

Where Do Students Go when For-Profit Colleges Lose Federal Aid?

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We examine the effects of federal sanctions imposed on for-profit institutions in the 1990s. Using county-level variation in the timing and magnitude of sanctions linked to student loan default rates, we estimate that sanctioned for-profits experience a 68% decrease in annual enrollment following sanction receipt. Enrollment losses due to for-profit sanctions are 60-70% offset by increased enrollment within local community colleges, where students are less likely to default on federal student loans. Conversely, for-profit sanctions decrease enrollment in local unsanctioned for-profit competitors, likely due to improved information about local options and reputational spillovers. Overall, market enrollment declines by 2%.

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After more than a decade of substantial growth, for-profit higher education has been the target of increased scrutiny, new regulation, and heated debate in recent years.¹ For-profit colleges may expand the market for higher education and provide important pathways to college for underserved students, but high student loan default rates, accusations of unethical marketing practices, and allegations of financial aid fraud sparked a wave of government investigations beginning in 2010 (U.S. Government Accountability Office 2010; U.S. Senate Committee on Health, Education, Labor and Pensions 2010). Under the Obama administration, the U.S. Department of Education’s (ED’s) “Gainful Employment” (GE) regulations sought to restrict access to federal student aid at for-profit colleges

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¹For-profit institutions currently enroll over 1.4 million students, down from a peak of 2 million students in 2010. Authors’ calculations using Table 301.10 of U.S. Department of Education (2018).

where graduates had high student loan payments and low earnings. (U.S. Department of Education 2010). ED also imposed sanctions on two large national for-profit chains - Corinthian Colleges and ITT Technical Institute.² Both companies subsequently filed for bankruptcy, shuttering hundreds of campuses across the country and leaving behind tens of thousands of students (Fain 2014*a*; Stratford 2015; Smith 2016). The Trump administration has signaled a reversal of these policies by reopening negotiated rule-making and proposing the elimination of GE accountability measures (Fain 2018), but the fate of the rule, and with it for-profit institutions and their students, remains uncertain. As these policies are debated, a key concern is how students will fare if colleges lose access to federal student aid or are otherwise induced to close by federal regulation. Previous research shows that restrictions on federal student aid at for-profit colleges led to enrollment declines within sanctioned institutions (Darolia 2013), but a key unanswered question for assessing the welfare implications of such restrictions is whether students in affected institutions switch to other institutions or exit higher education.

In this paper, we seek to answer this question by providing the first causal estimates of how restrictions on for-profit institutions' eligibility to provide students with federal aid affect market-level access to higher education and the distribution of students across local public, private nonprofit, and private for-profit institutions. Specifically, we quantify the extent to which the threat of student aid restrictions for an additional for-profit institution affects enrollment in nearby unsanctioned competitor institutions. To carry out our analysis, we exploit variation from regulations imposed in the late 1980s and early 1990s that were similar to the restrictions considered in current policy debates. As in the current case of GE, the previous round of regulations restricted institutions' eligibility to disburse federal student aid to current students if the institutions' alumni had difficulty repaying their student loans. While these cohort default rate (CDR) regulations applied to all colleges that participated in federal aid programs, the vast majority of affected institutions were for-profits. We show that the CDR regulations led to widespread for-profit enrollment declines and closures.

Our empirical approach is a generalized difference-in-differences strategy. We exploit the differential timing of sanctions from county to county as well as differential exposure based on enrollment within sanctioned institutions in the local market. We implement our empirical design using administrative data on Pell Grant recipients - students who are most affected by access to federal aid. Thus, we directly examine whether vulnerable students, whose enrollment decisions may be especially tied to student aid access, are affected by federal regulation. Our use of administrative data on Pell Grant recipient enrollment also represents an

²Regulators threatened to cut off the colleges' access to federal student aid for misrepresenting job placement rates of graduates (Corinthian) and failing to comply with accreditation standards (ITT Tech). See <http://www.ed.gov/news/press-releases/us-department-education-heightens-oversight-corinthian-colleges> and <http://www.ed.gov/news/press-releases/department-education-bans-itt-enrolling-new-title-iv-students-adds-tough-new-financial-oversight>.

improvement over the data used in prior estimates of the effects of for-profit sanctions on enrollment and in examinations of the for-profit sector more generally before 2000, since the institution-level data most commonly used – ED’s Integrated Postsecondary Education Data System (IPEDS) – excludes a large number of for-profit institutions that were in operation and potentially sanctioned in the early 1990s. These more comprehensive data are especially important when analyzing the market-level response to sanctions. We define markets locally since the time period we study is prior to widespread broadband penetration and nearly all sanctions were imposed on institutions offering two-year (or shorter) credentials. Consequently, most prospective for-profit students’ choice sets were limited to colleges in their immediate vicinity, allowing us to capture the full set of enrollment effects in a given market.

Our results show large market-level effects of sanctions on the distribution of students across schools and sectors. On average, annual Pell Grant student enrollment at sanctioned for-profit institutions fell by 68 percent in the eight years following sanction receipt. An additional for-profit sanction led to an approximately 7 percent increase in enrollment for the average local community college in the same market. Across all community colleges within a local higher education market, the increase in enrollment following a for-profit college sanction, albeit small relative to community colleges’ overall size, compensated for almost 70 percent of the sanction-driven enrollment decline at for-profit schools. In contrast, Pell Grant enrollment at unsanctioned for-profit institutions fell by 1.5 percent after a local for-profit competitor was sanctioned. Accounting for these negative enrollment spillovers, the public sector absorbed 60 to 70 percent of the total enrollment decline due to a for-profit sanction. Overall, an additional for-profit sanction resulted in an approximately two percent decrease in Pell Grant recipient enrollment within the local higher education market.

We next examine the mechanisms driving the negative spillovers to for-profit competitors. We find evidence of reputational effects within schools offering similar fields of study. Negative enrollment spillovers are larger in magnitude for institutions focused on the same industry (e.g., beauty, business) as the sanctioned for-profit institution relative to institutions that specialize in other fields. This result suggests that sanctions improve student information about the quality and costs of colleges in the field and ultimately lead students to alter their choices.

Beyond school attendance, what are the broader effects of sanctions on students? Given that the regulations aimed at schools with abnormally high student loan default rates, student borrowing and default are natural places to start. We find that sanctions are associated with sizable reductions in marketwide borrowing and default in the for-profit sector. For-profit sanctions lead to proportionally smaller increases in borrowing and defaults in community colleges. Back-of-the-envelope estimates suggest that roughly 50 percent of the students induced to stop borrowing after a for-profit school was sanctioned would have defaulted on

their loans in the absence of the sanction. Therefore, even with the decrease in aggregate market enrollment, for-profit sanctions appear to shift many vulnerable students to colleges where they were likely to reduce borrowing and be less likely to default.

Our findings contribute to the literature in three ways. First, our paper adds to a growing body of research on for-profit postsecondary education. Several recent studies describe for-profit business practices and missions, advising systems, recruitment, students, faculty work, online learning, and costs.³ Researchers generally find similar or negative returns to for-profit college attendance relative to other sectors (e.g., Deming, Goldin and Katz 2012; Cellini and Chaudhary 2014; Lang and Weinstein 2013; Cellini and Turner forthcoming). These findings are supported by Darolia et al. (2015) and Deming et al. (2016) who use resume audit study experimental designs and find that for-profit attendance is equally or less likely to generate job interview requests than public sector attendance.

Second, our paper also contributes more broadly to research on the supply side of the market for higher education. The question of whether for-profit and public institutions compete for students is independently important given the overlap in programs offered by two-year public and for-profit institutions (e.g., Cellini 2009) and disparate costs (e.g., Laband and Lentz 2004; Cellini 2012). Two recent papers examine enrollment spillovers between the for-profit and public sectors due to changes in prices, resources, or institutional availability and find evidence of substitution (Goodman and Henriques forthcoming; Armona, Chakrabarti and Lovenheim 2018). Similarly, our analysis contributes to broader debates in education policy, as issues of competition and public-private crowd-out arise in debates over universal preschool, charter schools, and voucher programs (e.g., Epple, Figlio and Romano 2004; Bassok, Fitzpatrick and Loeb 2014).

Finally, this study contributes to the broader literature on the effects of federal student aid policy. Research on the impact of federal student aid on enrollment and persistence has produced mixed results. Pell Grant aid does not appear to increase college entry among traditional-aged students (Kane 1995; Rubin 2011; Carruthers and Welch 2015; Turner 2017; Marx and Turner 2018), although it may enhance enrolled students' attainment and older individuals' initial enrollment decisions (Seftor and Turner 2002; Denning forthcoming; Denning, Marx and Turner forthcoming). Student loans have also been shown to impact college choice (e.g., Leslie and Brinkman, 1987; Hoxby and Avery 2004) and attainment, especially among community college students (Dunlop 2013; Wiederspan 2016; Marx and Turner forthcoming). Several studies provide evidence that higher ed-

³For research on for-profit institutions' business practices, see Breneman, Pusser and Turner (2006), Kinser (2007), and Tierney and Hentschke (2007). Rosenbaum, Deil-Amien and Person (2006) examine for-profit institutions' advising systems. Chung (2012) and Deming, Goldin and Katz (2012) provide information on the characteristics of for-profit students while Lechuga (2008) focuses on for-profit faculty. Gilpin, Saunders and Stoddard (2015) examine whether for-profit colleges are responsive to labor markets. Cottom (2017) examines recruiting practices and Bettinger et al. (2017) assess online learning in for-profit colleges. Finally, Laband and Lentz (2004) and Cellini (2012) assess the costs related to receiving a for-profit postsecondary education.

ucation institutions respond to federal student aid by altering tuition or institutional grants (e.g., Singell and Stone 2007; Turner 2012; Turner 2017). For-profit institutions may have particularly strong reactions to changes in access to federal aid programs, as these institutions may receive up to 90 percent of their revenue from federal student aid. Research on the for-profit sector suggests that both college openings and tuition are responsive to federal student aid (Cellini 2010; Cellini and Goldin 2014).

The remainder of this paper proceeds as follows. In Section I, we describe federal student aid programs and the standards that institutions must meet to maintain eligibility to disburse federal aid, present a conceptual framework that highlights potential mechanisms through which sanctions could affect enrollment in competitor institutions, and discuss our primary sources of data. Section II describes our identification strategy and sample. In Section III, we present estimates of the impact of sanctions on enrollment in sanctioned institutions and their competitors, and in Section IV, we provide descriptive evidence of effects on borrowing and student loan defaults. Section V concludes.

I. Institutional Background and Empirical Setting

College students receive substantial sums of aid from the federal government. The largest federal financial aid programs, including Pell Grants, work-study, and Stafford Loans, are authorized under Title IV of the Higher Education Act of 1965 and subsequent amendments (hereafter, Title IV). Title IV programs provide subsidies to low-income college students. Colleges must comply with a set of administrative and fiscal requirements to disburse Title IV aid to students.⁴ Until the recent GE regulations passed under the Obama administration, the most stringent student performance-based requirement relied on CDRs. During the time period we study, CDRs were defined as the percentage of an institution’s former borrowers who default on their federal student loans within two years of entering repayment. Institutions were required to maintain CDRs less than 25 percent in any three-year period and less than 40 percent in any given year. In the absence of a successful appeal, institutions violating these thresholds lost access to student loans, and potentially all federal student aid, for at least the remainder of the year and the following two years.⁵ Between 60 and 90 percent

⁴Among these requirements, for-profit colleges were limited in the percentage of revenue that could be obtained through Title IV programs beginning in 1992. The “85-15 rule” required that colleges receive no more than 85 percent of their revenue through Title IV. This percentage was increased to 90 percent in 1998 and the restriction is now known as the “90-10 rule.”

⁵Exceeding the 25 percent threshold for three consecutive cohort years resulted in loss of access to loans (and potential loss of Pell Grants beginning with the 1997 cohort). Exceeding the 40 percent threshold resulted in “limitation, suspension, or termination” of all Title IV aid programs. CDR thresholds were higher in the early years of enforcement. For instance, in 1991, institutions had to maintain CDRs below 35 percent for the 1987, 1988, and 1989 repayment cohorts and below 60 percent for the 1989 cohort. In 2012, the ED moved to a three-year CDR measure and higher sanction thresholds: institutions with CDRs exceeding 30 percent for three consecutive years lose eligibility to disburse both federal Pell Grants and federal loans, while institutions with CDRs exceeding 40 percent in any single year lose access

of all sanctioned institutions appealed this decision. We do not observe whether a sanction was ultimately applied (either due to an unsuccessful appeal or lack of an appeal) prior to 1997, but of the sanctions threatened between 1997 and 2000, 95 percent eventually resulted in federal aid loss.

CDR regulations were enacted in 1989 in response to concerns of poor student outcomes and abuse of federal student aid programs in the for-profit sector (U.S. General Accounting Office 1988; Fraas 1989; Dynarski 1991). While the regulations applied to all institutions, a disproportionate number of for-profits were affected. Prior to the regulation, close to 3,000 for-profit institutions participated in Title IV federal student aid programs. As reflected in Figure 1, the first CDR-driven sanctions were announced in September 1991 and the annual number of sanctions peaked in 1994. By September 2000, over 1,200 for-profit institutions and a handful of institutions in other sectors had been sanctioned.⁶ Sanctions were imposed almost exclusively on schools offering sub-baccalaureate credentials.

Although ED did not gather complete or systematic data on for-profits in the 1980s and 1990s in the IPEDS, research using these data from the time period suggests that the market for sub-baccalaureate education in the late 1980s had many important similarities to today's market. Using the 1989 IPEDS and related National Postsecondary Student Aid Study (NPSAS) data, Apling (1993) reports that, as is the case today, for-profit "proprietary schools" enrolled a disproportionate share of women, minority students, and older students in programs ranging from cosmetology to computer science that were typically less than two years in length. Most proprietary schools were small, though a few large institutions existed. Unlike today's chains that enroll tens or even hundreds of thousands of students, however, the largest institutions served roughly 12,000 students. While online education was not prevalent in the early 1990s, some schools offered correspondence courses by mail. Hoxby (2014) estimates that among students attending non-selective colleges in 1990, about 13 percent took at least one "distance education" course and Apling (1993) reports that 235 proprietary schools with median enrollment of 800 delivered at least some coursework by mail in 1988. However, many of these schools were not eligible for federal aid under Title IV; only 68 institutions with home school programs were accredited (and therefore potentially eligible for Title IV), suggesting that relatively few federal aid recipients pursued correspondence education in the early 1990s.

to federal loans. Appendix Table C.1 displays the full set of sanction triggers and penalties by year.

⁶Between 2003 and 2013, only 27 institutions received CDR-related sanctions. Of these, 23 ultimately avoided federal aid loss due to successful appeals. Despite heightened concern about the loan repayment challenges of for-profit college students, most for-profits avoided CDR sanctions in recent years, possibly due to strategic behavior in managing defaults (as discussed in letters between Secretary of Education Arne Duncan and Senator Tom Harkin dated December 12, 2012, and February 27, 2013). The December 12 letter notes that for-profit colleges "manipulate their Office of Postsecondary Education Identification (OPE-ID) numbers to avoid potential sanctions, including loss of federal financial aid eligibility. These tactics help colleges artificially avoid violating restrictions on high default rates."

A. Conceptual Framework

We begin with a basic model of college choice, loosely following Jacob, McCall and Stange (2018) and Long (2004). Student i chooses from $j \in J^m$ colleges in local higher education market m . Institutions are characterized by the expected out-of-pocket price paid by the student P_{ij} , academic characteristics A_j (e.g., programs, quality, or reputation), and distance D_{ij} to the institution. Prices vary both across and within institutions and depend on student characteristics X_i (e.g., family income, academic ability, in-state residency) and college characteristics Z_j (e.g., degree offerings, sector). A student's income is denoted I_i , such that $I_i - P_{ij}$ represents consumption of all other goods and ε_{ij} is an unobserved individual-specific preference for institution j . Both A and P are functions of S , where $S_j = 1$ when institution j receives a CDR sanction and is unable to give out federal student aid. An institution's reputation also may depend on whether other institutions in the same market and sector $c \in \{\text{public, nonprofit, for-profit}\}$ have also been sanctioned (S_{-jc}).

Individuals assess their expected utility from attending each institution U_{ij} , while also considering the option of attending no postsecondary education, and choose the option that maximizes their utility, where:

$$(1) \quad U_{ij} = \alpha_0 (I(X_i) - P(X_i, Z_j, S_j)) + \alpha_1 A(Z_j, S_j, S_{-jc}) + \alpha_2 D_{ij} + \gamma X_i + \varepsilon_{ij}.$$

When an institution is sanctioned, it affects students' college choice decisions through three channels. First, CDR sanctions may impact a student's expected cost of attendance. The amount that a student pays out-of-pocket for college equals the gross cost of attendance, less available financial aid. A sanctioned institution loses eligibility to disburse federal loans, and in some cases, Pell Grants and other federal aid. Sanctioned institutions could use their own funds to offset the loss of aid from federal sources, but Cellini and Darolia (2017) find that for-profit colleges typically provide very little institutional aid. As a result, current and prospective students of sanctioned institutions are likely to experience an increase in their expected out-of-pocket college costs. Students could absorb these costs, for example, by taking on relatively expensive private student loans or increasing work hours. They may also choose to forgo a college education or may switch to a lower-cost competitor institution. Even if a sanction is not upheld, the threat of a sanction might still affect students' expectations of their future cost of continuing in a threatened institution.

Second, sanctions may provide information to prospective students on the quality of a particular institution or sector, reducing perceived academic quality and lowering the potential utility from attendance. Institutions sanctioned under the CDR regulations have, by definition, a high percentage of students who cannot repay their student loans. Prospective students may consider a sanction to indicate poor institutional quality and therefore estimate a lower probability of their own

success and lower expected lifetime benefits from attendance at the sanctioned institution. Such a calculation would lead to a lower probability of enrolling in college j and would induce students to enroll in a competing institution rather than forgo education altogether, unless suitable alternative institutions/programs are unavailable locally or are expected to yield lower utility than nonenrollment. A key implication for our study is that reputational effects of a sanction may also spill over onto institutions that students view as similar — leading students to switch to local institutions in a different sector or a different field of study. For example, students may downgrade their impression of other institutions in the same sector (e.g., other for-profits when a for-profit college is sanctioned) or other institutions that are perceived to be related (e.g., another local branch of an institution within the same “chain”). Prospective students also may consider a sanction of an institution that specializes in a field of study to indicate poor job prospects in that field. For these reasons, we explore whether spillovers are more pronounced among chains or among institutions with similar fields of study.

Finally, sanctions may affect the supply of postsecondary education. Sanctioned institutions may be unable to support their operations if reductions in student enrollment are sufficiently large, leading some sanctioned institutions to close. This in turn limits prospective students’ choices and induces further switching to unsanctioned competitor institutions or exit from higher education. Our reduced form estimates of the impact of sanctions on enrollment in a local market will encompass all three of these effects. We focus our analyses on students who are likely to only seek out local college options — those that attend colleges that offer two-year and less-than-two-year credentials — and approximate a student’s choice set with counties.

The extent to which students are dissuaded from education or diverted to a different institution following a sanction depends on the availability of institutions offering similarly appealing programs, prices at these institutions, and whether students are fully informed of their options beyond the sanctioned school. All of these factors relate to the degree of competition between colleges in a given higher education market, which has been addressed to a limited extent in previous literature. Peltzman (1973) provides a theoretical framework for modeling how higher education institutions compete for students in an environment with both federal subsidies (Pell Grants and federal loans) and state subsidies (direct funding for public institutions). Cellini (2009) provides evidence that public funding for California community colleges drives for-profit colleges out of the local market, with a corresponding increase in community college enrollment. Using national data, Goodman and Henriques (forthcoming) estimate that a 10 percent decrease in state and local appropriations leads to a 2 percent increase in for-profit enrollment.

B. Data Sources

We primarily rely on ED administrative data on Pell Grant recipients and on institutions subject to CDR sanctions. The Pell Grant data include the total number of Pell Grant recipients in each federal-aid eligible institution for the 1973–74 through 2011–12 (hereafter, 1974 through 2012) academic years. Pell Grant recipient enrollment provides a reasonable proxy for the enrollment of financially vulnerable undergraduate college students as students with low income and assets (measured by the federal government’s calculation of need) are eligible to receive Pell Grants. We are particularly interested in these relatively low-income and low-asset students since they are most likely to be sensitive to the loss of federal student aid and are also the target of policy efforts to encourage college attendance and completion.⁷ In addition, although estimated effects of sanctions on sanctioned schools’ enrollment using IPEDS data in Darolia (2013) are internally valid, the IPEDS is missing historical records for a non-trivial number of for-profit colleges (Online Appendix A provides additional details). Thus the Pell Grant data used in this study are better suited than for estimating changes to the total amount and distribution of market-level enrollment during this key time period.

Data on sanctioned institutions include the specific CDR threshold that was violated in all years that sanctions were applied and institution-specific default rates for all but the first three cohorts of borrowers (i.e., those who entered loan repayment between 1987 and 1989). In most years, institutions had to maintain CDRs less than 25 percent in any three-year period and less than 40 percent each year. Violation of these thresholds resulted in loss of at least some federal financial aid in the year the sanction was applied and a minimum of two additional years. Specifically, institutions that were sanctioned due to three years of CDRs exceeding 25 percent lost eligibility to disburse federal loans but maintained eligibility for grant programs. Institutions that were sanctioned due to a single year’s CDR exceeding 40 percent could lose eligibility to disburse both grants and loans for an indefinite period. Following ED’s definition, in our main specifications, our definition of a sanctioned institution includes both sets of institutions. However, we also explore heterogeneous effects by sanction type to assess whether the loss of all Title IV aid has a larger effect on enrollment than the loss of access to loans alone.

Typically, sanctions were effective immediately and restricted aid access to current and prospective students. However, a sanctioned institution could appeal its case to ED in a process that typically lasted one to two years. During this time, the institution was allowed to continue participating in Title IV programs

⁷The ratio of Pell Grant recipients to IPEDS fall enrollment is approximately 0.2 in community colleges during the period we examine. While the IPEDS excludes many for-profit institutions that were operating in the 1980s and 1990s, Apling (1993) reports that close to 50 percent of for-profit received Pell Grant aid in fall 1986 (versus 12 percent of community college students), suggesting that the share of for-profit students receiving Pell Grants may have been substantially higher than the share of students receiving Pell Grants in the public sector during this period.

but would be responsible for repaying any loans disbursed during the appeals period if the sanction was ultimately upheld. An important limitation of our data is that we cannot observe whether institutions successfully appealed a sanction before the 1997 academic year.⁸ Thus, our main estimates can be thought of as intent-to-treat (ITT) effects that encompass both the impact of the actual loss of federal student aid (when institutions did not appeal or lost an appeal) and a threatened sanction with no subsequent loss of aid (due to a successful appeal) on enrollment. To the extent that students and/or institutions respond to threatened sanctions (even if the sanction is ultimately overturned on appeal), our estimates will represent the policy relevant treatment effect of federal regulation.

Our data exclude institutions that do not participate in Title IV federal student aid programs.⁹ We therefore cannot distinguish prospective students who forgo higher education in response to a sanction from those that give up their Pell Grant to attend a nonparticipating institution. One might interpret such non-Title IV offerings as potentially inferior, since students cannot obtain public financial aid to attend these institutions and because these programs would be generally unaccredited. To the extent that students leaving sanctioned institutions enroll in these non-Title IV institutions, our estimates will overstate the extent to which market-wide enrollment declines following a sanction.¹⁰

We use counties to proxy for local higher education markets, as in Cellini (2009), and exclude institutions in U.S. territories. We also examine the robustness of our estimates to alternative definitions of local higher education markets, in which we consider an institution’s competitors to be the set of schools within a 15 or 30 mile radius, and obtain similar results (Section III.A). We match institutions with counties using their address and/or zip code, when available in the Pell Grant administrative data or CDR data. For institutions with missing location information in these data sets, we use the Postsecondary Education Participants System (PEPS) to link institutions to counties.¹¹ PEPS also contains information on Title IV institution closures. All institutions that closed after 1982 remain in our sample and are assigned Pell Grant recipient enrollment equal to zero in the

⁸Through a Freedom of Information Act request, we were able to obtain information on institutions that unsuccessfully appealed sanctions related to the 1990 through 1994 repayment cohorts. However, this data does not allow us to distinguish between institutions that successfully appealed their sanction and institutions that never submitted an appeal. Thus, we cannot determine which institutions were ultimately penalized during these years. We observe all appeals and outcomes starting in 1997. Of the sanctions imposed between 1997 and 2000, 70 percent were appealed and 95 percent of appeals were unsuccessful.

⁹Cellini and Goldin (2014) document the large number of these institutions in the for-profit sector in more recent years.

¹⁰Our data cannot detect fraud or other differences in how Pell Grant recipients are reported across sectors. For example, allegations at the time claimed that some for-profit colleges enrolled “ghost students” who never enrolled or immediately dropped out in order to capture Pell Grant program funds (see “Inquiry into student aid,” *New York Times*, July 13, 1995, Section B, page 1). If students who are counted in for-profit institutions are not counted in public or nonprofit institutions, our market-level estimates will represent an upper bound on the magnitude of the enrollment decline from an additional sanctioned for-profit.

¹¹See <http://www2.ed.gov/offices/OSFAP/PEPS/index.html> for details.

closed years to account for enrollment declines due to sanction-driven closures.

C. Trends in Pell Grant Enrollment, Borrowers, and Closures

In the years prior to the implementation of federal regulations that tied CDRs to sanctions (1980–1988), the overall number of Pell Grant recipients was weakly increasing. The share of Pell Grant recipients attending for-profit institutions grew from less than 10 percent to just under 30 percent (Figure 2, Panel A). Over this same period, the share of these students enrolled in public institutions fell from 75 to just under 60 percent. Beginning in 1989, when the first set of institution-level CDRs (corresponding to the 1987 repayment cohort) were released, the for-profit share of Pell Grant recipients fell continuously until 1998, while public institutions enrolled an increasing percentage of recipients. Total Pell Grant recipient enrollment increased continuously until 1993 and remained constant for the next nine years (Appendix Figure C.1).

These patterns are even more pronounced among two-year public and for-profit institutions (Figure 2, Panel B), which experienced the majority of sanctions and/or competitor sanctions. Between 1988 and 1998, the share of Pell Grant recipients enrolled in two-year for-profit institutions fell by 15 percentage points (close to 60 percent), while the share enrolled in public two-year institutions grew by almost an equal magnitude.

The federal sanctions we focus on were explicitly linked to student loan default rates. Figure 3 displays the number of borrowers and corresponding CDRs across sectors beginning with the 1990 repayment cohort (sector-specific CDRs and borrower counts are not available before 1990). Panel A shows the the share of federal borrowers entering repayment by sector and cohort year. Echoing the patterns in Pell Grant recipient enrollment, the distribution of borrowers across sectors shifted during the years when federal sanctions were most prevalent.¹² The share of borrowers entering repayment from public institutions increased while the share leaving for-profit institutions fell. Panel B shows that the CDRs of for-profit colleges dropped precipitously in the years in which sanctions were most frequently imposed, while CDRs remained fairly constant in the public and nonprofit sectors.

Finally, we examine trends in institution closures over this period; additional details are available in Online Appendix B. A large number of for-profit institutions closed their doors beginning in 1989, the first year that institution-specific CDRs were released. Over the next decade, more than 2,000 Title IV for-profit institutions closed (Appendix Figure B.1). To further investigate the correlation between federal sanctions and changes in institution closure rates, we estimate descriptive hazard models. Appendix Figure B.2 displays the correlation between

¹² The total number of borrowers entering repayment was increasing for the 1994 and later cohorts, while the aggregate default rate steadily decreased for cohorts entering repayment between 1990 and 2003 (Appendix Figure C.2).

sanction receipt and the cumulative hazard of closure. These results suggest that sanction receipt in the for-profit sector is correlated with an over 40 percent increase in the likelihood of closure within the next five years.¹³ Sanctioned nonprofit institutions also have an increased hazard of closure, while public institutions appear to be unaffected. These results suggest that the “treatment” of an additional for-profit competitor being sanctioned likely affects institutions through multiple channels, with detrimental impacts on the prices and reputation of competitors that remain open paired with a reduction in the number of competitor institutions.

II. Empirical Framework

We use a generalized difference-in-differences framework to estimate the causal effect of sanctions on enrollment within sanctioned schools and their unsanctioned competitors in the same local higher education market. Our goal is to generate estimates of the impact of a sanction on the distribution of students across institutions and, ultimately, on market-wide enrollment. To do this, we examine enrollment responses to county-level variation in the timing of sanctions, as well as the magnitude of exposure based on the size (i.e., Pell Grant recipient enrollment) of the sanctioned institution. We limit our analysis to estimating the impact of the first sanction(s) in a given market. Thus, each institution is only “treated” (either by receiving a sanction or having a competitor sanctioned) once, but could contribute to the control group for multiple sanction years if it is located in a market in which no sanctions are imposed for at least one year.¹⁴

Two examples help illustrate this variation. In 1991, Prince George’s County, Maryland contained 14 for-profit colleges and a single public community college. That year, one small for-profit with just 57 students was sanctioned. In our model, the sanctioned college—Potomac Academy of Hair Design—is “treated” with sanction receipt. The number of Pell Grant recipients enrolled within this school (57), indicates the magnitude of the exposure for the unsanctioned competitor institutions. In contrast, consider Houston County, Alabama with three schools. When the for-profit Riley College was sanctioned in 1991, the remaining unsanctioned for-profit and community college were “treated” with 3,265 Pell Grant recipients exposed to a for-profit sanction.

We estimate ordinary least squares (OLS) regressions of the following form:

¹³Our measure of closure comes from the PEPS data. The PEPS data allow us to distinguish between closures and mergers, but only contain information on Title IV institutions. Thus, we do not observe closures for institutions that exit the Title IV program prior to closing. To the extent that CDR-related federal regulations induced institutions to leave Title IV, Appendix Figures B.1 and B.2 will underestimate the number of closures and the correlation between sanction receipt and closure.

¹⁴Appendix Table C.2 displays the number of sanctioned and competitor institutions in each sanction year in our “first event” sample. Robustness checks using all sanctions yield similar results (discussed in Section III.A). Further, Lafortune, Rothstein and Schanzenbach (2018) compare estimates obtained from first-event to multiple-event estimates using Monte Carlo simulations in the context of school finance reform and find both approaches yield similar results with minimal bias (see their Online Appendix D).

$$(2) \quad \ln(enrollment + 1)_{jy\tau} = \beta^c (\mathbf{s}_{j\tau} \times post_{y\tau}) + \sum_d \left\{ \gamma^{c,d} \ln(rs_{-j,m\tau}^d + 1) \times post_{y\tau} \right\} + \alpha_{j\tau} + \alpha_y + \alpha_t + t\alpha_c + \epsilon_{jmy\tau}$$

Here, j indexes institutions, y indexes calendar years, τ indexes potential sanction years (hereafter “sanction years”), m indexes local higher education markets, $c \in \{p, np, fp\}$ indicates the sector to which institution j belongs (public, non-profit, and for-profit, respectively), while $d \in \{p, np, fp\}$ indicates the sector of competitor institutions; bold terms represent vectors. $\mathbf{s}_{j\tau}$ indicates whether institution j received a sanction in sanction-year τ , and is interacted with $post_{y\tau}$, an indicator for the year of the sanction and the eight following academic years. Main “own enrollment” effects of sanctions, β^c , vary with sector c .

For unsanctioned institutions, we allow enrollment responses to differ depending on the number of Pell Grant recipients exposed to sanctions in each sector. Given the substantial variation in enrollment across sanctioned schools, an additional institution receiving a sanction would likely have larger effects on unsanctioned competitors’ enrollment in markets where sanctioned schools’ enrollment is larger than average.¹⁵ Thus, we measure the “bite” or magnitude of competitor sanctions with the number of students potentially exposed to a sanction within a given sector and market. Specifically, $rs_{-j,m\tau}^d$ is the number of Pell Grant recipients outside of institution j in market m , sanction year τ , and sector d that were enrolled in an institution the year prior to the sanction year. We use sanctioned institution enrollment in the year before the sanction was announced, as enrollment in the year of the sanction itself will likely endogenously respond to the loss (or threat of loss) of federal aid. We impose the assumption that institutions that are themselves sanctioned do not experience spillovers by setting $rs_{-j,m\tau}^d = 0$ when $s_{j\tau} = 1$. The exposure measure is interacted with $post_{y\tau}$ and effects are allowed to vary by sector c . Since baseline enrollment within unsanctioned competitors also varies substantially, we estimate specifications with $\ln(enrollment + 1)$ as the dependent variable and likewise, $\ln(rs_{-j,m\tau}^d + 1)$ serves as our measure of number of students exposed to sanctions.¹⁶

¹⁵In markets containing both sanctioned for-profits and unsanctioned competitor institutions, the number of for-profit Pell Grant recipients exposed to a sanction ranges from 7 to 7236, with a mean of 790 and median of 267.

¹⁶We use the natural log of enrollment and of Pell Grant recipients exposed to sanctions in our main specification since the number of students affected by a sanction varies substantially across markets and it is unclear how enrollment responses to sanctions might vary along these dimensions. Further, the distributions of both enrollment and number of students exposed to sanctions are rightward-skewed, and binned scatter plots (Chetty, Friedman and Rockoff 2014) show that a linear regression fits better with both transformations. Estimated effects of an additional school sanctioned on the level of Pell Grant enrollment and among just counties with a sanctioned school are discussed in Section III.A. To estimate the effect of an increase in the sanction exposure on the intensive margin (i.e., for counties with any sanctioned schools), we add to equation (2) interactions between $post$ and a set of indicators for whether the market had any Pell Grant recipients exposed to sanctions in each sector – $\sum_d \{ \delta^d \mathbf{1} [rs_{m\tau}^d > 0] \times post_{y\tau} \}$. These additional terms will account for extensive margin effects of sanction receipt and the $\gamma^{c,d}$ co-

Main effects of own and competitor sanctions are accounted for by institution by sanction-year fixed effects ($\alpha_{j\tau}$). These fixed effects also allow us to control for time-invariant market-specific characteristics, such as the overall level of Pell Grant recipient enrollment within the market in the year prior to own/competitor sanction receipt. Fixed effects for the number of years before or after the sanction (α_t , where $t = y - \tau$) account for main effects of $post_{y\tau}$. Calendar year fixed effects (α_y) account for year-specific shocks that affect all institutions similarly (e.g., changes in the maximum Pell Grant or rules dictating Pell Grant eligibility). We include a linear time trend that varies by sector, $t\alpha_c$, to account for changes in sector-specific features over time (e.g., reductions in state funding for community colleges) and $\epsilon_{jmy\tau}$ is a composite error term.

Our main focus in this paper is on identifying enrollment spillovers from sanctioned institutions to their unsanctioned competitors, and our key identifying assumption is similar to the assumption of parallel trends in the standard difference-in-differences setting: that no other factors affecting enrollment in competitors to the sanctioned institution were contemporaneous with the timing of sanction receipt or the number of Pell Grant recipients exposed to a sanction within the local higher education market.¹⁷ This assumption implies similar trends for unsanctioned competitor institutions in the same sector that were in markets that differed only in the number of recipients in a particular sector exposed to sanctions. A testable implication of this assumption is that pre-sanction enrollment trends for unsanctioned public institutions should be similar in markets that had n versus $n + 1$ Pell recipients exposed to sanctions in a given sector.

We test for parallel pre-trends in enrollment and examine impacts over time in the period after a sanction using an event-study approach in which $post_{y\tau}$ in equation (2) is replaced by a set of indicators for years before and after sanction receipt/number of competitors receiving sanctions:

$$(3) \quad \ln(enrollment + 1)_{jy\tau} = \sum_{k=-8}^8 \{\beta_k^e (s_{j\tau} \times \mathbf{1}[t = k]_{\tau})\} + \sum_{k=-8}^8 \sum_d \left\{ \gamma_k^d \left(\ln(rs_{-j,m\tau}^d + 1) \times \mathbf{1}[t = k]_{\tau} \right) \right\} + \alpha_{j\tau} + \alpha_y + \alpha_t + t\alpha_c + \epsilon_{jmy\tau}$$

where $\mathbf{1}[t = k]_{\tau}$ indicates the period k years before or after sanction year τ (with the year prior to the sanction serving as the omitted value). This allows for an explicit test of whether enrollment trends in years prior to the sanction are statistically distinguishable for schools that did and did not receive sanctions and whether pre-period enrollment within unsanctioned institutions that had one

efficients will represent the effect of a marginal increase in exposure to sanctions absent any extensive margin effect.

¹⁷This assumption will be violated if competitors to the (eventually) sanctioned institutions endogenously adjust their recruitment, tuition, and institutional aid practices in anticipation of a competitor being sanctioned or if students change behavior in anticipation of sanction. Darolia (2013) does not find evidence of an enrollment expectations effect when institutions exceeded the three year 25 percent CDR threshold for one or two years.

percent more students exposed to sanctions differed from pre-period trends of unsanctioned schools with one fewer sanctioned competitor. While equation (3) does not allow spillovers to vary by a competitor institution’s own sector, we also estimate models in which we allow for such interactions.

It is unlikely that competitors anticipate and preemptively adjust to sanctions that will be imposed on neighboring institutions in future years. However, in larger markets, schools might be exposed to more sanctioned competitors and it may be the case that overall enrollment trends in larger versus smaller markets differ. We address this concern with our event study and two additional exercises. First, we allow for market-specific time-trends in our main specification and show that our results are robust to removing these trends, allowing these trends to vary between for-profit and other institutions, and by for-profit field of study. Second, we separately examine institutions with above and below median marketwide Pell Grant recipient enrollment in 1990, the year before the first sanctions were imposed. As discussed in the following section, the results of these exercises suggest that our key identifying assumption is not violated.

A. *Sample definition*

We limit our main sample in a few ways. First, our main analyses focus on sub-baccalaureate institutions (institutions offering two-year and less-than-two-year credentials).¹⁸ Few for-profit institutions offered baccalaureate degrees during the period we study, and even fewer baccalaureate-granting for-profits received sanctions.¹⁹ Nonetheless, we show that our estimates are robust to the inclusion of four-year institutions in Section III.A. We further restrict our analysis to the effects of sanctions imposed between 1991 and 2000 (during the time period including the vast majority of sanctions), focusing on changes in enrollment in the eight years before and after sanction receipt. Third, our main sample excludes counties that contain more than 50 two-year institutions (on average, in a given year between 1982 and 2008) as we are unlikely to be able to detect enrollment spillovers from an additional for-profit sanction in these 12 large markets.²⁰ Our estimates are robust to the inclusion of all counties in our analysis sample. Online Appendix A provides a detailed description of our data sources and main analysis sample.

¹⁸It is not always possible to distinguish two-year from less-than-two-year institutions in the Pell Grant and CDR administrative data.

¹⁹Only 4 baccalaureate-granting public institutions, 23 baccalaureate-granting nonprofits, and 10 baccalaureate-granting for-profits were sanctioned. In the year prior to the release of the first set of CDR sanctions (1990), only 6 percent of for-profit colleges offered four-year degrees.

²⁰These counties include: Maricopa County (AZ), Los Angeles County (CA), Orange County (CA), San Diego County (CA), Miami-Dade County (FL), Cook County (IL), Wayne County (MI), New York (NY), Allegheny County (PA), Philadelphia County (PA), Dallas County (TX), and Harris County (TX). The remaining counties include institutions that received 75 percent of all sanctions imposed on two-year institutions.

B. Characteristics of institutions by sector and sanction receipt

Table 1 displays the characteristics of the institutions form the basis of our analytic sample. Beginning in 1991, a given institution could receive up to two CDR-related sanctions (resulting in either loss of loans and/or loss of all Title IV aid) on an annual basis. Panel A contains information on all school by sanction-year observations within our sample, regardless of own or competitor sanction receipt. Of all Pell Grant recipients enrolled in sub-baccalaureate schools, 80 percent attended public institutions, 3 percent attended a nonprofit schools, and the remaining 18 percent were enrolled in the for-profit sector (Panel A). In contrast, among students enrolled in sanctioned institutions, only 19 percent attended public community colleges, while 80 percent were enrolled in a for-profit school (Panel B). Very few nonprofit institutions received sanctions, and these schools contained only 1 percent of Pell Grant recipients exposed to sanctions. The distribution of Pell Grant recipients across sectors for unsanctioned schools with at least one sanctioned local for-profit competitor was similar to the distribution of overall enrollment, with 62 percent of students attending community colleges and 36 percent in for-profit schools (Panels C through E).

III. The Impact of Sanctions on Enrollment

We begin by exploring the dynamics of sanction-driven enrollment spillovers by estimating the event-study models according to equation (3). We first examine the effects of sanctions applied to the for-profit sector, as these account for the vast majority of sanctions imposed during the period we examine. Figure 4 plots the natural log of enrollment in the eight years before and after sanction imposition. In Panel A, which displays enrollment effects for sanctioned institutions, each solid marker represents the estimated change in log enrollment by years since the sanction was imposed, with the year immediately before sanction receipt serving as the omitted category. In Panel B, each marker represents the estimated effect of a one percent increase in the number of for-profit students exposed to a sanction on enrollment in all unsanctioned competitors pooled across sectors. We also estimate a more flexible version of equation 3 in which we allow the spillover effects from for-profit sanctions to vary between unsanctioned for-profit competitors (Panel C), and other unsanctioned (public and nonprofit) institutions (Panel D). Dark dashed lines indicate 95 percent confidence intervals.

In the years before a for-profit is sanctioned, enrollment growth significantly exceeds that of other institutions, suggesting that fast-growing for-profit schools were especially likely to have poor student loan repayment outcomes (Panel A). However, more important for our identification of spillover effects, we observe no significant differences in enrollment in the pre-sanction time period for unsanctioned competitors of for-profit institutions in markets that will eventually have greater for-profit sanction exposure (Panel B). Likewise, when we disaggregate

competitor institutions by sector (Panels C and D), we find no significant point estimates in any of the pre-sanction years.

Nonetheless, in Panels C and D, there is a visual trend break in the series around $t = -4$. To explicitly test whether the enrollment trend in the four years before competitor sanction(s) are the same as the trend in the years following competitor sanction(s), we modify the event study framework in equation (3). Let $T_{1\tau} = \mathbf{1}[t \in [-8, -5]]_{\tau}$, $T_{2\tau} = \mathbf{1}[t \in [-4, -1]]_{\tau}$, $T_{3\tau} = \mathbf{1}[t \in [0, 4]]_{\tau}$, $T_{4\tau} = \mathbf{1}[t \in [5, 8]]_{\tau}$, and \mathbf{T}_{τ} represent is a vector containing all four period indicators. We estimate models of the following form:

$$(4) \quad \ln(enrollment + 1)_{jy\tau} = \theta^c t (\mathbf{T}_{\tau} \mathbf{s}_{j\tau}) + \lambda^{pub} t \left(\mathbf{T}_{\tau} \ln(rs_{-j,m\tau}^{pub} + 1) \right) \\ + \lambda^{np} t \left(\mathbf{T}_{\tau} \ln(rs_{-j,m\tau}^{np} + 1) \right) + \lambda^{c,fp} t \left(\mathbf{T}_{\tau} \ln(rs_{-j,m\tau}^{fp} + 1) \right) + \delta_{j\tau} + \delta_y + \delta_t + t\delta\alpha_c + \nu_{jmy\tau}$$

We first test whether we can reject the hypothesis of equal trends in enrollment in the immediate pre- and post-sanction periods (i.e., $\lambda_2^{c,fp} = \lambda_3^{c,fp}$). Among unsanctioned competitors of sanctioned for-profit schools, we can reject the hypothesis that trends are equal with $p = 0.039$ and $p = 0.005$ for other competitors and for-profit competitors, respectively (see Appendix Table C.3 for point estimates and p -values).

Even though post-sanction trends in competitor enrollment are significantly larger in magnitude than pre-sanction trends, we include a set of more conservative “trend-adjusted” point estimates in Figure 4. Specifically, we jointly estimate equations (3) and (4) and subtract the estimated pre-sanction trend from post-sanction point estimates.²¹ Standard errors and 95 percent confidence intervals are constructed using the delta method and graphed along side our main estimates in Figure 4. Trend-adjusted estimates follow the same patterns as our main estimates, but spillovers are generally smaller in magnitude and less precise.

In the years after a for-profit college is sanctioned, we observe a sizable drop in own enrollment (Panel A). Pooling competitor institutions, we find evidence of small negative enrollment effects that are statistically significant at the five percent level beginning two years after a sanction (Panel B). Results are similar but imprecise when trend-adjusted.

Splitting out for-profit competitors from competitors in other sectors, Panel C shows a significant decrease in enrollment in every post-sanction year. Point estimates suggest that a one percent increase in the number of students exposed to a for-profit sanction leads to a 0.02 to 0.06 percent decrease in unsanctioned

²¹In practice, this requires a “recentering” of equation (4) to ensure that the omitted time period ($t = 0$ without recentering) is the same as the omitted period in equation (3) ($t = -1$). This is achieved by adding 1 to t in equation (4). As examples, $\hat{\gamma}_0^{fp,fp} - \hat{\lambda}_2^{fp,fp}$ provides an estimate of the trend-adjusted effect of for-profit sanction exposure on the enrollment of unsanctioned for-profit competitors in the year of the sanction, $\hat{\gamma}_1^{fp,fp} - (2\hat{\lambda}_2^{fp,fp})$ gives the trend-adjusted estimated effect one year after the sanction, and $\hat{\gamma}_2^{fp,fp} - (3\hat{\lambda}_2^{fp,fp})$ gives the trend-adjusted estimated effect two years after the sanction. Likewise, $\hat{\beta}_0^{fp} - \hat{\theta}_2^{fp}$ provides an estimate of the trend-adjusted effect of for-profit sanction receipt on sanctioned schools’ own enrollment.

for-profit competitor enrollment in each of the subsequent eight years. These results remain significant at the 95 percent level in the trend-adjusted model through year seven. In contrast, unsanctioned public and nonprofit competitors experience significant gains in enrollment in each year following sanction receipt (Panel D). These effects occur immediately and grow over time, rising to about 0.05 percent after eight years. Trend-adjusted estimates are positive but smaller in magnitude and less precise.

Figure 5 displays similar event study analyses of enrollment effects in response to sanctions imposed on public and nonprofit colleges. In contrast to the enrollment spillovers we find in response to for-profit sanctions, when public and nonprofit institutions are sanctioned, effects on competitor enrollment (Panels C and D) are imprecise and largely statistically insignificant. Because of the relatively small number of sanctions applied in these sectors, evidence of a negative trend in enrollment in the years before the sanction was imposed among sanctioned public institutions (Panel A), and the relative imprecision of these estimates, we focus our discussion on the effects of for-profit sanctions for the remainder of the paper.

Table 2 reports the estimates from equation (2) in which the eight post-sanction years are pooled. The first row of coefficients in Panel A represent own enrollment effects for sanctioned institutions (similar to Panel A of Figure 4 for for-profits, and Panels A and B of Figure 5 for public and nonprofit institutions, respectively). We observe large, statistically significant decreases in own enrollment for sanctioned for-profit institutions (-1.32 log points or 73 percent).

Own-enrollment effects of sanctions do not take into account enrollment spillovers from sanctioned institutions to their unsanctioned competitors, and such effects are critically important to assessing the overall impact of federal regulation on students' access to higher education. The second row of Panel A presents our pooled estimates of enrollment spillovers to competitor institutions. We find virtually no effect of for-profit sanctions on enrollment when competitors and post-sanction years are pooled.

However, these pooled estimates mask sizable enrollment spillovers from for-profit sanctions that vary by competitor sector. To assess these heterogeneous effects, we allow effects on enrollment to vary by both the sector of the sanctioned institution(s) and the sector of the competitor. These estimates are shown in the second through fourth rows of Panel B of Table 2. We find no statistically significant enrollment spillovers of enrollment on any competitor institutions when either a public (column 2) or nonprofit (column 3) is sanctioned. In contrast, when a for-profit is sanctioned (column 3), enrollment significantly increases for local unsanctioned public and nonprofit competitors, which is consistent with the event study results shown in Panel D of Figure 4. The estimates imply that a one percent increase in the number of Pell Grant recipients exposed to a for-profit sanction yields roughly a 0.07 percent (0.064 log point) increase in enrollment in the local public institution and a 0.04 percent (0.039 log point) increase in enrollment in local nonprofits. We see the opposite effect among for-profit competitors.

When a for-profit is sanctioned, other for-profits experience negative spillovers of roughly 0.015 percent (-0.015 log point). We explore potential explanations for these pattern in Section III.B.²²

To understand the overall impact on enrollment in the market, we translate these effects into the numbers of students shifting sectors when an additional public, nonprofit, or for-profit institution is sanctioned. To measure the change in number of enrolled students per sanctioned institution, we multiply exponentiated point estimates in Panel B of Table 2 by average enrollment in the year before the sanction (row 1, Table 3). To quantify the magnitude of enrollment spillovers due an additional sanction, we multiply exponentiated point estimates for effects on competitor enrollment by baseline enrollment for each type of institution (Table 3, rows 2 through 4) and the number of each type of institution in the average market with at least one sanctioned institution in the specified sector (rows 9 through 11, Table 3). Market wide enrollment is obtained by summing over the above quantities and standard errors are constructed using the delta method.

Estimated effects of an additional public or nonprofit college sanction on overall market enrollment are small and statistically insignificant (bottom rows of Table 3). In contrast, an additional for-profit sanction leads to an annual decrease in marketwide enrollment of 96 students, a 2.2 percent decline for the average market with for-profit sanctions. The average sanctioned for-profit college loses about 226 Pell Grant recipients following sanction receipt - approximately 68 percent of baseline Pell Grant enrollment. In response to the sanction, local public institutions experience a gain of about 55 students each, or 154 students across all public institutions in the market (Panel D). A small number of students are also absorbed by local nonprofits. In total, 158 - about 70 percent - of students displaced from the sanctioned for-profit institution are absorbed by the public and nonprofit sectors. At the same time, 28 students exit unsanctioned for-profits marketwide. When these negative enrollment spillovers are taken into account, the public sector absorbs 60 percent of the overall enrollment decline due to an additional for-profit sanction. We further explore the nature of these enrollment spillovers in Section III.B. Together, these spillovers result in a significant, but small decline in overall market enrollment indicating that most displaced for-profit students find substitute programs in other sectors.

A. *Robustness*

In this section, we show that our main estimates are robust to a variety of specifications and sample definitions. We report the results of these tests in Online Appendix C.

We first test the robustness of our results to the inclusion of all sanction-events in a market, rather than only the first sanction. These results are shown

²²Estimates from the trend-adjusted model reveal slightly smaller spillovers of 0.037 log point for public competitors and -0.013 log point for for-profit competitors (Appendix Table C.4).

in Appendix Table C.5. Effects are slightly smaller in magnitude than in our main specification, but the patterns of heterogeneous effects by competitor sector are consistent with those shown in Table 2. Unsanctioned public and nonprofit competitor institutions see significant increases in enrollment and unsanctioned for-profit competitors see slight declines when additional for-profit Pell Grant recipients are exposed to sanctions. Putting these effects together to consider marketwide enrollment changes from an additional sanctioned for-profit school, the increase in public and nonprofit enrollment is enough to offset the negative enrollment effects in the for-profit sector and we find no statistically significant effects on overall market enrollment.

We next consider an alternative specification in which we examine effects of the number of schools (rather than number of students) exposed to sanctions on enrollment (rather than log enrollment).²³ As shown in Appendix Table C.6, estimated impacts are consistent with our main results. with offsetting positive enrollment impacts in public and nonprofits and a slight decline in competitor for-profits when a for-profit is sanctioned. An additional for-profit sanction decreases marketwide enrollment by a statistically insignificant 30 students per year.

In our next robustness test, we estimate the effect of exposure to competitor sanctions using only intensive margin variation. Specifically, we include in our main specification an indicator for whether a given market contains any sanctioned schools in a given sector, interacted with an indicator for post sanction receipt. With this specification, estimates of enrollment spillovers from sanctioned schools to unsanctioned competitors will only be identified from variation in the magnitude of exposure within counties that contain sanctioned schools. Results – shown in Appendix Table C.7 – are similar albeit less precise when it comes to the effects of sanction receipt on own enrollment.²⁴ Spillovers from sanctioned for-profits to unsanctioned public and nonprofit competitors are approximately twice as large in magnitude but are not statistically distinguishable from estimates from our main specification. Estimated spillovers to unsanctioned for-profit competitors of sanctioned for-profits are small, positive, and insignificant when using only intensive margin variation, but are also not statistically distinguishable from our main results.

Our main estimates are also robust to the inclusion of the largest counties with more than 50 two-year institutions, where we might expect a weaker reaction of competitors to sanctioned institutions (Appendix Table C.8). Relative to our main estimates, we find similar positive enrollment spillovers to public competitors of sanctioned for-profits, slightly larger positive spillovers to nonprofit competitors, and slightly larger negative spillovers to for-profit competitors. In

²³Specifically, we estimate: $enrollment_{jy\tau} = \beta^c (s_{j\tau} \times post_{y\tau}) + \gamma^{c,d} (s_{-j,m\tau}^d \times post_{y\tau}) + \alpha_{j\tau} + \alpha_y + \alpha_t + t\alpha_c + \epsilon_{jmy\tau}$, where $s_{-j,m\tau}^d$ is the number of institution j 's sector d competitors sanctioned in sanction-year τ .

²⁴Event study estimates of the effects of for-profit sanctions on own and competitor enrollment using only intensive margin variation are shown in Appendix Figure C.3.

aggregate, county-wide enrollment falls by a marginally significant ($p < 0.10$) 102 Pell recipients in a given year, representing a 1 percent decline.

In our next robustness test, we include four-year institutions in our sample. Many four-year public and nonprofit institutions draw students from outside of the local market. and thus, we would not necessarily expect to see a response to changes in local demand. Sanctions almost exclusively targeted two-year institutions and there were very few for-profits classified as four-year institutions in the 1990s. As shown in Appendix Table C.9, we obtain similar results when we include this broader group of institutions in our sample and estimate that each additional for-profit sanction leads to a statistically insignificant 22 student per year (0.3 percent) drop in county-wide enrollment.

An additional concern might be that differences in employment trends between similar sized counties with more versus fewer sanctioned schools are contributing to our results. To give an example, if unemployment is increasing in particular industries that disproportionately employ former for-profit students, for-profit borrowers might have trouble repaying their loans and enrollment within for-profits might respond to the lack of employment opportunities related to the programs these schools offer. Appendix Table C.10 shows that our results are robust to the inclusion of county by sector and county by for-profit industry linear time trends. The first column presents estimated own enrollment effects and enrollment spillovers from an additional sanctioned competitor from a specification that excludes county-specific linear trends, while column 2 duplicates our main estimates from Table 2 that include county-specific trends. Column 3 includes estimates from specifications that allow county-linear trends to vary between for-profit and other institutions and column 4 allows county-specific for-profit trends to vary by broad for-profit industry categories (described in Appendix A). Estimates are similar regardless of whether the county time trends are allowed to vary for for-profits or by for-profit industry. Estimated market level enrollment declines from an additional for-profit sanction are smaller without county specific trends – the opposite of what we would expect to find if counties that had more for-profit sanctions were also experiencing rising unemployment.

We next show that our main estimates are robust to alternative definitions of an institution’s local higher education market. Although counties have been used to proxy for local higher education markets in many previous papers (e.g., Cellini 2009; Cellini 2010), it is not necessarily the case that current and prospective students would not search for higher education alternatives across county lines. Thus, we geocode the locations of institutions in our sample and consider other institutions to be local competitors if they fall within a 15 or 30 mile radius.²⁵ Appendix Table C.11 shows that the estimates produced using the alternative market definitions are very similar to our main results, suggesting that using counties to proxy for local higher education markets is a reasonable practice.

We test for heterogeneity by market size by dividing our main sample by mar-

²⁵A small number of institutions could not be geocoded and thus are excluded.

ketwide enrollment in 1990, the year before the first sanctions were imposed. Estimated effects of for-profit sanctions on own and competitor enrollment levels are qualitatively similar across these groups, and the overall reduction in market enrollment due to an additional for-profit sanction remains at 2 percent in both sets of counties (Appendix Table C.12). These results provide evidence that the spillover effects estimated using our main sample cannot be explained by differences in the size of markets with more versus fewer sanctioned schools.

The estimates we have presented thus far combine the effects of sanctions that cause schools to lose access to student loans with sanctions that may cause an institution to lose of all Title IV aid. We might expect stronger effects on enrollment (particularly Pell Grant enrollment) for institutions threatened with the loss of all Title IV aid, as grant aid directly reduces the net cost of college. Separate estimates by type of sanction are shown in Appendix Table C.13. Effects of for-profit sanctions on own enrollment are significantly larger in magnitude for sanctions that result in loss of student loans than for sanctions that lead to potential loss of all Title IV aid ($p = 0.003$). Positive enrollment spillovers from the threatened loss of Title IV and loss of only loans are similar in magnitude for public and for-profit competitors. Negative spillovers to unsanctioned for-profit competitors are significantly larger for sanctions that lead to loss of all federal student aid ($p = 0.099$), suggesting that the loss of Title IV generates stronger reputational impacts across sectors than simply the loss of access to loans.

Finally, we explore heterogeneity by sanction-year; results are shown in Appendix Table C.14. The largest decrease in own-enrollment was due to sanctions imposed in in 1991. One interpretation of this finding is that the for-profit institutions that were most affected by a sanction closed or left Title IV, leaving a (relatively) positively selected sample of for-profits that could potentially be sanctioned in subsequent years. The temporal patterns of positive spillovers to public institutions and negative spillovers to other for-profits is loosely consistent with an reputational effects: impacts appear strongest in the first several years of the policy (1991-1993) as potential students learn more about for-profit colleges, and may weaken when many sanctioned and unsanctioned for-profit schools have closed or left the Title IV program, and (presumably) potential students already gained more information about the sector as a whole.

B. Mechanisms

As discussed in Section I.A, when a local for-profit college is sanctioned and its students are exposed to a loss of federal student aid, we would expect to see weakly positive enrollment changes in other institutions and sectors, as these schools have become relatively cheaper. Thus, without additional channels through which sanctions affect competitor enrollment, we would not expect a reduction in enrollment within unsanctioned for-profit institutions. One explanation is that — not unlike today — the reputation of the sector was tarnished when individual institutions were sanctioned. To explore this hypothesis, we conducted an archival

analysis of newspaper records.²⁶ We found several national news stories on cohort default rates and the problems of student loan repayment in “private trade schools.” Perhaps more relevant to our study were numerous stories in local newspapers reporting on sanctions and the closure of local institutions. We present select quotations in Appendix D. These quotes suggest that when one school gets sanctioned, other private trade schools may suffer; for example, after a local competitor shut down in 1991, a for-profit college founder suggested that the image of the local trade school industry “has taken a pretty heavy beating. Any damage to any private career school affects every other school” (Morgan 1991).

Our archival analysis also suggested that spillovers might be more likely among branches of the same chain of institutions. To assess this empirically, we code chains by matching names of colleges in more than one location.²⁷ Our match is necessarily imprecise, but we are able to identify at least a subset of chains with multiple locations in the same county. We would be most concerned that our negative within-county spillovers are driven by two locations of the same chain in a given county, but just 6 percent of all for-profits had multiple locations in same county during the 1990s. Of the 9,688 unique for-profit schools located in markets within which at least one for-profit competitor was sanctioned, we only find 137 (1 percent) that appear to belong to the same chain as the sanctioned school. Thus, it is unlikely that preemptive actions taken by unsanctioned locations within a chain that also contains sanctioned institutions in the same county can explain the negative spillovers.

Nonetheless, we test for within-chain spillovers; estimates in Table 4 come from a specification that allows spillover effects of additional students exposed to sanctions to vary by whether the sanctioned competitor belonged to the same chain. We find weak evidence that spillovers from sanctioned for-profits to other branches in in the same chain are larger than spillovers to non-chain (or different chain) for-profits. The estimated spillover from a marginal increase in the number of Pell recipients exposed to a sanction within an institution in the same chain (-0.112 log points) is larger in magnitude than the estimated enrollment spillover from other sanctioned for profits (-0.013 log points), but the difference between these estimates is not significant at conventional levels ($p = 0.136$). While this result is suggestive of stronger negative spillovers to branches of chain institutions, reputational effects within chains are unlikely to drive our results due to the small number of chain institutions in our sample. Once we take into account the size and composition of markets which contained unsanctioned and sanctioned

²⁶We ran queries in Lexis-Nexis using the names of the 20 largest sanctioned for-profit colleges as well as 10 other randomly selected smaller sanctioned for-profits.

²⁷Unfortunately, given idiosyncratic reporting of chains, it is difficult to identify branches in our data. For example, some colleges may have separate Office of Postsecondary Education Identification (OPEID) numbers for each branch campus, while others have only one OPEID for all branches. In the process of identifying chains, institutions with common/generic names (e.g., American Business Institute, American Career College) were only classified as belonging to the same chain if all locations were located in the same state or geographic region or if independent confirmation (e.g., from newspaper articles or other reports) was obtained. Appendix A provides further details.

for-profits in the same chain and other markets that contained sanctioned and unsanctioned for-profits, the estimated change in marketwide enrollment is very similar (93 fewer students per year versus 83 fewer students per year).

Institutions need not be part of the same chain to incur negative reputational effects from competitor sanctions. In fact, reputational effects may be stronger among institutions that offer programs in fields or industries similar to those offered by sanctioned institutions. For example, an article describing a local sanctioned beauty school noted, “[h]air-dressing schools are also prevalent on state and national lists,” perhaps leading students away from other for-profit cosmetology schools in the same market. To assess spillovers by industry, we group for-profit colleges into five broad “field” categories using keywords appearing in college names, with the remainder whose names do not indicate a particular field depicted as “General” for-profits (see Online Appendix A).²⁸

We first estimate a modified version of our event study specification (equation (3)) in which we allow the main and spillover effects from for-profit sanctions to vary by industry. For unsanctioned for-profit competitors, we ask whether the effects of a sanction on a local school in the same field has a stronger impact than a sanction imposed on a school in a different field. Point estimates suggest similar effects of sanctions on own enrollment within fields, with one exception: among beauty schools, we find evidence of a negative pre-trend in log enrollment starting four years before sanction receipt (Appendix Figure C.4). We therefore treat beauty schools separately in our subsequent models. To improve precision, we also pool together for-profit competitors across all fields (except beauty) and allow spillover effects to vary by whether the sanctioned for-profit is in the same or a different industry as the unsanctioned for-profit competitor. Figure 6 displays point estimates and 95 percent confidence intervals from these analyses. Panel A shows a significant decline in enrollment in for-profit competitor institutions belonging to the sanctioned for-profit institution’s same industry in the years after a sanction. In contrast, Panel B shows a more modest impact of sanctions on competitors in different fields.²⁹

Table 5 explores within-industry spillover effects in more detail. Column (1) shows results from a specification that pools all industries and tests for differences in reputational effects of for-profits in the same fields. We find significantly larger

²⁸For-profit institutions vary substantially in size across field of study. For example, in 1990, schools classified as offering beauty programs enrolled 46 students on average, while general for-profits had 284 Pell grant recipients. Appendix Table A.1 shows that total number of unique for-profit institutions and average enrollment by field of study, the number and average enrollment of sanctioned for-profit institutions by field, and the number and average enrollment of unsanctioned for-profit competitors of different-field sanctioned for profits and same-field sanctioned for-profits, by field. The final two columns display characteristics of public competitors of sanctioned for-profits, by sanctioned for-profit industry.

²⁹Appendix Figure C.5 displays estimated spillover effects from for-profit sanctions in the same and in different industries for the unsanctioned beauty school competitors that are excluded from Figure 6. Estimates are relatively imprecise, but there appears to be an enrollment pre-trend beginning four years before competitor sanction receipt that corresponds to the pre-trend shown in Appendix Figure C.4. Reassuringly, our main estimates remain quite similar to those shown in Table 2 when we exclude beauty schools and beauty school-related sanction exposure (Appendix Table C.15).

($p = 0.026$) spillovers for institutions offering programs in the same field (-0.041 log points) compared to institutions in other fields (0.007 log points). Next, corresponding to our event study approach, we allow for separate effects from beauty school and non-beauty school sanctions in column (2). Estimated own enrollment and same-industry spillovers from sanctioned non-beauty for-profits are larger in magnitude, albeit less precise. Cross-industry spillovers from for-profit sanctions remain positive and statistically insignificant for both beauty schools and other for-profits.

The third set of estimates in Table 5 column (3), allow for own-enrollment and spillover effects of sanctions to vary across for-profit industries. Own enrollment effects vary significantly across fields ($p = 0.001$), with the largest own enrollment declines occurring within sanctioned for-profits focusing on computing and business (-2.205 log points) and within general for-profits (-1.433 log points). Own enrollment effects for sanctioned beauty schools are similar to estimates from the second specification (-0.859 log points). For-profits with a mechanics/engineer focus experience marginally significant enrollment declines following sanction receipt (-0.924 log points, $p < 0.1$), while enrollment losses in sanctioned health and culinary/arts focused for-profits are not statistically distinguishable from zero.

Spillovers from for-profit sanctions to unsanctioned public competitors also vary significantly across fields ($p = 0.001$). Sanctions applied to general for-profits generate significant positive enrollment spillovers to public institutions, which typically offer a range of fields. More surprisingly, health-focused for-profit sanctions appear to result in negative public sector spillovers. Finally, although estimated nonprofit sector enrollment gains from a marginal increase in the number of for-profit students exposed to sanctions are jointly significant ($p = 0.008$), we cannot reject a test of equality of effects by for-profit industry ($p = 0.239$).

Supporting the hypothesis of stronger reputational effects within-field, spillovers from sanctioned for-profits to unsanctioned for-profit competitors offering similar programs are negative and larger in magnitude than spillovers from sanctioned for-profits in other fields. The largest significant field-specific spillovers are for computer/business and beauty-related fields. A one percent increase in the number of Pell Grant recipients exposed to sanctions within these sectors leads to 0.111 and 0.035 log-point enrollment declines within other local computer/business and beauty schools, respectively. None of the estimated enrollment spillovers to unsanctioned for-profit competitors in different fields are statistically significant and we cannot reject a test that the coefficients are jointly insignificant ($p = 0.922$). However, effects of for-profit sanctions on unsanctioned for-profit competitors in the same field are also jointly insignificant ($p = 0.181$).

Although not definitive, the direction and magnitudes of these results suggest that reputational effects within fields may be at play, at least for some fields. More generally, the negative spillovers to competitor for-profits suggest that sanctions may have improved student information about the quality and costs of colleges in this sector, leading students to make more informed choices. While we cannot

assess this hypothesis directly, we provide suggestive evidence by examining effects on student borrowing and defaults.

IV. Descriptive Evidence on Borrowing and Defaults

Thus far, we have shown that when for-profit institutions are threatened with federal sanctions, their own enrollment falls, enrollment in competitor for-profit institutions likewise decreases, and public institutions absorb most of these students. However, it remains unclear whether this reallocation of students across sectors in response to for-profit sanctions represents a gain in private or social welfare. Ideally, we would compare attainment and earnings outcomes of students affected by sanctions to their outcomes in the absence of sanctions. Given data limitations, we can only proxy for student outcomes by examining changes in the number of borrowers and defaults across sectors in response to sanctions. To do so, we generate estimates from modified versions of equations (2) and (3). First, we use the natural log of the number of borrowers and defaulters as dependent variables. Unfortunately, since we first observe borrowers and defaulters beginning with the 1990 cohort (i.e., students who entered repayment in 1990 and who could have defaulted on their loans by 1992), we only observe pre-sanction borrowing and defaults for one year prior to sanction receipt for every institution, and thus limit the pre-period to $t - 1$. Because this restricts our ability to account for pre-trends in pre-sanction borrowing outcomes, we consider these analyses to be descriptive.

Figure 7, which displays point estimates and 95 percent confidence intervals from equation (3), shows that borrowing and defaults attributable to students who attended sanctioned for-profits decline dramatically in the years after sanction receipt (Panel A). Borrowing and default in competitor for-profits do not appear to change in first five years after a competitor is sanctioned (Panel B). In contrast, we find evidence of increases in both borrowers and defaults within public and nonprofit competitors of sanctioned for-profits (Panel C), outcomes which might be expected given the substantial enrollment gains in the public sector after for-profit competitor sanction. However, as we discuss below, the increased public-sector borrowing and defaults are small in magnitude relative to the declines in borrowing and default in sanctioned institutions.

We calculate the implied declines in marketwide borrowing and default in Table 6. As in Table 3, we exponentiate the point estimates from a modified version of our main specification (results shown in Appendix Table C.16) and multiply by the baseline number of borrowers or defaulters, sanctioned competitor borrowers or defaults, and the number of unsanctioned institutions in each sector. When a for-profit college is sanctioned, the number of borrowers in a market declines by about 59 students (3 percent of the market baseline). Similarly, the number of students defaulting on loans declines by 30 students (5 percent of the market baseline). Taken together, these estimates suggest that around half of all students who would have borrowed in the absence of a for-profit sanction also would have

defaulted on their loans. Importantly, in percentage terms, both borrowing and defaults appear to decline by more than the fall in Pell Grant recipient enrollment (2 percent; Table 3). This disproportionate response is consistent with findings of lower levels of borrowing and default in public institutions relative to for-profits and might therefore be expected as students switch to the public sector in response to a sanction.

V. Conclusions

In recent years, expansive growth followed by increased scrutiny of the for-profit sector has led to the closure of several large for-profit college chains and has stimulated debates over new regulations that may further restrict federal student aid at many other institutions in this sector. To shed light on how these changes might affect aggregate college enrollment and the distribution of students across sectors, this study draws on historical data from a time when policymakers implemented similarly restrictive regulations. We use these cohort default rate regulations with a generalized difference-in-differences design to assess whether and how student enrollment shifts within and across sectors when (primarily) for-profit institutions lose eligibility for federal student aid due to federal sanctions.

Overall, our results suggest have important implications for the sub-baccalaureate market. First, regulations restricting financial aid availability affected student enrollment. We find that Pell Grant recipient enrollment fell in for-profit institutions that were threatened with the loss of federal aid. Our results reveal larger own-enrollment effects than previous research (Darolia 2013), likely because the enrollment of vulnerable students — recipients of the means-tested Pell Grant, studied here — is more strongly affected by federal aid loss than total enrollment. Second, it appears that most students who would have attended a for-profit institution in the absence of a sanction could and did find programs to fit their needs in the public sector. Similar to the findings of Cellini (2009) and Goodman and Henriques (forthcoming), who use more recent data, our findings are consistent with strong competition for students across sectors at the sub-baccalaureate college level. Capacity constraints at lower-cost competitor public institutions did not appear to be a concern in the time period and context that we study, as public institutions absorbed most students who exited for-profit institutions in response to federal sanctions. The majority of students who would have enrolled in a sanctioned for-profit institutions — about 70 percent — ultimately enrolled in the public and nonprofit sectors. Third, further extending the literature, our results reveal that when a for-profit college was sanctioned, enrollment in other local competitor for-profit colleges also declined. Archival news analysis and analysis by field suggests that — much like today — the whole sector suffers the reputational impacts of federal sanctions placed on individual institutions. Altogether, marketwide enrollment declined by about two percent. Finally, we find evidence suggesting that student loan outcomes improved after a poorly performing for-profit college was sanctioned, as the number of borrowers and student loan

defaults disproportionately declined in the market.

Although we study an earlier time period, our results can inform current debates surrounding accountability in higher education and restrictions on access to federal student aid for low-performing for-profit colleges. The climate of rapid for-profit college growth, questionable practices in this sector, and subsequent regulations in the late 1980s and early 1990s bears a strong resemblance to the current U.S. higher education landscape. After the growth in the for-profit sector during the first decade of the 2000s, renewed concern over student outcomes led to the 2014 Gainful Employment (GE) regulations. Under GE, eligibility for federal student aid would be based on graduates' loan payment-to-earnings ratios, calculated at the program level.³⁰ The most recent estimates based on informational loan-payment-to-earnings ratios suggest that roughly 190,000 students are in programs that would face sanctions because they failed GE thresholds (based on the 2015-2016 school year; see Federal Register Vol. 83, No. 157). Under the Trump administration, implementation of the rule was delayed and a full repeal is currently being considered. While proponents argue that the GE standards will protect vulnerable students from profit-seeking firms that do not prioritize students' interests, opponents argue that the rules will limit underserved students' access to higher education (Fain 2014*b*; Gleason and Mitchell 2014; Mitchell and Zibel 2014; Fain 2018).

To better assess how the loss of federal aid for for-profit colleges under GE might affect enrollment and outcomes, we extrapolate our estimates to the current environment. Of course, there are many important differences in the higher education market of today relative to the 1990s – including the prevalence of online education options and chain institutions, declining public support for community colleges, and concerns over capacity constraints in some states – that may affect the generalizability of our estimates to the effects of regulations in the present. We also acknowledge that, unlike CDRs, Gainful Employment sanctions apply only to programs within an institution, rather than the entire institution. Nonetheless, if we assume that 190,000 students will be impacted by GE, applying our estimates suggest that about 129,000 would exit sanctioned colleges and another 16,000 would choose to avoid their unsanctioned for-profit competitors due to reputational effects. Approximately 90,000 of these students would shift to public and nonprofit institutions, while about 55,000 students would exit higher education altogether – a reduction in current undergraduate enrollment of less than one half of one percent. The roughly 45,000 students who remain in these schools and programs would have to fund their programs with sources other than

³⁰Specifically, payment-to-earnings ratios are categorized as pass (average loan payments are less than 8 percent of total or 20 percent of discretionary earnings), zone (average loan payments are 8–12 percent of total or 20–30 percent of discretionary earnings), or fail (average loan payments are greater than 12 percent of total or greater than 30 percent of discretionary earnings). Programs become ineligible to disburse Title IV funds if they fail this measure in two out of any three consecutive years or are in the zone for four consecutive years. See <https://www.ed.gov/news/press-releases/education-department-releases-final-debt-earnings-rates-gainful-employment-programs>.

federal aid.

Counteracting enrollment losses, the loan disbursement limitations and shifted enrollment brought about by GE regulations would likely improve federal student loan outcomes. Our estimated effects of federal sanctions on borrowing and defaults are difficult to apply to the current context without a number of additional assumptions, but they suggest declines in borrowing and default among students who forgo higher education and those shift to other sectors. The schools and programs most likely to be penalized are those where students also are most likely to accrue debt that they will not be able to repay. Further, though some students who would have attended for-profit colleges will still borrow and default when shifting to the public sector, our estimates also suggest that their rate of borrowing and default will decline. It is also possible that some students who would forgo higher education might be better off than if they had attended a sanctioned school: evidence on student outcomes in the two-year for-profit sector are suggestive of low earnings gains that may not be enough to offset debt, even for the average student (e.g., Cellini and Turner forthcoming).

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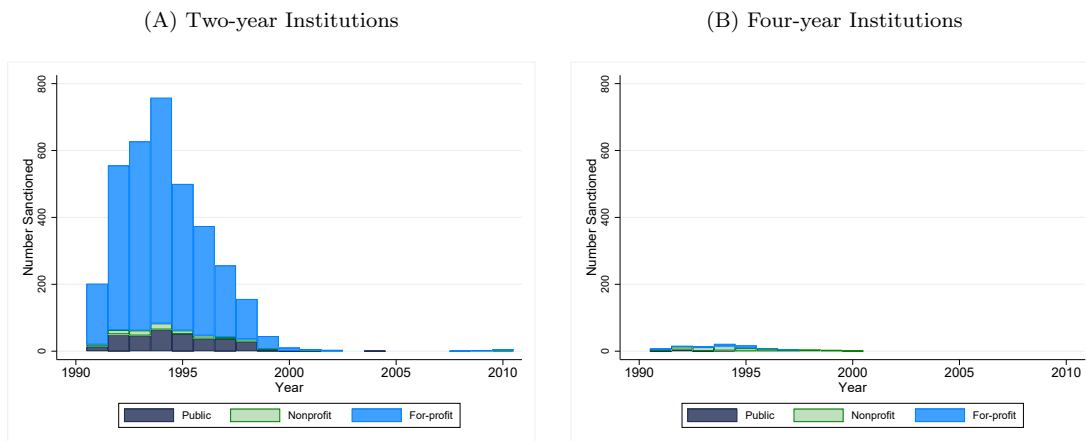


FIGURE 1. SANCTIONS BY SECTOR, YEAR, AND LEVEL

Source: Sanction administrative data. Notes: Sample includes all two- and four-year institutions with federal borrowers entering repayment. Institutions may receive a sanction in more than one year.

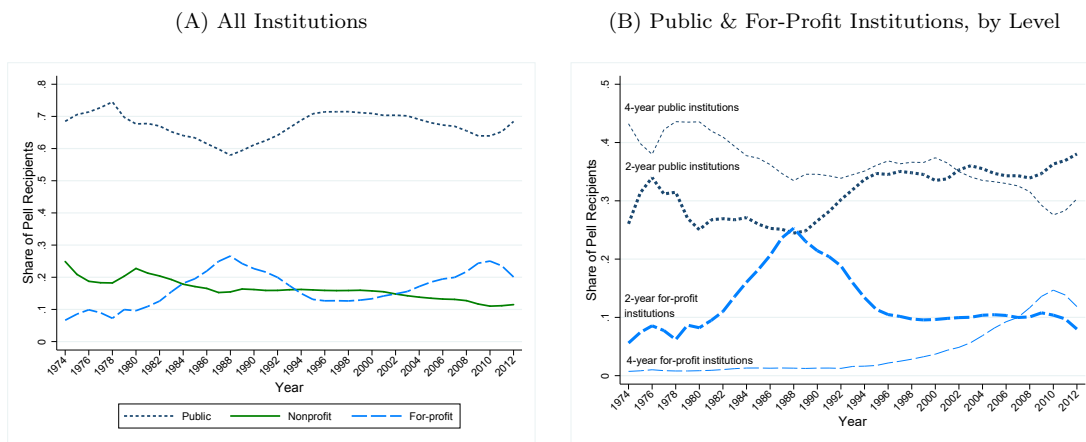
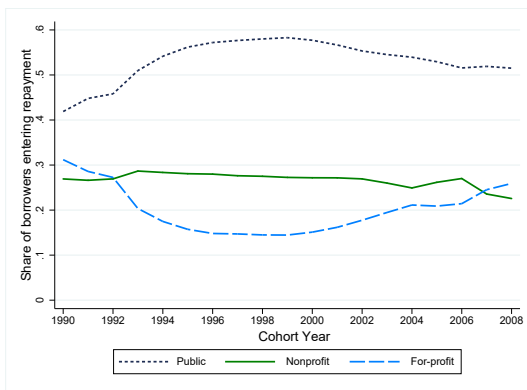


FIGURE 2. THE DISTRIBUTION OF PELL GRANT RECIPIENTS ACROSS SECTORS BY YEAR

Source: Pell Grant administrative data. Notes: Sample includes two- and four-year institutions (Panel A) or two- and four-year public and for-profit institutions (Panel B) with Pell Grant enrollment. In Panel B, thick lines represent two-year institutions (including less than two-year institutions), and thin lines represent four-year institutions.

(A) Federal Borrowers



(B) Cohort Default Rates

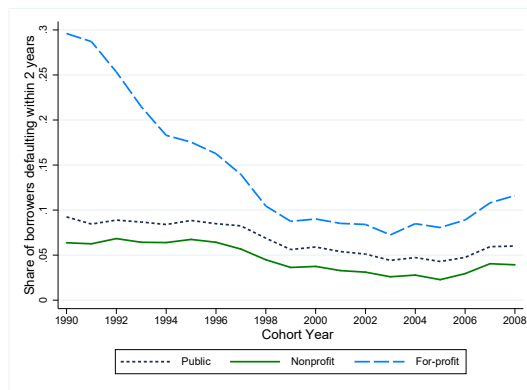


FIGURE 3. BORROWERS AND DEFAULT RATES BY SECTOR AND YEAR

Source: CDR administrative data. *Notes:* Sample includes two- and four-year institutions with CDR data. Panel A shows the share of federal borrowers entering repayment in the specified cohort-year by sector. Panel B shows the share of total federal borrowers who defaulted within two years of entering repayment.

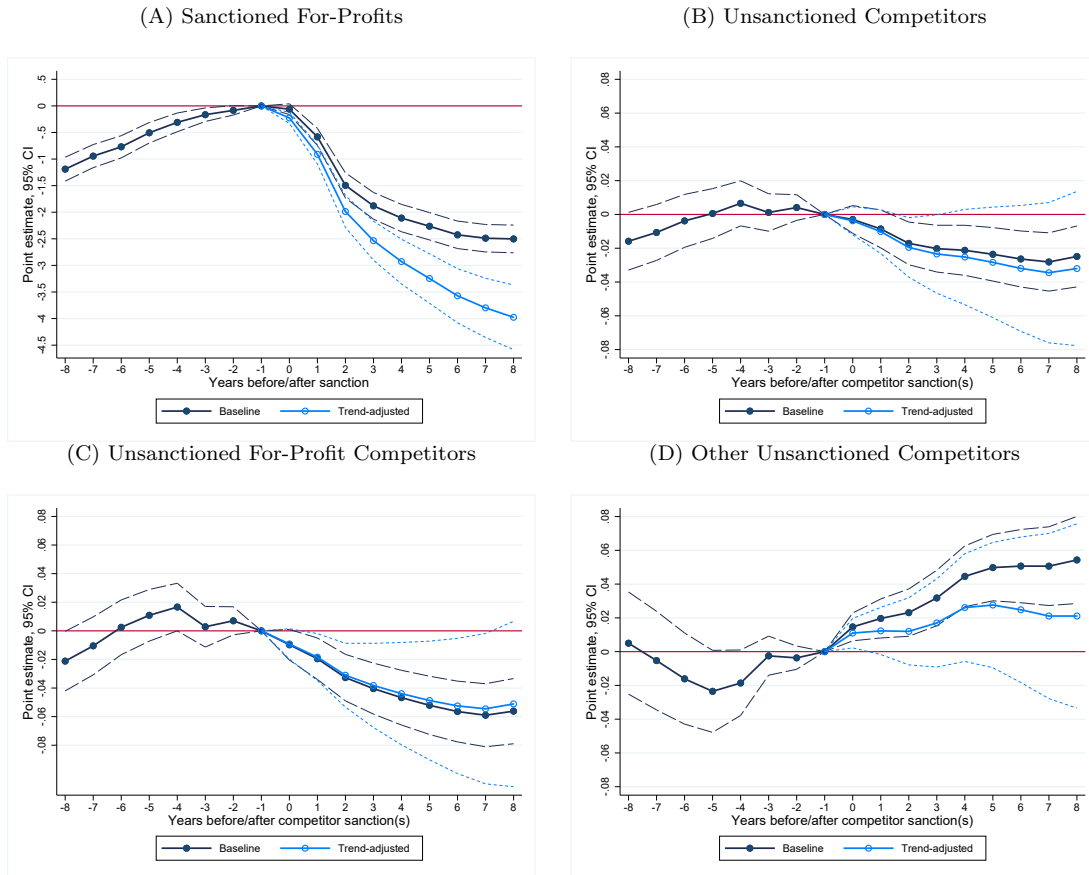
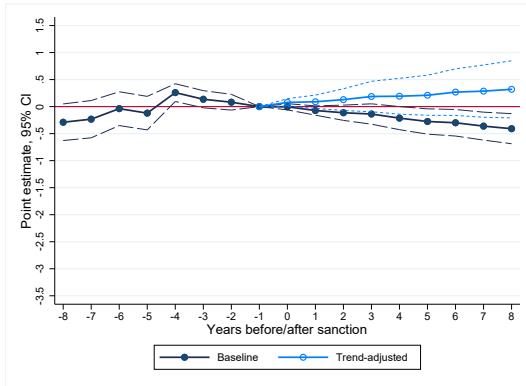


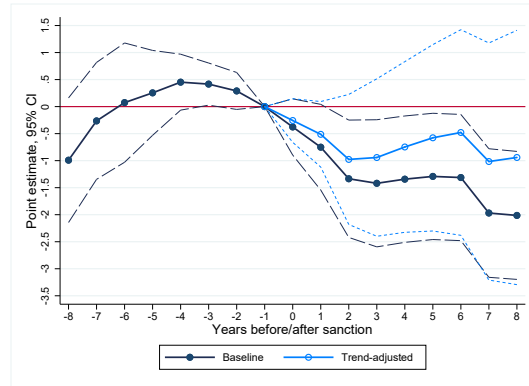
FIGURE 4. THE EFFECT OF FOR-PROFIT COLLEGE SANCTIONS ON PELL GRANT RECIPIENT ENROLLMENT

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* Sample includes two-year institutions with a Title IV program participation and Pell Grant recipient enrollment between in at least one year between 1982 and 2008 in counties with fewer than 50 institutions (on average, across years). Closed institution enrollment is set to zero. Point estimates and 95 percent confidence intervals from a regression of $\ln(\text{Pell Grant recipients} + 1)_t$ on sector-specific sanction indicators, $\ln(\text{Pell recipients exposed to sanctions})_{\tau-1}$ in a given sector, interacted with sector and years pre-/post-sanction receipt (with $t = \tau - 1$ serving as the omitted category), institution by sanction-year fixed effects, year fixed effects, years before/after sanction receipt fixed effects, and sector and county linear trends. Confidence intervals constructed from robust standard errors clustered by institution. Hollow circle markers and lighter confidence intervals correspond to the “trend adjusted” estimates (see Section III for details).

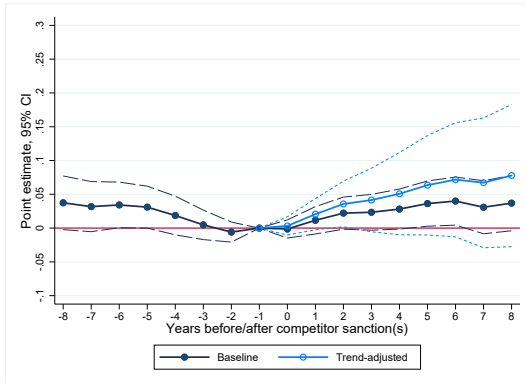
(A) Sanctioned Public Institutions



(B) Sanctioned Nonprofit Institutions



(C) Unsanctioned Competitors of Sanctioned Public Institutions



(D) Unsanctioned Competitors of Sanctioned Nonprofit Institutions

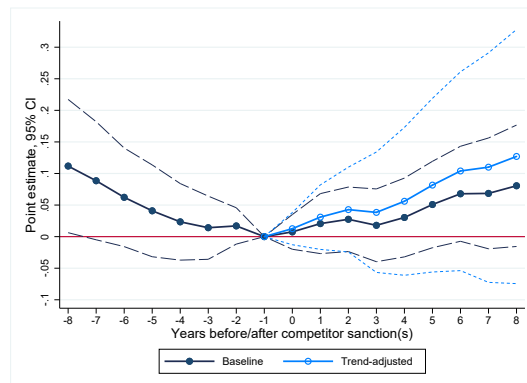
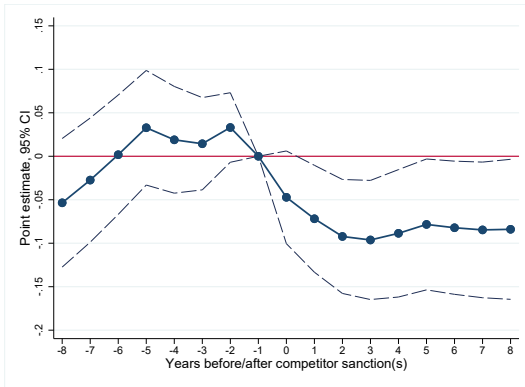


FIGURE 5. THE EFFECT OF PUBLIC AND NONPROFIT COLLEGE SANCTIONS ON PELL GRANT RECIPIENT ENROLLMENT

Source: Pell Grant, CDR, sanction, and PEPS administrative data. Notes: See Figure 4 notes for sample and specifications. Hollow circle markers and lighter confidence intervals correspond to the “trend-adjusted” estimates (see Section III for details).

(A) Unsanctioned For-Profit Competitors of Sanctioned For-Profits: Same Industry



(B) Unsanctioned For-Profit Competitors of Sanctioned For-Profits: Different Industry

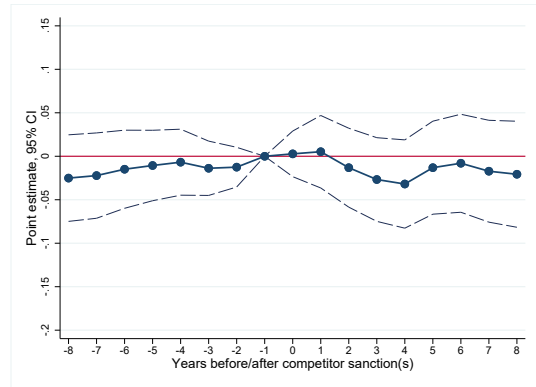


FIGURE 6. THE EFFECT OF FOR-PROFIT COLLEGE SANCTIONS ON COMPETITOR ENROLLMENT: HETEROGENEITY BY INDUSTRY

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Figure 4 notes for sample description. Point estimates and 95 percent confidence intervals from a regression of $\ln(\text{Pell Grant recipient enrollment} + 1)_t$ on $\ln(\text{Pell recipients exposed to sanction} + 1)_{\tau-1}$ within sanctioned for-profit institutions in the market and same industry (Panel A) or a different industry (Panel B) interacted with years before/after sanction receipt (with $t = \tau - 1$ serving as the omitted category), allowing for separate own enrollment and spillover effects for beauty schools (see Appendix Figure C.5 for these estimates). Regressions also control for effects of sanctions, allowed to vary by sector and years before/after sanction receipt, $\ln(\text{Pell recipients exposed to sanction} + 1)_{\tau-1}$ in the public and nonprofit sectors, allowed to vary with sector and years before/after sanction receipt, and indicators for institution by sanction-year fixed effects, year fixed effects, years before/after sanction receipt fixed effects, and sector and county linear trends. Confidence intervals constructed from robust standard errors clustered by institution.

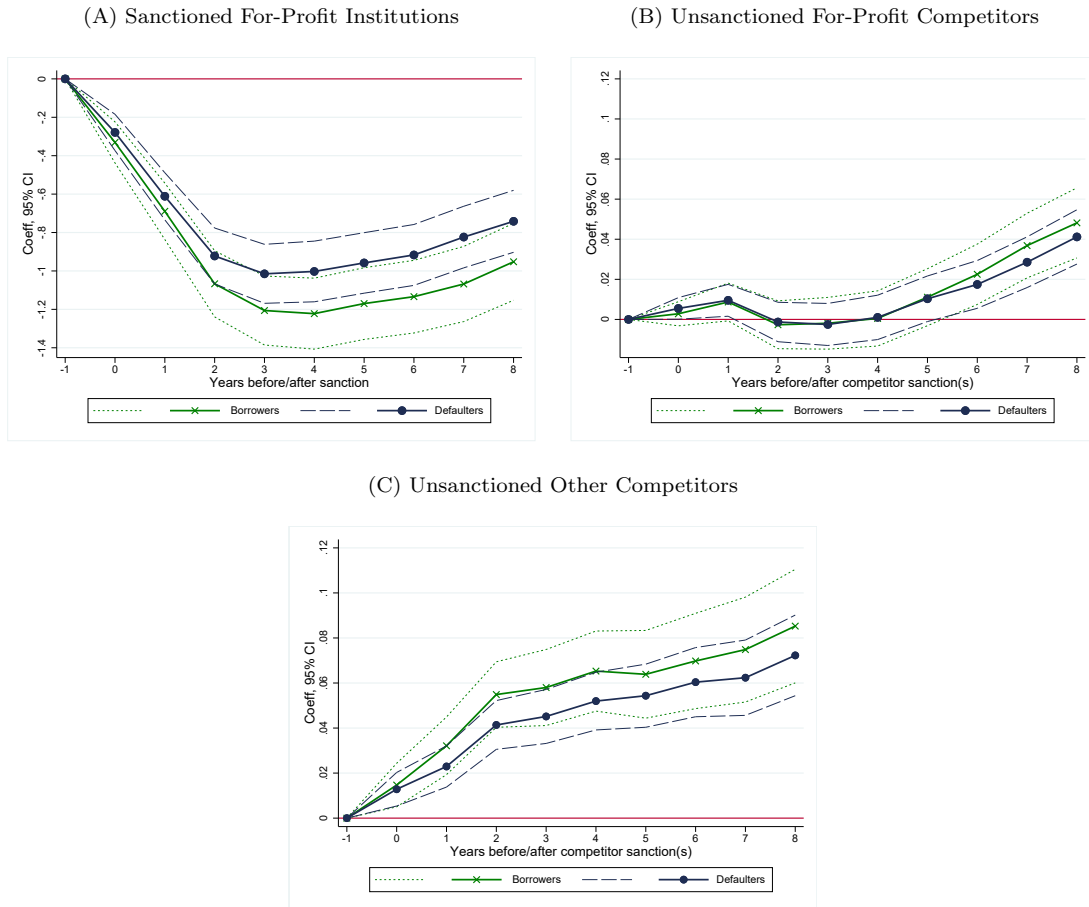


FIGURE 7. BORROWING AND DEFAULTS FOLLOWING FOR-PROFIT SANCTIONS

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* Sample includes two-year institutions with a Title IV program participation agreement and Pell Grant recipient enrollment between in at least one year between 1982 and 2008. Institutions in counties with more than 50 institutions (on average, across years) are excluded. Competitor institutions are other unsanctioned two-year institutions in the local higher education market (county). Closed institution borrowers and defaulters are set to zero. Point estimates and 95 percent confidence intervals from a regression of $\ln(\text{borrowers} + 1)_t$ (green Xs with light green confidence interval) or $\ln(\text{defaulters} + 1)_t$ (blue hollow circles with medium-blue confidence interval) on an indicator for whether the for-profit institution received a sanction in the sanction year (Panel A), $\ln(\text{Pell recipients exposed to sanction} + 1)_{\tau-1}$ within sanctioned for-profit institutions in the market interacted with an indicator for public or nonprofit sector (Panel B) or for-profit sector (C) and years before/after sanction receipt (with $t = \tau - 1$ serving as the omitted category). Regressions also include institution by sanction-year fixed effects, year fixed effects, years since sanction fixed effects, and sector and county linear trends. Confidence intervals constructed from robust standard errors clustered by institution. See Section IV for additional details.

TABLE 1—CHARACTERISTICS OF INSTITUTIONS BY SECTOR AND SANCTION RECEIPT

	(1) Public	(2) Nonprofit	(3) For-profit
<i>A. All institutions in sector</i>			
Average Pell Grant recipient enrollment			
Pre-sanction ($t - 8$ to $t - 1$)	467	66	74
Post-sanction ($t = 0$ to $t + 8$)	667	70	74
Share of Pell recipients ($t - 1$)	0.80	0.03	0.18
Observations (school by sanction year)	10,005	2,975	14,288
<i>B. Sanctioned institutions in sector</i>			
Average Pell Grant recipient enrollment			
Pre-sanction ($t - 8$ to $t - 1$)	330	110	223
Post-sanction ($t = 0$ to $t + 8$)	467	83	117
Share of Pell recipients ($t - 1$)	0.19	0.01	0.80
Observations	97	14	465
<i>C. Unsanctioned competitors of sanctioned public institution</i>			
Average Pell Grant recipient enrollment			
Pre-sanction ($t - 8$ to $t - 1$)	585	44	70
Post-sanction ($t = 0$ to $t + 8$)	955	61	79
Share of Pell recipients ($t - 1$)	0.69	0.02	0.28
Observations (school by sanction year)	98	55	342
<i>D. Unsanctioned competitor of sanctioned nonprofit institution</i>			
Average Pell Grant recipient enrollment			
Pre-sanction ($t - 8$ to $t - 1$)	721	59	88
Post-sanction ($t = 0$ to $t + 8$)	1212	46	103
Share of Pell recipients ($t - 1$)	0.61	0.02	0.37
Observations (school by sanction year)	27	24	129
<i>E. Unsanctioned competitors of sanctioned for-profit institution</i>			
Average Pell Grant recipient enrollment			
Pre-sanction ($t - 8$ to $t - 1$)	634	51	107
Post-sanction ($t = 0$ to $t + 8$)	1075	62	99
Share of Pell recipients ($t - 1$)	0.62	0.03	0.36
Observations (school by sanction year)	552	334	2,100

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* Sample includes two-year institutions with a Title IV program participation and Pell Grant recipient enrollment between in at least one year between 1982 and 2008 in counties with fewer than 50 institutions (on average, across years). Competitor institutions are other unsanctioned two-year institutions in the local higher education market (county). Closed institution enrollment is set to zero.

TABLE 2—THE EFFECT OF SANCTIONS ON PELL GRANT RECIPIENT ENROLLMENT

<i>Sanctioned sector:</i>	<i>1. Public</i>	<i>2. Nonprofit</i>	<i>3. For-profit</i>	Test of equality (<i>p</i> -value)
<i>A. Pooled competitor effects</i>				
Post X Threatened sanction	-0.212 (0.111)+	-1.390 (0.449)**	-1.131 (0.094)**	<0.001
Post X ln(recipients exposed to sanctions in sector)	0.003 (0.011)	-0.0002 (0.030)	0.005 (0.005)	0.973
Counties		1,364		
Institutions		5,845		
Observations		463,556		
<i>B. Heterogeneity by Competitor Sector</i>				
Post X Threatened sanction	-0.198 (0.111)+	-1.379 (0.449)**	-1.139 (0.094)**	<0.001
Post X ln(recipients exposed to sanctions in sector)				
x Public	0.024 (0.025)	0.012 (0.054)	0.064 (0.011)**	0.277
x Nonprofit	-0.021 (0.027)	-0.049 (0.090)	0.039 (0.017)*	0.146
x For-profit	0.002 (0.016)	0.012 (0.036)	-0.015 (0.007)*	0.554
Test of equality (<i>p</i> -value)	0.481	0.817	<0.001	
Counties		1,364		
Institutions		5,845		
Observations		463,556		

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Table 1 notes for sample description. Estimates from a regression of $\ln(\text{Pell recipient enrollment} + 1)_t$ on sanction receipt interacted with post-sanction receipt and sector, $\ln(\text{Pell recipients exposed to sanction} + 1)_{t-1}$ within sanctioned public, nonprofit, and for-profit institutions in the market, interacted with post-sanction receipt and sector, institution by sanction-year fixed effects, year fixed effects, years since sanction fixed effects, and sector and county linear trends. Robust standard errors clustered by institution in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

TABLE 3—THE EFFECT OF SANCTIONS ON MARKET-WIDE PELL GRANT RECIPIENT ENROLLMENT

<i>Sanctioned sector:</i>	<i>1. Public</i>	<i>2. Nonprofit</i>	<i>3. For-profit</i>
Baseline (<i>t</i> - 1) enrollment:			
Sanctioned school	385	137	333
Public competitor	764	1007	831
NP competitor	45	38	57
FP competitor	90	127	126
Predicted Δ institution enrollment:			
Sanctioned school	-69	-103	-226
Public competitor	19	12	55
NP competitor	-1	-2	2
FP competitor	0.2	2	-2
Number of institutions:			
Public competitor	2.8	2.9	2.8
NP competitor	3.3	2.9	2.2
FP competitor	14.5	17.3	14.8
Predicted Δ market-wide competitor enrollment:			
Public competitor	53	35	154
NP competitor	-3	-6	4
FP competitor	3	26	-28
Predicted Δ market enrollment:	-16	-45	-96
additional sanction in sector	(61)	(193)	(28)**
% change (rel to market baseline)	-0.4%	-0.9%	-2.2%

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Table 1 notes for sample description. N = 463,556. Predicted change in institutional enrollment is calculated from exponentiated point estimates in Table 2, multiplied by baseline enrollment. Predicted change in market-wide competitor enrollment is calculated by multiplying the average number of unsanctioned public, nonprofit, or for-profit competitors in markets with the predicted change in institutional enrollment. The total predicted change in market enrollment is the sum of sanctioned school and market-wide competitor enrollments.

TABLE 4—THE EFFECT OF SANCTIONS ON PELL GRANT RECIPIENT ENROLLMENT: HETEROGENEITY BY CHAIN STATUS

<i>Sanctioned sector:</i>	<i>For-profit</i>
Post x Threatened sanction	-1.139 (0.094)**
Post x ln(recipients exposed to sanctions in sector)	
x Public	0.065 (0.011)**
x Nonprofit	0.039 (0.017)*
x For-profit	
x Nonchain or different chain than sanctioned competitor(s)	-0.013 (0.007)+
x Same chain as sanctioned competitor(s)	-0.112 (0.065)+
	[0.136]
<i>No competitors in same chain</i>	
<i>Predicted Δ market enrollment</i>	-93 (28)**
<i>% change (rel to baseline)</i>	-2.2%
<i>1+ competitors in same chain</i>	
<i>Predicted Δ market enrollment</i>	-83 (27)**
<i>% change (rel to baseline)</i>	-1.9%

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Table 1 notes for sample description. $N = 463,556$. Estimates from a regression of $\ln(\text{Pell recipient enrollment} + 1)_t$ on sanction receipt interacted with post-sanction receipt and sector, $\ln(\text{Pell recipients exposed to sanction} + 1)_{\tau-1}$ within sanctioned public, nonprofit, and for-profit institutions in the market, interacted with post-sanction receipt and sector, institution by sanction-year fixed effects, year fixed effects, years since sanction fixed effects, and sector and county linear trends. Effects of for-profit Pell recipients exposed to sanctions on unsanctioned for-profit competitor enrollment allowed to vary by same-chain membership. See Online Appendix A for description of the classification of for-profit chains. Estimated main and spillover effects of public and nonprofit sanctions are not reported. Robust standard errors clustered by institution in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

TABLE 5—THE EFFECT OF SANCTIONS ON PELL GRANT RECIPIENT ENROLLMENT: HETEROGENEITY BY INDUSTRY

	(1) Pooled	(2) Beauty vs other FPs		(3) By for-profit industry						Test of joint sig. (<i>p</i> -val)	Test of eq. (<i>p</i> -val)
		<i>Beauty</i>	<i>Other</i>	<i>General</i>	<i>Health</i>	<i>Beauty</i>	<i>Comp/business</i>	<i>Culinary/arts</i>	<i>Mech/engin</i>		
Post x Threatened sanction											
x For-profit	-1.134 (0.094)**	-0.783 (0.096)**	-1.563 (0.192)**	-1.433 (0.302)**	-1.033 (0.655)	-0.859 (0.094)**	-2.205 (0.292)**	-0.895 (0.725)	-0.924 (0.526)+	<0.001	0.001
Post x ln(FP recipients exposed to sanction)											
x Public	0.066 (0.011)**	-0.012 (0.023)	0.081 (0.016)**	0.060 (0.018)**	-0.129 (0.039)**	0.047 (0.015)**	0.043 (0.027)	0.144 (0.077)+	0.019 (0.039)	<0.001	0.001
x Nonprofit	0.041 (0.017)*	0.033 (0.035)	0.034 (0.023)	-0.004 (0.029)	0.137 (0.060)*	0.045 (0.026)+	-0.0003 (0.029)	0.101 (0.072)	0.070 (0.057)	0.008	0.239
x For-profit											
x Same industry	-0.041 (0.016)*	-0.011 (0.019)	-0.057 (0.032)+	-0.029 (0.027)	-0.015 (0.129)	-0.034 (0.018)+	-0.111 (0.056)*	-0.045 (0.195)	-0.104 (0.125)	0.181	0.818
x Different industry	0.007 (0.010)	0.013 (0.014)	0.020 (0.022)	0.001 (0.014)	0.036 (0.031)	-0.002 (0.018)	0.011 (0.018)	-0.005 (0.048)	-0.005 (0.027)	0.922	0.912
Test of equality (<i>p</i> -value)	0.026	0.345	0.073	0.351	0.700	0.220	0.045	0.841	0.449		

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Table 1 notes for sample description. N = 463,556. Column (1) contains estimates from a regression of $\ln(\text{Pell recipient enrollment} + 1)_t$ on sanction receipt interacted with post-sanction receipt and sector, $\ln(\text{Pell recipients exposed to sanction} + 1)_{t-1}$ within sanctioned public, nonprofit, and for-profit institutions in the market, interacted with post-sanction receipt and sector, institution by sanction-year fixed effects, year fixed effects, years since sanction fixed effects, and sector and county linear trends. Effects of for-profit Pell recipients exposed to sanctions on unsanctioned for-profit competitor enrollment allowed to vary by same-industry classification. In (2), effects of for-profit sanctions on own and competitor enrollment are allowed to vary across beauty schools and other non-beauty for-profits. In (3), effects are allowed to vary across all for-profit industries. See Online Appendix A for a description of the classification of for-profit industries. Estimated main and spillover effects of public and nonprofit sanctions are not reported. Robust standard errors clustered by institution in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

TABLE 6—MARKET-WIDE BORROWING AND DEFAULTING BORROWING AND DEFAULTS FOLLOWING FOR-PROFIT SANCTIONS

<i>Sanctioned sector:</i>	<i>1. Public</i>	<i>2. Nonprofit</i>	<i>3. For-profit</i>
A. Borrowers			
Baseline number of borrowers in:			
Sanctioned school	48	82	143
Public competitor	150	264	151
NP competitor	36	32	37
FP competitor	96	113	94
Predicted Δ in sanctioned school borrowers	-8	-50	-91
Predicted Δ in market-wide borrowers in:			
Public competitors	24	27	24
NP competitors	3	4	2
FP competitors	-8	-42	6
Predicted Δ in market borrowers per additional sanction in sector	10 (19)	-61 (48)	-59 (7)**
% change (rel to market baseline)	1%	-2%	-3%
B. Defaulters			
Baseline number of defaulters in:			
Sanctioned school	13	38	61
Public competitor	27	36	23
NP competitor	4	3	4
FP competitor	23	26	27
Predicted Δ in sanctioned school defaulters	-2	-21	-34
Predicted Δ in market-wide defaulters in:			
Public competitors	24	4	3
NP competitors	3	0.3	0.3
FP competitors	-8	-8	2
Predicted Δ in market defaulters per additional sanction in sector	3 (4)	-25 (8)**	-30 (2)**
% change (rel to market baseline)	1%	-5%	-5%

Source: Pell Grant, CDR, sanction, and PEPS administrative data. *Notes:* See Table 1 notes for sample description. Observations from more than one year prior to sanction receipt or competitor sanction receipt are excluded to maintain a balanced panel ($N = 272,680$). Closed institution borrowers and defaulters set to zero. Predicted change in sanctioned borrowers or defaulters is calculated from exponentiated point estimates in Appendix Table C.14, which contains estimates from a regression of $\ln(\text{borrowers} + 1)_t$ (Panel A) or $\ln(\text{defaulters} + 1)_t$ (Panel B) on sanction receipt and $\ln(\text{Pell recipients exposed to sanction} + 1)_{t-1}$ in the for-profit, nonprofit, and public sectors, both allowed to vary with an institution's sector and interacted with an indicator for post-sanction receipt. Regressions also control for institution by institution by sanction-year fixed effects, year fixed effects, years before/after sanction receipt fixed effects, and sector and county linear trends. See Section IV for additional details. Robust standard errors clustered by institution in parentheses; ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.