	–
	(2.4)	(0.04)	(0.09)	–	–	–
Similar program	−31.1	−0.40	−0.26	−26.9	−0.27	−0.19
	(5.5)	(0.05)	(0.11)	(5.7)	(0.06)	(0.11)
Colleges	3,226	3,226	3,226	2,263	2,263	2,263
College-years	25,808	25,808	25,808	18,104	18,104	18,104

Notes: The omitted group in Columns (1)-(3) is schools not exposed to any nearby TAA program; the omitted group in Columns (4)-(6) is schools exposed to a nearby TAA program in fields they don't offer. Pre-treatment years include 2007-2010, and post-treatment years include 2015-2018; intervening grant years 2011-2014 omitted. All specifications include non-parametric trends for combinations of the schools' top 2-digit field and the number of fields offered. Heteroskedasticity-robust standard errors clustered at the school-level in parentheses.

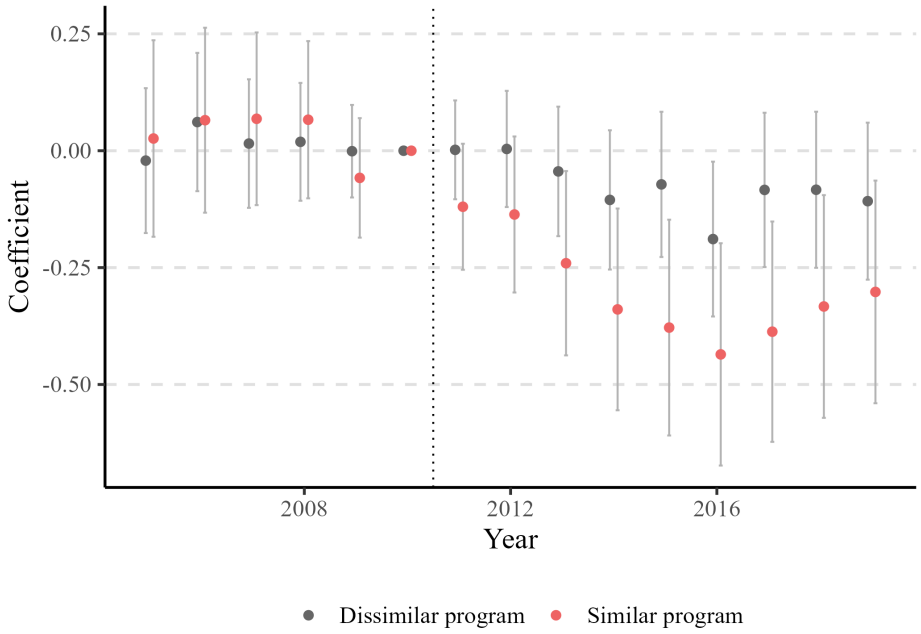


Figure 6: For-Profit Log Enrollment Responses to New TAA Programs

Notes: TWFE event-study regressions for log for-profit enrollment, where the omitted group is schools not exposed to any nearby TAA programs; treated schools are exposed to new TAA programs at some point between 2011-2014. Includes non-parametric trends for combinations of the schools' top 2-digit field and the number of fields offered. Error bars are 95% CIs constructed from heteroskedasticity-robust standard errors clustered at the school-level.

treatment. Given my staggered-treatment setting, I use the estimator proposed in Callaway and Sant'Anna (2021) to ensure robustness to heterogeneous treatment effects. Figure 7 again shows large and persistent excess log enrollment losses at for-profits exposed to new programs in similar fields, relative to those exposed to new but dissimilar programs.[34]

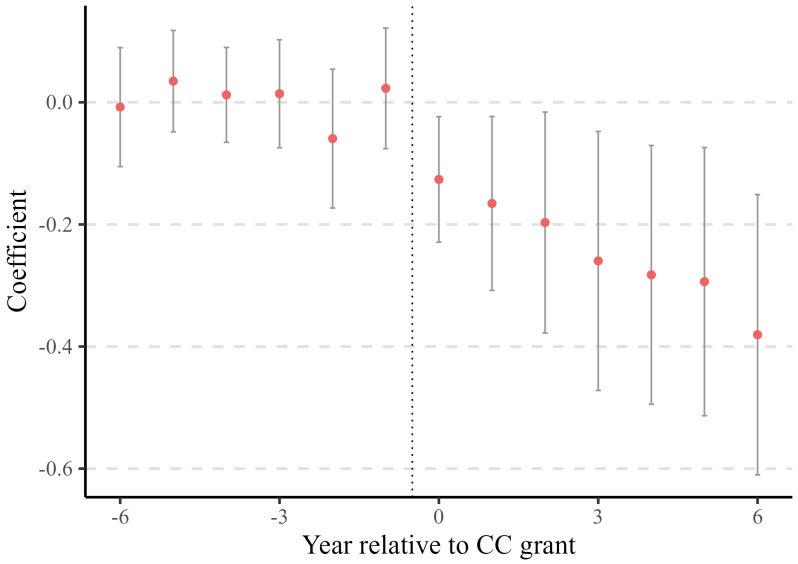


Figure 7: Excess TAA Effect on For-Profits with Similar Programs

Notes: Estimates of a Callaway and Sant'Anna (2021) event study for log for-profit enrollment, where the omitted group is for-profits experiencing nearby TAA entrants, but in dissimilar fields. Includes non-parametric trends for combinations of the schools' top 2-digit field and the number of fields offered. Error bars are 95% CIs constructed from heteroskedasticity-robust standard errors clustered at the school-level.

Endogeneity concerns may remain even without comparison to never-treated colleges. For example, to the extent that CCs choose to introduce programs in fields with growing local labor market prospects, for-profits offering similar (dissimilar) programs would have gained (lost) enrollment even absent the TAACCCT grants. Yet if local field-specific labor market shocks do drive CCs' program choices, these will tend to understate any enrollment losses at for-profits offering similar programs. Reassuringly, the estimated pre-treatment enrollment trends are parallel and quite precisely estimated, and the enrollment response dynamics appear natural given that TAA program implementation was finalized no later than four years after grant receipt.

[34]See Appendix Figure A9 for plain TWFE estimates, Gardner (2022)'s estimator, and de Chaisemartin and D'Haultfœuille (2024)'s estimator, which all show similar results.

6 Discussion

This paper makes three principal findings. First, I show that for-profit colleges do expand college access for students on the margins of higher education – only about half of for-profit students are diverted from other sectors. Second, public colleges slow the degree progression of for-profit students diverted there, which may itself contribute to for-profit college demand. Third, for-profits' appeal can be attributed in part to their narrow focus on a distinctive set of career-oriented program offerings not offered by public colleges. Taken together, these results suggest that community colleges' degree programs may fail to cater to the interests and timelines of for-profit students, as their desired vocational fields and accelerated pathways may not be available.

My results primarily speak to the extent of for-profit college differentiation and the ways in which for-profits fill gaps left open by community colleges. The analysis of substitution patterns can also be used as an input to understanding how the for-profit sector shapes college returns. Whether the for-profit sector benefits students in terms of pecuniary returns depends on both the net return to for-profit enrollment among those students on the no-college margin, as well as the net return to for-profit college relative to community college for those on the CC margin. The sign of the average return relative to no college is contested (Cellini and Turner 2019; Jepsen et al. 2025), while the latter return is likely negative on average due to significantly higher for-profit tuition. Yet my finding that community colleges significantly reduce the enrollment intensity of for-profit students suggests that for-profit students may not derive the same returns from public colleges as do CC students, since persistence and degree attainment are significant mediators of total college returns. Richer longitudinal data would allow for a deeper understanding of these forces.

Beyond helping characterize the role of for-profit colleges in human capital formation, these findings have concrete implications for the analysis of education policy. Evidence that vocational program-offering differentiation contributes to for-profit demand suggests that public colleges can become more competitive and expand their appeal to non-traditional students by investing further in CTE programs. Meanwhile, policies which constrain for-profit college supply are likely to impact students on multiple decision margins – including many for whom not attending college is a likely alternative.

These implications come with at least two caveats. Since all the for-profits in my sample have closed, we must carefully consider their external validity for the rest of the sector. I show in Appendix E that surviving for-profits resemble the chain for-profits in important ways, and they share the same key differences relative to the public sector. I also find somewhat lower estimates of diversion when expanding the sample to include all closures (Appendix Table

A6), though the identifying assumptions are less credible in this wider sample. Secondly, my estimates must be understood as partial-equilibrium results. To estimate for-profit students' second choices, it was important to hold other market features fixed. But understanding the full equilibrium impacts of for-profit closures would require estimating supply responses too.

To the extent that my findings do generalize, they help inform ongoing policy discussions. Gainful-employment rules remain a relevant tool for for-profit regulation: the Biden administration in 2023 finalized another iteration of these standards, which were projected to induce yet another wave of for-profit closures (Knott 2023). Even absent such regulatory changes, Kelchen, Ritter and Webber (2024) forecast faster rates of for-profit closures in the coming years due to declining enrollment and a "demographic cliff." These developments lend additional salience to my findings.

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A Additional Proofs

Diversion Ratios as Second-Choice Market Shares The mean diversion ratios among students originally choosing college j can also be represented as second-choice market shares:

$$\begin{aligned}
\mathbb{E}[D_{jk,i}(\boldsymbol{X}) \mid d_{ij}(p_j, \boldsymbol{X}) = 1] &= \bar{D}_{jk}(p_j, \boldsymbol{X}) \\
&= \frac{s_k(\bar{p}_j, \boldsymbol{X}) - s_k(p_j, \boldsymbol{X})}{s_j(p_j, \boldsymbol{X})} \\
&= \frac{\mathbb{E}[d_{ik}(\bar{p}_j, \boldsymbol{X}) - d_{ik}(p_j, \boldsymbol{X})]}{\mathbb{E}[d_{ij}(p_j, \boldsymbol{X})]} \\
&= \frac{\mathbb{E}[\big(d_{ik}(\bar{p}_j, \boldsymbol{X}) - d_{ik}(p_j, \boldsymbol{X})\big) d_{ij}(p_j, \boldsymbol{X})]}{\mathbb{E}[d_{ij}(p_j, \boldsymbol{X})]} \\
&= \frac{\mathbb{E}[d_{ik}(\bar{p}_j, \boldsymbol{X}) d_{ij}(p_j, \boldsymbol{X})]}{\mathbb{E}[d_{ij}(p_j, \boldsymbol{X})]} \\
&= \frac{Pr(d_{ij}(p_j, \boldsymbol{X}) = d_{ik}(\bar{p}_j, \boldsymbol{X}) = 1)}{Pr(d_{ij}(p_j, \boldsymbol{X}) = 1)} \\
&= Pr(d_{ik}(\bar{p}_j, \boldsymbol{X}) = 1 \mid d_{ij}(p_j, \boldsymbol{X}) = 1).
\end{aligned}$$

The first line applies Proposition 1 of Conlon and Mortimer (2021). The second and third lines are by definition of total diversion and market shares. The fourth line follows from the law of total expectation, and noting that exogenous closures and exclusion of p_j from u_{ik}, $k \neq j$, imply

$$d_{ij}(p_j, \boldsymbol{X}) = 0 \implies d_{ik}(\bar{p}_j, \boldsymbol{X}) = d_{ik}(p_j, \boldsymbol{X}).$$

The fifth line follows from unit demand, so that $d_{ik}(p_j, \boldsymbol{X}) d_{ij}(p_j, \boldsymbol{X}) = 0$, and the last two lines are by probability rules.

B For-Profit Closures

Figure A1 shows the intensive-margin variation in the share of closing chain campuses, for each of the 10 large chains in my sample. The plotted market shares are the chain campuses' share of the county's total first-year enrollment at non-selective, primarily sub-baccalaureate colleges, in the year before the closure.

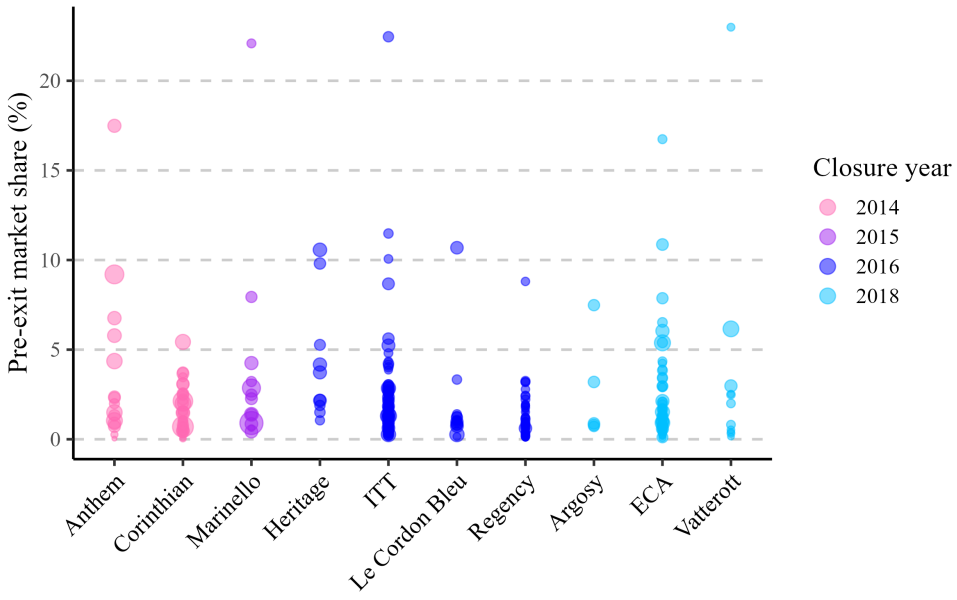


Figure A1: Chain-Campus Market Share Variation

Notes: Closure year corresponds to the year in which the chain stopped admitting new students. Corinthian shuttered its campuses in two batches between 2014 and 2015.

Most of these chains closed as a result of federal regulatory pressure. Six of the largest chains, including ITT Tech, Corinthian Colleges, Education Corporation of America, Argosy, Marinello, and Vatterott, were all directly sanctioned by DoE with financial aid restrictions just before their closure. And Anthem faced financial and legal troubles after an investigation by DoE. The remaining three chains – Regency, Le Cordon Bleu, and Heritage – did not face direct restrictions on their federal student aid participation prior to closure, instead citing worsening regulatory conditions and/or financial pressure due to declining enrollment.

Table A1 describes the timing of each closure. Several chains stopped admitting new students shortly before announcing their complete shutdown. The effective closure date – the official date of the last campus closure – falls shortly after or even concurrently with the announcement date for most chains. Note that Marinello closed 56 campuses, but IPEDS only tracks enrollment at a subset of 22. In addition, Anthem's owner continued to operate its sister chain Florida Career College after Anthem shut down. Just 10 of all these chain campuses had more than 10% of students enrolled online, and I exclude these in my analysis.

Most chains were owned by private equity groups, but some were publicly traded or a subsidiary of a publicly traded company. Their stock price movements can shed light on the abruptness and unexpectedness of their closures. ITT Tech's case is particularly illuminating, as its parent company operated no other major enterprises. On August 25, 2016, the DoE barred ITT Tech from accessing federal aid for new students, and its stock price dropped

Table A1: Timing of Chain Closures

Chain	Announcement date	Effective date	# campuses
Anthem College	8/25/2014	8/28/2014	18
Corinthian College	4/26/2015	4/27/2015	50
Marinello School of Beauty	2/4/2016	2/5/2016	22
ITT Technical Institute	9/6/2016	9/6/2016	132
Le Cordon Bleu	12/16/2015	9/29/2017	16
Regency Beauty Institute	9/28/2016	9/28/2016	82
Heritage College	11/1/2016	11/1/2016	10
Argosy University	7/10/2018	3/8/2019	35
Education Corporation of America	12/5/2018	12/21/2018	70
Vatterott College	12/17/2018	12/17/2018	16

Notes: Corinthian announced a portion of these closures on July 3, 2014. In some cases, Anthem operated as the Bryman School; Corinthian as Everest, Heald, and WyoTech; Heritage as Missouri College; Argosy as the Art Institute; Education Corporation of America as Brightwood College, Virginia College, Golf Academy of America, and the Ecotech Institute; and Vatterott as L'Ecole Culinaire. Chains generally stopped admitting new students on or before the announcement date. The effective date is later in some cases as they continued to teach current students.

75% by the following day (Figure A2; Wall Street Journal 2025). Within days, ITT Tech announced its nationwide closure. This case study shows both the abruptness of this largest chain closure, as well as the extent to which profitability depends on access to federal aid.

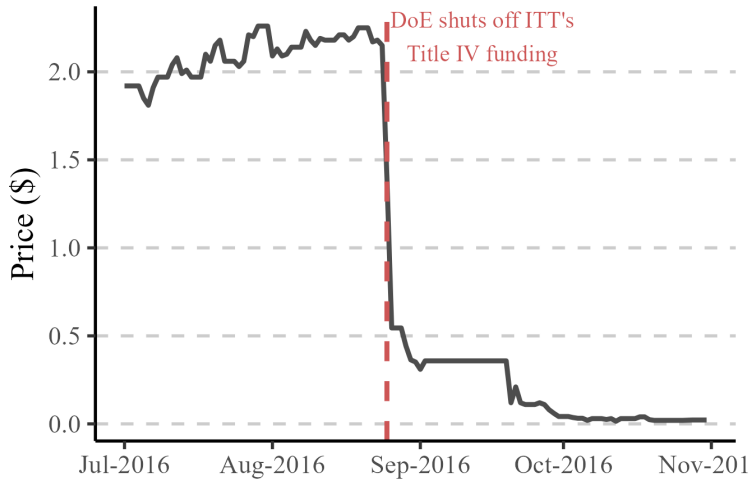


Figure A2: Stock Price of ITT Tech

Notes: Share price of ITT Educational Services, Inc. (ESINQ) before and after DoE's announcement on August 25, 2016, that ITT Tech would lose Title-IV funding eligibility.

C Characteristics of For-Profit and Public Sectors

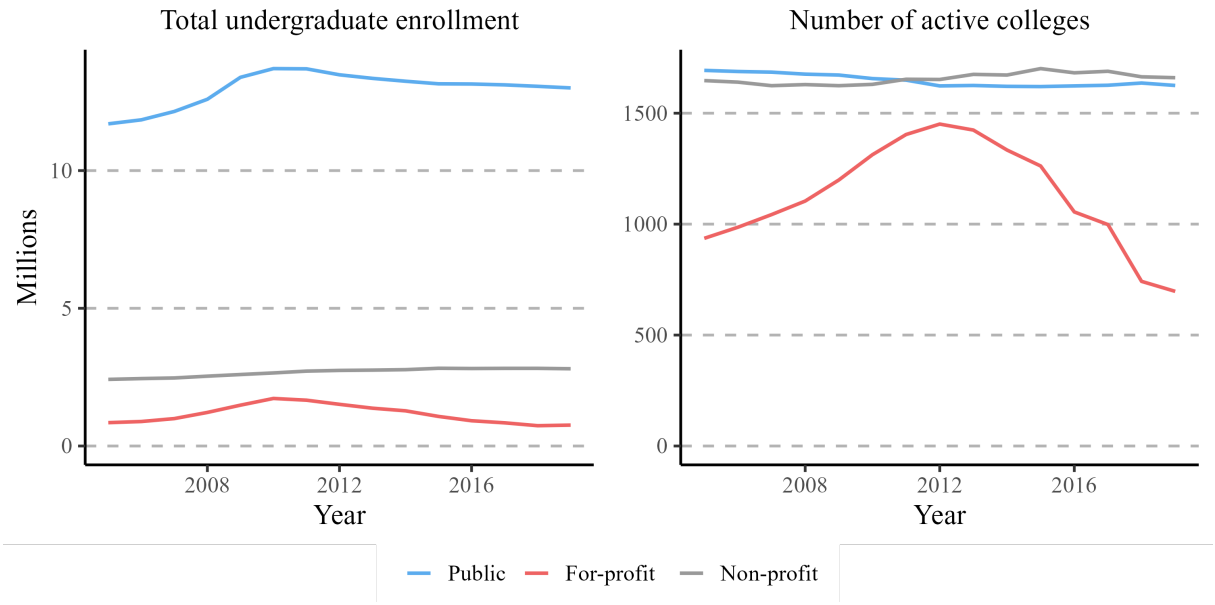


Figure A3: Enrollment and Net Entry by Sector, 2005-2019

Notes: Data from NCES Tables 303.70 and 317.10 (National Center for Education Statistics 2017-2021). Plotted are the time series of total undergraduate fall enrollment in degree-granting colleges, and the count of active degree-granting colleges (including branch campuses).

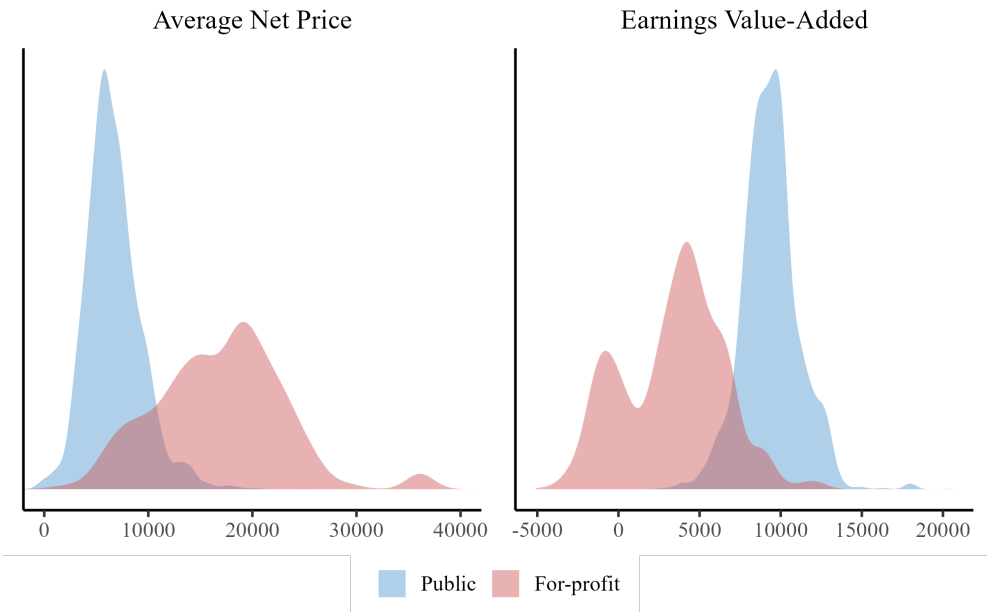


Figure A4: Distributions of Net Price and Value-Added

Notes: Non-selective, sub-baccalaureate colleges only. Densities weighted by enrollment. Value-added estimates from Armona and Cao (2024).

Table A2: Mean Institution Characteristics by Control

Panel A: *Core characteristics*	Public	Non-profit	For-profit
Full-time enrollment	3,982	508	393
Tuition & fees ($)	3,792	13,826	15,229
Graduated $\leq$ 150% of expected time (%)	33.8	54.5	57.1
Earnings value-added ($)	8,948	3,755	2,168
Offers < 1-year certificate (%)	82.2	40.0	60.4
Offers 1-year certificate (%)	96.2	65.0	81.9
Offers Associate's (%)	76.2	51.4	30.4
Offers Bachelor's (%)	6.3	23.9	9.6
# degree programs	53.9	9.7	5.8
# 2-digit CIP offerings	14.9	3.5	2.2
Panel B: *Flexbility and other amenities*	Public	Non-profit	For-profit
Offers online classes (%)	81.3	29.1	11.7
% students fully online	7.7	1.3	0.4
% students partly online	15.0	5.1	1.6
Offers weekend/evening (%)	50.9	46.1	44.9
Continuous calendar (%)	7.1	28.6	55.1
Offers remedial services (%)	96.2	54.6	27.8
Offers academic counseling (%)	98.0	92.5	79.9
Offers on-campus jobs (%)	84.0	68.2	49.2
Offers placement services (%)	80.4	75.0	92.8
Offers daycare (%)	40.0	6.1	0.6
Panel C: *Financial characteristics*	Public	Non-profit	For-profit
Total revenue per student ($)	14,922	21,640	15,891
Total spending per student ($)	14,116	19,422	14,413
Instructional spending per student ($)	6,390	7,880	5,373
Services spending per student ($)	2,444	4,582	5,597
Number of colleges	1,326	280	2,216

Notes: Sample includes all non-selective primarily sub-baccalaureate Title-IV-eligible colleges, not primarily online. Characteristics drawn from 2010-2013 IPEDS files (prior to the chain for-profit closures).

Table A3: Uniqueness of For-Profit Degree Programs by Field

Field	Share of For-Profit Programs (%)	Offered by Other Local College (%)	Offered by Local Public College (%)
Health	36.9	51.3	46.6
Personal Services	25.2	25.9	24.7
Business	9.9	51.2	49.5
IT	7.5	40.3	38.3
Engineering Tech	5.1	47.7	46.8
Security Services	3.9	25.6	22.2
Legal Services	3.0	63.0	62.7
Mechanical	2.5	43.6	41.6
Arts	2.4	32.3	31.3
Construction	0.8	35.6	34.7

Notes: Fields include the top ten 2-digit CIP programs offered by for-profits. Displayed are the shares of all 6-digit sub-baccalaureate for-profit programs in each field which are also offered by non-profit two-year colleges in the same county. Program offerings drawn from 2013 IPEDS files (prior to the chain for-profit closures).

Table A4: Student Characteristics by Control

	Public	Non-profit	For-profit
Median parental income ($)	49,800	38,000	30,200
Median student income ($)	16,100	19,000	13,300
Independent (%)	57.2	69.9	78.6
High school diploma (%)	84.6	83.9	79.8
Age at entry $\geq$ 20 (%)	36.3	41.6	51.4
Full-time job (%)	38.6	45.4	46.4
Veteran (%)	5.4	5.7	9.5
Single parent (%)	15.1	21.5	32.8

Notes: Non-selective colleges only. Parental income is conditional on being dependent, while student income is conditional on being independent. Data from author's calculation using 2016 NPSAS via PowerStats (https://nces.ed.gov/datalab/powerstats/default.aspx).

D Additional Diversion Estimates

Table A5: Unweighted Diversion Estimates

	Logit	First Differences		Wald
	(1)	(2)	(3)	(4)
Panel A: Full-time				
Public	0.76	0.03	0.09	0.17
	(0.01)	(0.16)	(0.24)	(0.26)
Non-profit	0.02	0.05	0.09	0.00
	(0.002)	(0.06)	(0.10)	(0.07)
Other for-profit	0.13	−0.13	−0.11	−0.09
	(0.01)	(0.10)	(0.12)	(0.14)
Total	0.00	−1.00	−0.86	−0.68
	(0.00)	(0.16)	(0.22)	(0.33)
Panel B: Part-time				
Public	0.08	0.17	0.40	0.51
	(0.001)	(0.13)	(0.16)	(0.25)
Non-profit	0.00	0.00	0.00	0.00
	(0.00)	(0.004)	(0.01)	(0.01)
Other for-profit	0.01	0.08	−0.04	−0.02
	(0.001)	(0.08)	(0.05)	(0.05)
Total	0.00	0.21	0.30	0.42
	(0.00)	(0.13)	(0.18)	(0.26)
$\geq 2\%$ closure share				✓
$\leq 20\%$ closure share			✓	
Calendar-year FE		✓	✓	
Observations	251	251	248	181

Notes: Column (1) presents the mean diversion ratios predicted by a plain logit demand model with no substitution to the outside good, and with fixed full-time/part-time status. Columns (2)-(3) present the first-differences regressions with year FE. Column (4) presents the mean of closure-specific Wald estimators $\tilde{W}_K$. Heteroskedasticity-robust standard errors in parentheses.

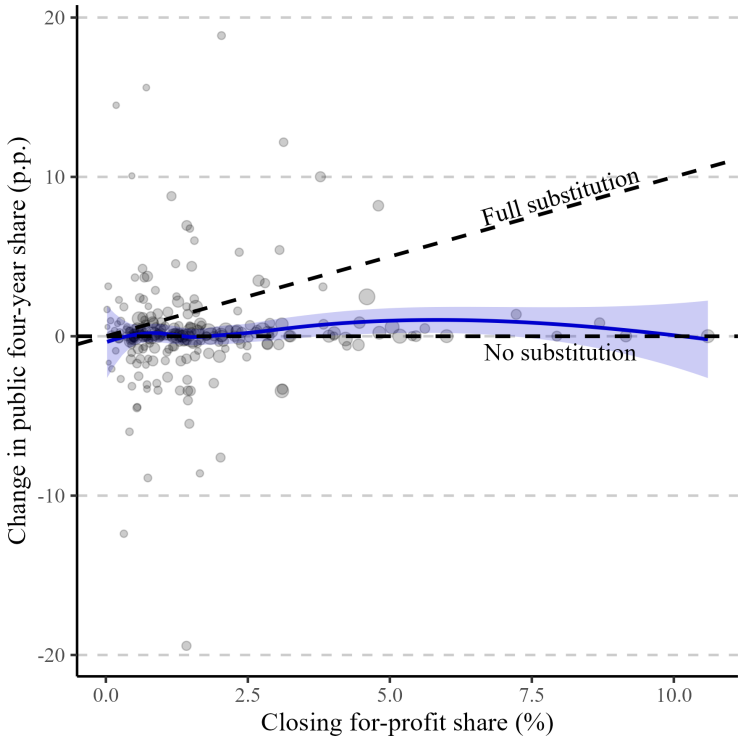


Figure A5: Four-Year Public Diversion in Unadjusted Data

Notes: Based on the market definition in Column (2) of Table 4, which includes all non-selective four-year publics in the county and, where none exists, the nearest such out-of-county college. Blue curve represents estimated diversion. Shaded regions are 95% pointwise confidence bands for the predicted mean of a loess regression. Dot size proportional to closing-campus enrollment.

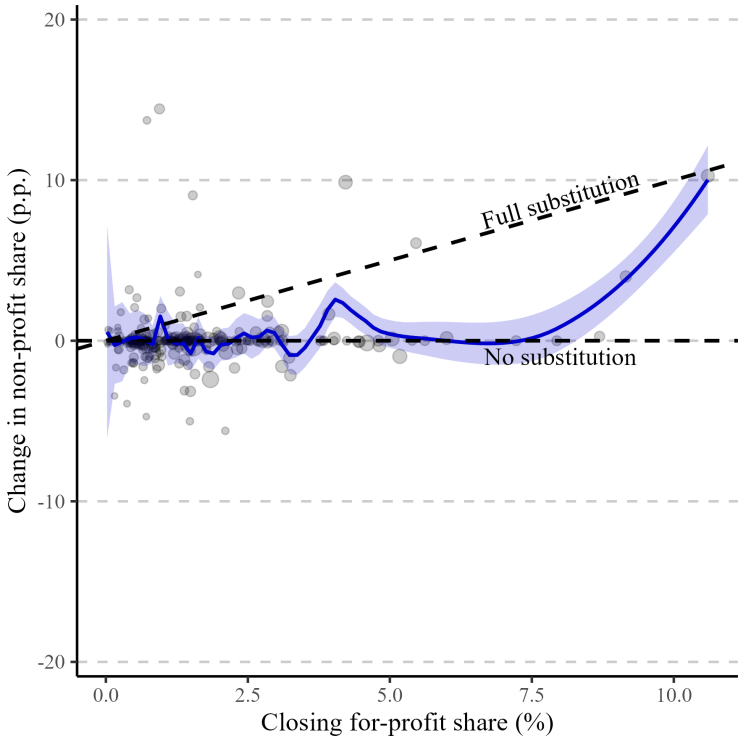


Figure A6: Non-Profit Diversion in Unadjusted Data

Notes: Based on the market definition in Column (2) of Table 4. Blue curve represents estimated diversion. Shaded regions are 95% pointwise confidence bands for the predicted mean of a loess regression. Dot size proportional to closing-campus enrollment.

Table A6: Diversion Ratios from All Closing For-Profits to Other Sectors

	First Differences		Wald
	(1)	(2)	(3)
Public			
Full-time	0.09	−0.003	−0.01
	(0.08)	(0.22)	(0.17)
Part-time	0.13	0.31	0.14
	(0.06)	(0.14)	(0.15)
Non-profit	0.02	0.09	0.02
	(0.02)	(0.11)	(0.09)
Other for-profit	−0.06	−0.01	−0.10
	(0.10)	(0.11)	(0.10)
Total enrollment			
Full-time	−0.88	−0.83	−0.88
	(0.11)	(0.21)	(0.22)
Part-time	0.09	0.21	0.05
	(0.05)	(0.16)	(0.15)
$\geq 2\%$ closure share			✓
$\leq 20\%$ closure share		✓	
Calendar-year FE	✓	✓	
Observations	606	595	473

Notes: Analogue of Table 1, but expanding the sample to all for-profit closures. Columns (1)-(2) present the first-differences regressions with year FE. Column (3) presents the mean of closure-specific Wald estimators $\tilde{W}_K$. Heteroskedasticity-robust standard errors clustered by county in parentheses.

D.1 IV Estimates: Across-Sector Diversion

The prior results describe substitution rates from a particular set of for-profit colleges to other groups of colleges, including other for-profits. An alternative notion of *across-sector* diversion asks where students would substitute if the entire for-profit sector became unavailable, rather than only the student's first-choice for-profit. When students barred from their first-choice for-profit instead substitute to another for-profit college, we do not observe their second-choice sector, so these students do not inform estimates of across-sector diversion. Since within-sector diversion is very low, approximately zero, this distinction turns out not to matter much in practice. Still, here I present the results of an IV specification which yields exactly the across-sector diversion for those students induced to switch out of the sector by the chain closures.

In particular, I regress the change in public-sector market share on the change in total for-profit market share, instrumenting for the latter with the change in the closing-chain campus share (i.e., its pre-exit market share).[35] The first stage is strong, consistent with the earlier result of low-to-zero diversion to other for-profits. The F-statistic is 137.1 in the main specification and 131.3 when excluding outliers.

In Table A7, I present IV results along with the OLS version to show that naively correlating changes in sector shares may lead to biased estimates of substitution rates, likely due to unobserved local college demand shocks. Positively correlated shocks to the inside-good utilities cause sector shares to move together, so that the naive OLS estimates suggest almost no substitution at all (precise zeroes). Point estimates show larger substitution of 30-41% to part-time public enrollment, consistent with this source of bias. Again, we see that the for-profit sector promotes full-time enrollment: although 90% of for-profit students enrolled full-time, between 73-85% of for-profit students choose not to enroll full-time anywhere else. Full-time enrollment losses are partially recuperated with additions to total part-time enrollment of 28-33%, whereas we would expect a decline of 10% under no substitution. Finally, Table A8 presents unweighted versions of these results.

[35] Algebraically, these IV estimates simply scale up the estimated diversion to the public sector by the out-of-sector diversion rate, i.e., one minus the diversion to other for-profits. The public and non-profit diversion estimates in Table 1 are the reduced-form of this IV system. To keep the same interpretation of the coefficients, I use the decline in for-profit market share as the endogenous variable (multiplying the change in for-profit share by negative one).

Table A7: Across-Sector Diversion (Weighted)

	OLS	IV	
	(1)	(2)	(3)
Panel A: Full-time			
Public	−0.05	−0.03	−0.005
	(0.11)	(0.28)	(0.32)
Non-profit	0.06	0.17	0.20
	(0.05)	(0.15)	(0.18)
Total	−0.90	−0.85	−0.73
	(0.08)	(0.19)	(0.21)
Panel B: Part-time			
Public	0.09	0.30	0.41
	(0.07)	(0.16)	(0.18)
Non-profit	0.002	−0.000	−0.000
	(0.002)	(0.004)	(0.004)
Total	0.01	0.28	0.33
	(0.07)	(0.16)	(0.19)
First-stage regression	–	1.00	1.07
		(0.16)	(0.16)
≤ 20% closure share			✓
Calendar-year FE	✓	✓	✓
Observations	251	251	248

Notes: Estimates come from regressions of changes in other sectors' market shares on changes in the total for-profit share, instrumented with the closing for-profits' share in Columns (2) and (3). F-statistics are 137.1 and 131.3 for the IV specifications in Columns (2) and (3), respectively. All estimates weighted by closing-campus enrollment. Heteroskedasticity-robust standard errors in parentheses.

Table A8: Across-Sector Diversion (Unweighted)

	OLS	IV	
	(1)	(2)	(3)
Panel A: Full-time			
Public	−0.03	0.03	0.08
	(0.09)	(0.15)	(0.21)
Non-profit	0.03	0.05	0.08
	(0.02)	(0.06)	(0.09)
Total	−0.89	−0.95	−0.75
	(0.09)	(0.15)	(0.18)
Panel B: Part-time			
Public	0.05	0.16	0.35
	(0.06)	(0.12)	(0.15)
Non-profit	0.003	0.002	0.002
	(0.003)	(0.004)	(0.01)
Total	−0.06	0.20	0.26
	(0.07)	(0.13)	(0.16)
First-stage regression	–	1.04	1.15
		(0.13)	(0.12)
≤ 20% closure share			✓
Calendar-year FE	✓	✓	✓
Observations	251	251	248

Notes: Estimates come from regressions of changes in other sectors' market shares on changes in the total for-profit share, instrumented with the closing for-profits' share in Columns (2) and (3). F-statistics are 162.9 and 128.4 for the IV specifications in Columns (2) and (3), respectively. Heteroskedasticity-robust standard errors in parentheses.

E External Validity

Given that my empirical results exclusively use two-year for-profit chain closures, in order to assess their external validity for the broader sector it may be helpful to compare the characteristics of chain for-profits with those of other two-year for-profits which remained open. Prior to the wave of closures, chains accounted for about one-third of the entire sector's enrollment as of 2009 (Deming, Goldin and Katz 2012). In addition, by taking a relatively large sample of chains distributed across the U.S., my sample is quite diverse in its characteristics, with primary program offerings spanning culinary school, cosmetology, IT, business, healthcare, and arts. In any case, my results will be less informative about four-year for-profits which, as described in Section 2.2, differ significantly from the two-year sector.

Table A9: Balance of Mean Characteristics: Chains vs. Other For-Profits

	Unweighted		Enrollment-weighted	
	Chain	Other for-profit	Chain	Other for-profit
FTE enrollment	663	333	3,312	1,399
Tuition & fees ($)	16,706	14,897	16,311	17,322
Graduation rate (150%)	42.9	60.1	47.2	59.4
Earnings value-added ($)	4,180	1,641	5,112	3,693
Offers < 1-year certificate (%)	23.1	68.7	29.2	59.1
Offers 1-year certificate (%)	63.5	85.9	71.2	89.6
Offers Associate's (%)	67.7	22.1	83.7	46.6
Offers Bachelor's (%)	32.5	4.5	28.5	11.6
# degree programs	9.7	5.0	11.9	8.9
# 2-digit CIP offerings	3.6	1.9	4.1	2.9
Offers online classes (%)	38.5	5.6	36.2	12.2
Offers weekend/evening (%)	53.8	42.9	49.3	58.4
Continuous calendar (%)	39.0	58.7	25.3	44.8
Total revenue per student ($)	18,040	15,487	12,521	12,984
Total spending per student ($)	16,210	14,076	9,838	11,521
Instructional spending per student ($)	4,713	5,497	3,105	3,960
Services spending per student ($)	8,049	5,136	4,893	5,197
Number of colleges	403	1,813	403	1,813

Notes: College characteristics from 2010-2013 IPEDS data, prior to the chain closures. Value-added estimates from Armona and Cao (2024). Restricting to non-selective, primarily sub-baccalaureate colleges which are not primarily online. Weighted means weight by annual full-time equivalent (FTE) enrollment.

Table A9 provides further concrete evidence on similarities and differences between closing chains and the rest of the sector which remained open through 2019, using school and student characteristics from IPEDS. I show comparisons to non-selective two-year for-profits, which

are not primarily online. The closing chains are larger than other for-profits, but they are both quite small compared to CCs. They charge very similar average tuition, but closing chains yielded larger gross returns according to estimates from Armona and Cao (2024).[36] While both groups offer significantly higher graduation rates than CCs, surviving for-profits' graduation rates are even higher. In terms of program offerings, chains tend to offer longer degree programs than other for-profits, and they offer a somewhat wider array of fields of study. They are less likely to operate on continuous academic calendars but more likely to offer some online classes. On financial measures, chains closely resemble surviving for-profits: they have similar per-student revenue and expenditures, and both spend more on academic services and support than on instruction (weighting by enrollment).

Given the importance of field-of-study offerings in explaining substitution patterns, I plot in Figure A7 the distributions of program completions by field. Both chains and other for-profits specialize heavily in health degrees and "personal services" degrees (including cosmetology and culinary school). The chains are relatively more represented in the fields of IT, engineering, and security, and relatively less so in the "handy trades" like construction and automobile repair. Unlike public colleges, virtually none offer degrees in the liberal arts, natural science, and social science.[37] For substitution patterns, what matters is not aggregate program similarity, but the similarity of a given for-profit college with other colleges in its local market. Figure A8 displays the distribution of local for-profit–public program similarity, by whether the for-profit is a closing chain or another for-profit which remained open. Most for-profit colleges are quite dissimilar from local public options in this dimension, and if anything the non-chain for-profits are even more differentiated from public colleges.

Overall, the closing chains and other for-profits share key similarities in terms of tuition, spending, size, graduation rates, and program offerings. Where they do differ, the surviving for-profits are generally even *less* similar to community colleges – they have even smaller student bodies, higher graduation rates, less traditional academic calendars, fewer online classes, narrower program offerings, and even more distinctive program specialties. To the extent that for-profit students sort on the basis of these characteristics, we would expect public colleges to be less closely substitutable with these other for-profit colleges. Consistent with this view, when expanding the set of closures to include non-chain for-profits, I find somewhat lower estimated diversion ratios (Appendix Table A6). Still, other potential drivers of college demand remain unobserved, so these mean differences should only be taken as suggestive evidence about the substitution patterns of other for-profit students. For in-

[36] The lower value-added among surviving for-profits is driven by cosmetology schools.

[37] To quantify how similar these distributions are, I calculate the cosine similarity of different sectors' aggregate CIP program completion vectors. Chain and other for-profit colleges have a similarity of 0.942, compared to just 0.599 (0.586) between chain (other for-profit) and public colleges.

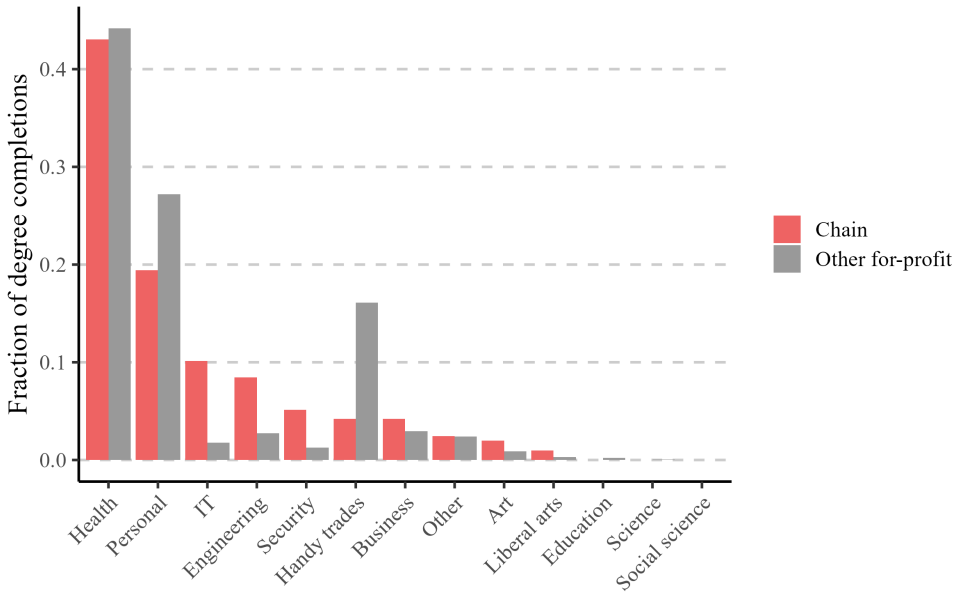


Figure A7: Programs of Study: Chain vs. Other For-Profit

Notes: Degree completions by program taken from 2011-2015 IPEDS files. Restricting to non-selective, primarily sub-baccalaureate colleges which are not primarily online. Programs of study correspond to 2-digit CIP codes, sometimes grouped: "Handy trades" groups 46-49; "Engineering" groups 14-15; "Liberal arts" groups 23, 24, 30, 38, 54; and "Science" groups 26, 27, 40.

stance, those who enrolled in closing chains may have been even less informed about their college options than other for-profit students, since much of the fraud and scandals plaguing the for-profit sector were concentrated among the closing chains.

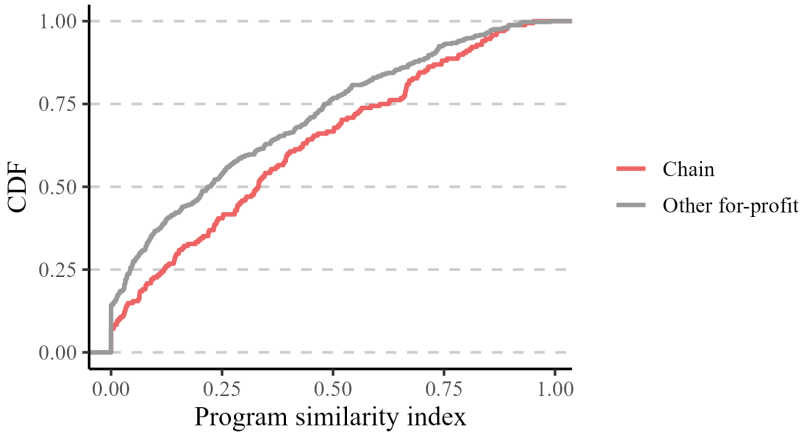


Figure A8: Similarity of For-Profit and Public Programs

Notes: Degree completions by program taken from 2011-2015 IPEDS files. Chain similarity distribution contains 168 county-level markets, while other for-profit distribution contains 410 (omitting markets without a two-year public college).

F Alternative TAACCCT Specifications

Next I plot alternatives to Figure 7 using four different event-study estimators. These measure the log for-profit enrollment effects of new TAA-funded CC programs in fields similar to nearby for-profits, relative to new but dissimilar programs.

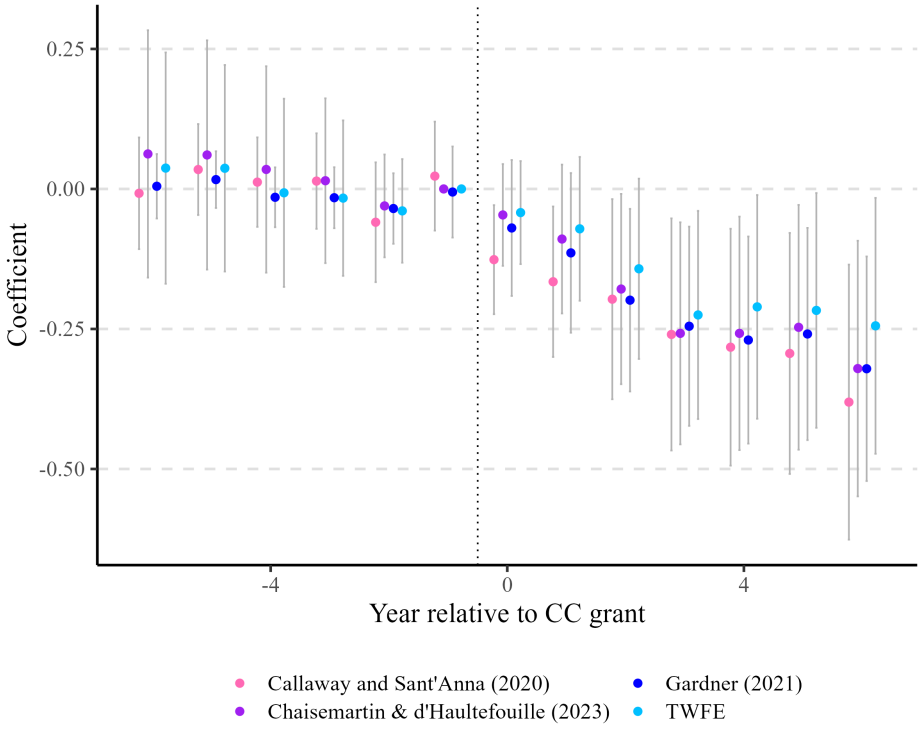


Figure A9: Excess TAA Effect on For-Profits with Similar Programs

Notes: Event studies for log for-profit enrollment, where omitted group is for-profits exposed to nearby TAA entrants, but in dissimilar fields. All include non-parametric trends for combinations of the schools' top 2-digit field and the number of fields offered. Error bars are 95% CIs.

Table A10: For-Profit Enrollment Effects of New TAA Programs: Diff-in-Diff Estimates

	Linear			Log-linear			Poisson		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dissimilar program	−1.6	0.2	−0.7	−0.11	−0.10	−0.15	−0.03	−0.02	−0.05
	(2.9)	(3.0)	(3.3)	(0.04)	(0.04)	(0.05)	(0.10)	(0.11)	(0.11)
Similar program	−30.6	−29.0	−32.9	−0.32	−0.31	−0.29	−0.25	−0.23	−0.23
	(5.4)	(5.2)	(6.0)	(0.06)	(0.06)	(0.06)	(0.10)	(0.10)	(0.11)
State trends	✓	✓	✓	✓	✓	✓	✓	✓	✓
Local industry trends		✓	✓		✓	✓		✓	✓
Omitting chains			✓			✓			✓
Observations	25,808	25,808	22,624	25,808	25,808	22,624	25,808	25,808	22,624

Notes: The omitted group is schools not exposed to any nearby TAA program. Pre-treatment years include 2007-2010, and post-treatment years include 2015-2018; intervening grant years 2011-2014 omitted. All specifications include non-parametric trends for combinations of the schools' top 2-digit field and the number of fields offered, and non-parametric state-specific trends. Some specifications also include non-parametric trends for different pre-treatment local industry concentrations: local industry groups defined based on above-/below-median healthcare and manufacturing employment, using County Business Patterns data with imputations by Eckert et al. (2021). Some specifications also omit the closing for-profit chains. Heteroskedasticity-robust standard errors clustered at the school-level in parentheses.