



Integrity versus access? The effect of federal financial aid availability on postsecondary enrollment[☆]



Rajeev Darolia

University of Missouri, 118 Middlebush Hall, Columbia, MO 65211, United States

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ABSTRACT

It is generally believed that access to financial aid will increase the likelihood that students will attend and graduate from college. There is a surprising lack of research, however, on the consequences when postsecondary institutions lose eligibility to disburse financial aid. This paper provides among the first causal estimates of institution-level financial aid funding loss on enrollment and composition of student bodies. I implement a dynamic regression discontinuity design using a multi-year rule that restricts institutions' eligibility to offer federal aid such as Pell Grants and subsidized loans when alumni's loan repayment rates are below allowed thresholds. Results suggest that financial aid loss discourages enrollment at for-profit institutions and institutions that offer programs of two years or less. The decline in enrollment appears to be driven by fewer new enrollees, particularly at for-profit colleges. I find less conclusive evidence that ineligibility to disburse federal financial aid substantially alters student body composition. This research is particularly relevant considering recently proposed federal rulemaking that will further limit the number of institutions eligible to disburse financial aid based on additional student loan debt repayment requirements. Restrictions such as these are intended to protect students and the integrity of federal aid programs, but may also have implications for access to higher education.

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1. Introduction

A robust economics literature examines the effect of financial aid on students' likelihood of enrolling in and graduating from college. Little is known, however, about the consequences for students and schools when postsecondary institutions lose the ability to disburse financial aid. Recently, spurred by government and media reports of federal aid fraud and abuse at some for-profit colleges (e.g., GAO, 2010; Lewin, 2010), the US Department of Education ("ED") proposed controversial additions to requirements that determine which institutions are eligible to disburse federal financial aid. These regulations attempt to further limit the number of postsecondary institutions disbursing federal financial aid, including Pell Grants, Stafford Loans, and other aid.

This type of rulemaking endeavors to protect students, taxpayers, and the "integrity" of federal aid programs by holding institutions accountable for the use of federal student aid funds. Opponents, however, allege that such regulations are discriminatory, unnecessarily burdensome, disproportionately harm at-risk and underserved students, and

have potentially negative implications for access to postsecondary education (ED, 2011a). Regulatory initiatives of this type are not uncommon. In the past 20 years, the ED has implemented a number of policies that determine which institutions are allowed to disburse federal aid. Key components of recently proposed program integrity regulatory efforts were vacated because of the lack of empirical evidence available to support requirements. Because of ongoing concern about the value of educational experiences at certain institutions, however, there will likely be significant contentious policy proposals related to institution-level financial aid disbursement eligibility considered in the future.

In this study, I present the first causal estimates to my knowledge of the effects on institution-level enrollment and student body composition when postsecondary institutions lose eligibility to disburse federal financial aid. Certain federal financial aid program integrity regulations aim to safeguard students by discouraging their attendance at schools that allegedly provide little return to human capital investments or result in unmanageable debt. The rules also attempt to protect taxpayers by limiting publicly subsidized enrollment at institutions where former students do not or cannot repay student loans. As a result, institutions that lose aid disbursement eligibility may experience decreases in the matriculation of financially and/or geographically constrained students. As well, the lack of debt repayment by former students may serve as a negative signal to prospective students about the returns to education at ineligible schools. Over time, aid disbursement ineligibility may also affect the management and survival of institutions, leading to closure

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E-mail address: DaroliaR@missouri.edu.

or an operations overhaul. Alternatively, if losses of institutional aid eligibility have no impact on schools' enrollments or student body compositions, such findings would suggest that institutions can compensate for the loss of federal aid or that this aid is not critical to operations.

The challenge with isolating causal estimates of institutional aid eligibility loss is that ineligibility is possibly correlated with a number of institutional strategies or problems, such as poor management or lack of viable program offerings that could also affect enrollment and student body composition. To isolate the loss of federal aid eligibility as the source of enrollment changes, I take advantage of one of the existing federal requirements that renders schools ineligible to disburse funds if the rate at which former students default on student loan obligations (the yearly cohort default rate or "CDR") exceeds 40% in a single year or 25% for three consecutive years.

These thresholds are used to implement a dynamic regression discontinuity ("DRD") design. In the DRD, I identify differences in institution-level outcomes by comparing the enrollment and student body composition of institutions that are just over the eligibility cutoffs against the outcomes of institutions that are just under. Schools cannot precisely select themselves into ineligible (treatment) and eligible (control) groups and therefore variation in treatment near the cutoff can be considered as good as randomized (Lee and Lemieux, 2010). I use a relatively new variation of a regression discontinuity design to dynamically take into account the multiple year conditionality of one of the thresholds. Institutional data comes from the National Center for Education Statistics Integrated Postsecondary Education Data System ("IPEDS") and is combined with institution-level cohort default rate data available from the ED.

The primary findings provide evidence that program integrity rules are effective in discouraging enrollment at schools with alumni who default on student loans at a high rate, with eligibility loss due to CDR threshold violations resulting in lower annual overall enrollment of approximately 12–16% at for-profit and two-year institutions. The results appear driven by declines in first-year students, particularly at for-profit institutions, and observed medium-to-long run consequences are larger than immediate ones. Reduced enrollment at institutions that fail student loan debt measures might be considered a favorable outcome if these schools produce a large number of students with unaffordable debt burdens. Further research is needed, however, to directly assess whether students who forgo education at ineligible schools transfer to other programs or drop out of higher education altogether. In addition to lower enrollment, I observe that completions declined at for-profit institutions after eligibility loss, though this result is not robust to all sensitivity tests. I find little conclusive evidence that the composition of student bodies at ineligible schools changes substantially because of loss of eligibility.

The paper is organized as follows. Section 2 provides context on federal financial aid. Section 3 reviews existing literature on the effects of financial aid and discusses why institutional federal financial aid program ineligibility has consequences for enrollment and student body composition. Section 4 presents identifying assumptions and empirical research design. Sections 5 and 6 provide an overview of data and a discussion of results. Section 7 concludes and provides policy implications.

2. Background on federal financial aid and cohort default rates

Title IV of the Higher Education Act of 1965 and subsequent amendments ("Title IV") govern the primary set of federal programs providing financial assistance for postsecondary students. Title IV authorizes a number of well-known federal grant and loan programs, such as Pell Grants, federal student aid work–study, and Stafford and PLUS loans through the William D. Ford Federal Direct Loan Program ("FDLP").¹ Table 1 lists Title IV grant and loan programs, along with related expenditures for the 2009–2010 school year. Concerns persist

¹ Prior to July 1, 2010, the Federal Family Education Loan ("FFEL") program allowed private lenders to offer federally subsidized student loans.

Table 1
Title IV federal student aid, 2009–2010 academic year.

Program	Total aid (\$MM)	10-Year change in total aid
<i>Grants</i>		
Pell	\$28,213	203%
Supplemental Educational Opportunity (SEOG)	\$758	– 5%
Leveraging Educational Assistance Partnership (LEAP)	\$63	95%
Academic Competitiveness (ACG)	\$503	n/a
Science and Mathematics Access to Retain Talent (SMART)	\$361	n/a
<i>Loans</i>		
Perkins	\$1106	– 22%
Subsidized Stafford – Federal Direct (FDLP)	\$14,190	105%
Subsidized Stafford – Federal Family Education (FFELP)	\$22,551	61%
Unsubsidized Stafford – Federal Direct (FDLP)	\$16,721	251%
Unsubsidized Stafford – Federal Family Education (FFELP)	\$27,968	155%
PLUS – Federal Direct (FDLP)	\$5934	309%
PLUS – Federal Family Education (FFELP)	\$8231	195%
<i>Federal Work Study</i>	\$1417	20%

Source: Baum et al. (2010). ACG and SMART grants started within the past 10 years and therefore 10 year change is not displayed.

that some of this public investment is wasted by generating revenue for certain schools that do not offer a high-quality education (GAO, 2010). Indeed, much of the consternation about alleged abuses by for-profit schools concerns claims that some schools commandeer federal funds by having students pay for tuition using public money and provide very little in return.

Institutions lose Title IV eligibility for a number of reasons: voluntary withdrawal from the program; change in status (e.g., school opening/closure, change in ownership, or merger); failure to comply with program requirements (e.g., gain or loss of accreditation, financial responsibility, or administrative capability); federal policy changes; and subject to loss because of high student loan cohort default rates (Congressional Research Service, 2007). For this study, I focus on Title IV eligibility changes due to the final reason, subject to loss because of high student loan cohort default rates.

The main empirical identification strategy in this study examines loss of Title IV eligibility because of high institution-level student loan CDRs. CDRs are calculated as the percentage of a school's former students who, within a two year period after starting repayment of particular FFEL and FDLP loans, default on student loan obligations.² Institutions with CDRs greater than or equal to 25% for three consecutive years or exceeding 40% for one year are subject to loss of Title IV program eligibility for the remainder of the year in which the ED notifies the institution of its violation and the ensuing two fiscal years.³ In order to regain Title IV fund disbursement eligibility, institutions need to prove compliance with regulations. There is some additional flexibility in disbursement afforded to schools with extremely low CDRs, though they are not likely to be generous enough to substantially influence institution behavior.⁴

² PLUS, Grad PLUS, Insured Student Loans, and Perkins loans are not included in cohort default rate calculations. For schools with less than 30 borrowers entering repayment in any fiscal year, the ED calculates default rates as an average repayment rate over a three year period.

³ Starting in 1992, institutions lose Title IV eligibility if they have cohort default rates exceeding 25% for 3 consecutive years or 40% for 1 year. The thresholds were 35 and 30% in for the multiple year cut-off and 45 and 40% for the single year cut-off in 1990 and 1991 respectively. In this study, I use the threshold value to which they were subject for each year.

⁴ For example, in some years, schools with cohort default rates less than five percent gain some privileges associated with disbursing loan proceeds to students studying abroad, and schools with cohort default rates less than ten percent earn flexibility with the timing and number of installments for loan disbursements.

3. Consequences of institutional aid loss and extant literature

There exists little prior research on the effect of institutional loss of financial aid on student or institution outcomes. Nevertheless, there is a great need to understand the consequences of institution-level aid availability. Important rules governing Title IV eligibility have been evaluated in the absence of evidence to inform such decisions. For example, the ED proposed “gainful employment” rules in 2010 as additional restrictions to the disbursement of Title IV financial aid at institutions. The rule applies to vocational programs broadly, though it is largely viewed as targeting programs in the for-profit sector. This policy proposed adding two additional measures of student loan repayment in order for institutions to be able to disburse Title IV funds to students of pertinent programs – the first dealing with the percentage of program alumni that are actively repaying their loans and the second regarding the ratio loan payments to income earned. The ED’s (2011a) regulatory impact analysis of the controversial gainful employment rule used hundreds of assumptions to estimate the impact on programs and students if eligibility to disburse Title IV funds is lost, yet there was little empirical evidence on which to rely for many of the central assumptions (Guryan and Thompson, 2010). Key components of this rule were ultimately struck down by a US District Court for not being based on expert studies or industry standards, providing a clear example of the dearth of available research to advise rulemaking of this type.

3.1. Supply-side effects

Though Title IV dollars do not directly accrue to the institution, schools may seek Title IV eligibility for a number of reasons. To the extent financial aid lowers net cost of attendance, eligibility may allow schools to attract a larger number of students, increasing the revenue earned by the institution. Because of a reliance on federal aid funds, the revenue provided through federal financial aid programs may even be necessary for some schools to operate (Moore, 1995; Taylor, 2010), although new research casts some doubt on these claims (Cellini and Goldin, 2012). Institutions may also have non-fiscal motivations for gaining Title IV eligibility. Financial aid may allow schools to attract a more diverse student body, which some believe is one of the aims of postsecondary institutions (Bowen and Bok, 1998).

Responses to loss of federal aid could include the provision of offsetting benefits to attract financially marginal students. One response would be to decrease tuition such that the price is lower than competitor schools. Schools may also engage in expenditures such as institution-provided scholarships and financial aid or other types of perks such as career counseling services or child care. Loss of aid may also compel institutions to increase marketing efforts and adjust operations.

Title IV program loss could differentially affect various types of institutions and therefore heterogeneity in effects across college sectors is examined in subsequent sections. For-profit colleges with relatively high tuition that rely on federal financial aid for revenue (Moore, 1995) may be at most risk for closure. The for-profit sector, however, may also be better suited to offer other benefits to attract financially marginal students, such as accelerated programs or convenient class schedules. Private schools with many funding sources and a relatively wealthy student body may not experience lower overall enrollment because of Title IV ineligibility, but may no longer be able to attract low-income students. Comparatively inexpensive colleges may experience minimal or no effects of Title IV eligibility loss if the time and effort of applying for federal financial aid outweigh potential benefits for many students.

3.2. Demand-side effects

Following basic human capital theory, raising the cost of postsecondary education for a given individual because of restricted

access to financial aid will result in a decreased likelihood postsecondary education is undertaken. As such, one would expect loss of federal aid to result in decreased demand at the institution by aid eligible students because of higher prices. Based on studies of individual-level outcomes, there is substantial evidence demonstrating that lowering costs increases access to postsecondary education (e.g., Dynarski, 2002; Heller, 1997; Kane, 2003; Leslie and Brinkman, 1987; Van der Klaauw, 2002), though the evidence regarding specific federal grant and loan programs is less conclusive. There is mixed evidence that Pell Grant availability encourages enrollment, especially among low-income students, though the effect of state-level grants appears to more consistently demonstrate an increase in enrollment and altered distribution of students across types and locations of institutions (e.g., Cellini, 2010; Cornwell et al., 2004; Curs et al., 2007; Dynarski, 2000; Hansen, 1983; Seftor and Turner, 2002). More equivocal is the ability for student loans to increase matriculation at higher education institutions. In a review of research, Haskins et al. (2009) conclude that student loans are necessary for the postsecondary matriculation and persistence decisions, though other studies are pessimistic that loans encourage low-income students to attend postsecondary education (e.g., Campagne and Hossler, 1998; Carneiro and Heckman, 2002; Heller, 2008).

Compared to individual access to financial resources, loss of financial aid availability on an institution level may uniquely affect the enrollment and application behavior of students on the financial margin. Some financially constrained students may have the opportunity to transfer to schools with more financial assistance. Certain students, however, might only undertake or continue with postsecondary studies at specific institutions; for example, schools that are geographically proximate offer conveniently scheduled courses, or provide online learning. If attendance is too costly at these schools because of a lack of federal financial aid, marginal students may forgo or drop out of higher education altogether. Other sources, such as state or institutional financial aid programs, may be able to partially offset loss in federal aid. Lack of aid availability may also affect decisions on the intensive margin, such that students at ineligible institutions may decide to borrow higher levels of private student loans or be more likely to decide to work to make up for the loss of federal aid.

Beyond just limitations in financial resources, demand may also decline because of the harmed reputation of the school that may result from failing eligibility requirements. A high rate of student loan defaults at an institution could serve as a powerful signaling mechanism about post-college prospects. If former students at an institution are not repaying debt obligations, this may lead prospective students to believe that there are low returns to education at the institution. As such, prospective students may seek to attend schools where observable debt repayment activity is better or choose not to undertake postsecondary studies altogether.

Smaller enrollment at institutions without federal financial assistance could reflect a lower number of students attending postsecondary education overall (decreased access) or simply a transfer of students to schools with more available resources. Enrollment, transfer, and drop-out behavior in response to institution-level financial aid availability, moreover, may alter the distribution of students across institutions and student body composition at each school, potentially affecting academic experience and performance (e.g., Gurin et al., 2002; Zimmerman, 2003). Lack of financial aid resources at an institution may result in a student body that has a high proportion of students who do not need financial aid. In addition to a potentially more homogenous student body along socioeconomic dimensions, lack of financial assistance may result in less racial and ethnic diversity, to the extent that minority students are associated with lower incomes and wealth (e.g., Keister and Moller, 2000). Therefore, in addition to analyzing enrollment and completion volume effects, I also investigate changes in student composition.

As well, ineligible institutional responses to losing aid eligibility could result in a student body that has fewer students who are familiar

or comfortable with financial aid. Institutions that lose federal funding may lower tuition such that it has a lower tuition sticker price than competitor schools. Even with this lower sticker price, the institution may still have a higher net price after taking into account financial aid available at other institutions. Financially savvy students will be more likely to evaluate schools based on costs net of financial aid, while students with limited information may be attracted to the ostensibly lower sticker price.

Even for knowledgeable students and families, evaluating the net price may be challenging because of the complexity, timing, and unpredictability of the financial aid process, making it difficult for students to predict how much aid they will receive (Dynarski and Scott-Clayton, 2006). This may result in students avoiding financial aid and gravitating toward schools with lower tuition sticker prices, even if they are generally aware about financial aid. The lack of information may be particularly problematic for students from poor communities that have few members that have navigated the complicated aid process, as decisions about financial aid can be highly affected by information and peer networks (Perna, 2008). Bettinger et al. (2012) provide compelling experimental evidence of the information effect, as they find that providing information about and assistance with federal financial aid application paperwork increases application for and receipt of financial aid. As discussed subsequently, however, I do not find evidence of institution-level effects based on only information about schools' Title IV eligibility status being at risk.

4. Empirical estimation/identification strategy

To examine the effect of losing Title IV funds, I use the program requirement indicating that institutions with cohort default rates greater than or exceeding 25% for three consecutive years or exceeding 40% for one year are subject to loss of Title IV eligibility. An institution, *i*, becomes ineligible to disburse Title IV funds in a given year, *t*, according to the following mapping:

$$Ineligibility_{it}^* = \begin{cases} 1 \text{ if } (CDR_{it} \geq c^* \wedge CDR_{i,(t-1)} \geq c^* \wedge CDR_{i,(t-2)} \geq c^*) \\ 0 \text{ if } (CDR_{it} < c^* \vee CDR_{i,(t-1)} < c^* \vee CDR_{i,(t-2)} < c^*) \end{cases} \quad (1a)$$

$$Ineligibility_{it}^{**} = \begin{cases} 1 \text{ if } CDR_{it} > c^{**} \\ 0 \text{ if } CDR_{it} \leq c^{**} \end{cases} \quad (1b)$$

where *c** and *Ineligibility_{it}^{*}* represent the three year default rate cutoff (e.g., 25%) and ineligibility based on this threshold and *c^{**}* and *Ineligibility_{it}^{**}* represent the single year default rate cutoff (e.g., 40%) and related ineligibility. The primary outcome I analyze is the effect of Title IV loss on subsequent enrollment. I also examine student completions and two available measures of student body composition, the proportion of students who are minority race or ethnicity and the proportion of students identified as taking coursework part-time.

To examine the relationship between sanction from the Title IV financial aid program and outcomes, I start with simple ordinary least squares (“OLS”) estimation of the equation:

$$y_{it} = \beta_0 + \beta_1 Ineligibility_{it} + d_t + d_i + \varepsilon_{it} \quad (2)$$

where *i* indexes institutions and *t* indexes year; *d_t* represents indicators for year; *β₀* and *β₁* are estimated parameters; and *ε_{it}* is an error term. I define *Ineligibility_{it}* in two separate ways in various estimations and present results using both measures. The first is a vector including separate indicators for school-level Title IV ineligibility in year, *t* (i.e., *Ineligibility_{it}^{*}* and *Ineligibility_{it}^{**}*). I also estimate models using an ineligibility indicator for violation of either threshold (which is my preferred measure for ease of interpretation). I estimate the OLS models with an

institution fixed effect, *d_i*, to account for time-invariant factors that are common to each institution over time and may be especially important because of relative differences in size among schools. For the OLS models and all the regressions discussed hereafter, heteroskedasticity robust standard errors are clustered by institution.

The parameter *β₁* is the estimated relationship between an institution being subject to sanction from the Title IV program and outcomes. The OLS estimation, however, may not allow inference beyond conditional correlation on the relationship between Title IV ineligibility and outcomes. Changes in eligibility status might be correlated with changes in other unobserved school level characteristics, such as poor management or lack of viable program offerings that could also affect enrollment, that affect the outcome of interest, such that *E[ε_{it}] ≠ 0*, yielding biased estimates of *β₁*. As such, by exploiting variation in Title IV eligibility based on the level of former students default behavior, I undertake a regression discontinuity approach as a quasi-experimental design to estimate causal effects of institutional financial aid eligibility loss.

4.1. Dynamic regression discontinuity estimation

Using the cutoff for a single year threshold as an example, consider two potential outcomes for each institution, *i*: *y_{1i}* if the institution is ineligible to disburse Title IV funding and *y_{0i}* if the institution is eligible, such that *E[y_{1i}] – E[y_{0i}]* represents average effects of ineligibility. Institutions with CDRs lower than the eligibility cut-off, *c^{**}*, are Title IV eligible such that only *E[y_{0i}|CDR_i]* is observed when *CDR_i ≤ c^{**}*, while institutions with CDRs above the cutoff are Title IV ineligible, such that only *E[y_{1i}|CDR_i]* is observed when *CDR_i > c^{**}*.

The inexact nature of institutions' default rates and assignment into eligible and ineligible groups (discussed further below) allows for the assumption that schools in the neighborhood of the financial aid eligibility cutoff are as good as randomly assigned. Institutions that are just under the eligibility cutoff, the control group, therefore serve as a counterfactual to institutions that are just over the eligibility cutoff, the treatment group. Eq. (3) depicts a cross-sectional regression discontinuity design:

$$y_{it} = \alpha_0 + \alpha_1 Ineligibility_{it} + f\{(1 - Over_{it}^*) \times \widehat{CDR}_{it}^* \gamma_0\} + f\{Over_{it}^* \times (1 - Over_{it}^{**}) \times \widehat{CDR}_{it}^* \gamma_1\} + f\{Over_{it}^{**} \times \widehat{CDR}_{it}^{**} \gamma_2\} + d_t + d_i + u_{it} \quad (3)$$

The term, *Ineligibility_{it}* indicates whether institution, *i*, is eligible to disburse Title IV funds in year, *t*; *CDR_{it}^{*}* = *CDR_{it}* – *c**, the distance between each institution's CDR and threshold *c** (the 25% threshold); *CDR_{it}^{**}* = *CDR_{it}* – *c^{**}*, the distance between each institution's CDR and threshold *c^{**}* (the 40% threshold); *i* indexes institutions and *t* indexes year; *α₀* and *α₁* are estimated parameters; and *u_{it}* is an error term. As in Eq. (2), *d_t* represents year controls, and the model includes institution fixed effects, *d_i*, to account for the unique time-invariant characteristics of each institution over time.

I include three groups of terms to control for the relative distance between each institution's CDR and the two CDR thresholds, with terms constructed to allow slopes to differ in each range. *f*{(1 – *Over_{it}^{*}*) × *CDR_{it}^{*}*, *γ₀*} is a vector of polynomial functions of *CDR_{it}^{*}* interacted with an indicator for being under the cutoff *c**, with coefficients *γ₀*, and controls for relative distance where CDR is less than 25%. *f*{*Over_{it}^{*}* × (1 – *Over_{it}^{**}*) × *CDR_{it}^{*}*, *γ₁*} is a vector of polynomial functions of *CDR_{it}^{*}* interacted with an indicator for being over the cutoff *c** but under the cutoff *c^{**}*, with coefficients *γ₁*, and controls for relative distance where CDR is greater than or equal to 25%, but less than or equal to 40%. *f*{*Over_{it}^{**}* × *CDR_{it}^{**}*, *γ₂*} is a vector of polynomial functions

of \widehat{CDR}_{it}^{**} interacted with an indicator for being over the cutoff c^{**} , with coefficients γ_2 , and controls for relative distance where CDR is greater than 40%.

The nature of the multiple year cutoff, c^* , makes identification of ineligible institutions potentially problematic in a cross-sectional RD design. Sanctions based on violation of c^* in year t depend on violation of the threshold in the prior two years, $t - 1$ and $t - 2$. As such, institutions with CDRs above the cutoff in the current year may still be Title IV eligible if they did not exceed the threshold in the two prior years ($E[y_{0it}|CDR_{it}]$ can be observed when $CDR_{it} \geq c^*$ if $CDR_{it-1} < c^*$ or $CDR_{it-2} < c^*$).

To address this, I follow a dynamic regression discontinuity approach, as introduced by Cellini et al. (2010). I add to Eq. (3) lagged terms for the two years prior to the year in which the school is ineligible, yielding:

$$y_{it} = \alpha_0 + \alpha_1 Ineligibility_{it} + \sum_{\tau=0}^{\bar{\tau}} [\theta_{\tau} Over_{it-\tau}^* + f\{(1 - Over_{it-\tau}^*) \times \widehat{CDR}_{it-\tau}^* \gamma_{0\tau}\}] + f\{Over_{it-\tau}^* \times (1 - Over_{it-\tau}^{**}) \times \widehat{CDR}_{it-\tau}^{**} \gamma_{1\tau}\} + f\{Over_{it-\tau}^{**} \times \widehat{CDR}_{it-\tau}^{**} \gamma_{2\tau}\} + d_t + d_i + u_{it} \tag{4}$$

where i indexes institution, t indexes year, and τ indexes the number of years prior to eligibility loss, for $\tau \in [0, 1, 2]$. Eq. (4) includes, for the current and prior two years, indicators being over the eligibility threshold for the multiple year threshold, $Over_{it-\tau}^*$ as well as polynomial functions and interacted polynomial functions of relative distance between each institution's CDR and the two CDR thresholds as in Eq. (3). By adding these lagged factors, I control for satisfaction of the condition of needing cohort default rates equal to or above 25% in the two previous years, such that violation of the cutoff in the third year, $\tau = 0$, is arbitrary.

Following Cellini et al. (2010), my preferred sample includes all institutions in the analysis sample and absorbs variation from schools farther away from the cutoff using flexible controls for CDR. This approach is attractive because of sample size considerations and because there are two separate cutoffs 15 percentage points apart, such that narrow bands would lose potentially valuable variation around the different thresholds. My preferred specification includes first- (linear), second-, and third-order polynomials, though I find consistent results among bandwidths and polynomials (see discussion of sensitivity and falsification checks in Section 6).

The coefficient on the $Ineligibility_{it}$ term, α_1 , represents the local average treatment effect. Given perfect capture of ineligibility status, α_1 would represent treatment-on-the-treated (“TOT”) effects. However, because of measurement error that may lead to imprecise assignment into eligible and ineligible groups (discussed in Section 5), results are akin to intent-to-treat (“ITT”) effects, which are expected to be smaller than TOT effects. It is important to note that observed local treatment effects may not be relevant to the full population of schools. These local results, however, serve as evidence for more currently policy-relevant questions — namely, the consequences of losing Title IV eligibility for schools at risk of failing various student loan debt repayment requirements.

4.1.1. Tests for endogenous sorting

As a key assumption of the regression discontinuity design, I argue that schools cannot select themselves exactly into eligible or ineligible status. Even if schools attempt to strategically respond to rising or high CDRs, as long as they cannot precisely manipulate assignment, RD designs will produce estimates akin to random assignment (Lee and Lemieux, 2010). Schools may monitor loan repayments and attempt to influence repayment behavior, and in fact are encouraged to do so (ED, 2011b). A number of challenges, however, present difficulty for institutions to precisely manipulate CDRs. The determinant of student loan default is former students' actions post-graduation, when schools

will have little control over repayment behavior. Schools may try to restrict enrollment to only those students who are likely to repay loans or do not need financial aid, though predicting financial performance would be imprecise (especially since most students have little prior credit history). Alternatively, schools may offer more institutional aid or lower tuition in an effort to reduce overall costs to students. Some allege that schools may have repaid loans for students at risk of defaulting (Taylor, 2010), though this practice is explicitly prohibited and evidence for this practice is anecdotal.

Regardless of the strategic behavior in which institutions might try to engage (if any), the nature of default rates and the timing of their calculation would make it difficult to quickly affect CDRs, as attempts to influence CDRs would likely not manifest for many years. CDRs are calculated based on students who have already left the institution and have started loan repayment (which is deferred for many students). Therefore, any efforts put forth to influence repayment behavior of students would not be measured for some time.

To examine for potential manipulation by institutions, I test for density discontinuities around the two cutoffs. Higher densities just below the 25 or 40% cutoffs could signify that institutions are able to select their position into the Title IV eligible group. In Fig. 1, panels A and B, I plot a histogram of the density of institutions by CDR for all years in the sample and also kernel density plots using a triangular kernel for each year. Based on these graphs, there is no evidence of endogenous sorting of schools just below the default rate thresholds.⁵ To examine sorting more formally, I use the McCrary (2008) density test and find no evidence of higher density of schools just below the cutoffs. In fact, the positive point estimates of McCrary's parameter of interest (where $\hat{\theta} = \ln f^+ - \ln f^-$, the log difference in height just above and below the cutoffs) indicate a higher density of schools just above the 25 ($\hat{\theta} = 0.28$) and 40 ($\hat{\theta} = 2.46$) percent thresholds, the opposite of what one would expect if institutions can manipulate their CDRs to get under the relevant thresholds.

A more specific concern might be that behavior by institutions that have missed the 25% threshold for two consecutive years may attempt to get under the threshold in the third year. Therefore, in panels C and D in Fig. 1, I plot graphs including only the subset of institutions that failed the 25% cutoff in two consecutive years. Using McCrary density tests for this group, I find no evidence that institutions are able to select themselves under the 25 ($\hat{\theta} = 0.17$) or 40 ($\hat{\theta} = 0.10$) percent thresholds. This may reflect the impaired ability for institutions to affect their third year CDR because of timing and control issues previously discussed.

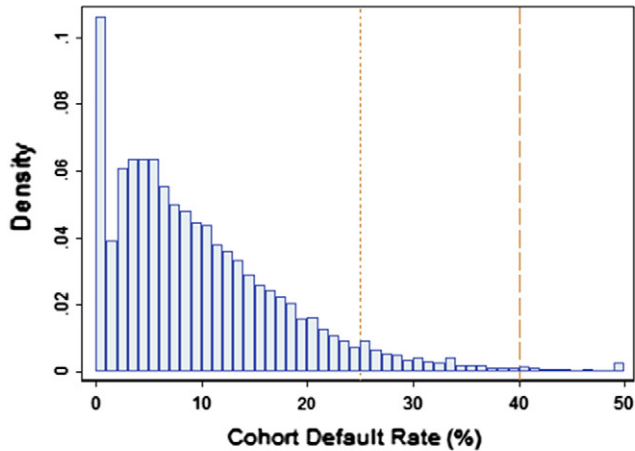
4.2. Expectations

In addition to responses to actual loss of eligibility, students and institutions may react to available information about whether an institution is at risk for failing Title IV debt requirements if the institution fails the multi-year CDR threshold for one or two years. Students at a school with default rates that exceed the multiple year CDR threshold two years in a row may preemptively transfer rather than risk failure in the third year and associated financial aid loss. Institutions may change its marketing or admittance behavior to prevent rising CDR rates. To test for this phenomenon, I fit the estimation:

$$y_{it} = \alpha_0 + \alpha_1 Ineligibility_{it} + \sum_{\tau=0}^{\bar{\tau}} [\theta_{\tau} Over_{it-\tau}^* + f\{(1 - Over_{it-\tau}^*) \times \widehat{CDR}_{it-\tau}^* \gamma_{0\tau}\}] + f\{Over_{it-\tau}^* \times (1 - Over_{it-\tau}^{**}) \times \widehat{CDR}_{it-\tau}^{**} \gamma_{1\tau}\} + f\{Over_{it-\tau}^* \times \widehat{CDR}_{it-\tau}^{**} \gamma_{2\tau}\} + \phi_1 (Over_{it}^* \times Over_{it-1}^*) + \phi_2 (Over_{it-1}^* \times Over_{it-2}^*) + d_t + d_i + u_{it} \tag{5}$$

⁵ For ease, I use a bin size equal to 1 percentage point in the graphs, but using the recommended bin size based on McCrary (2008) of 0.92 percentage points leads to the same conclusion.

(A) All Institutions, All Years



(B) All Institutions, By Year

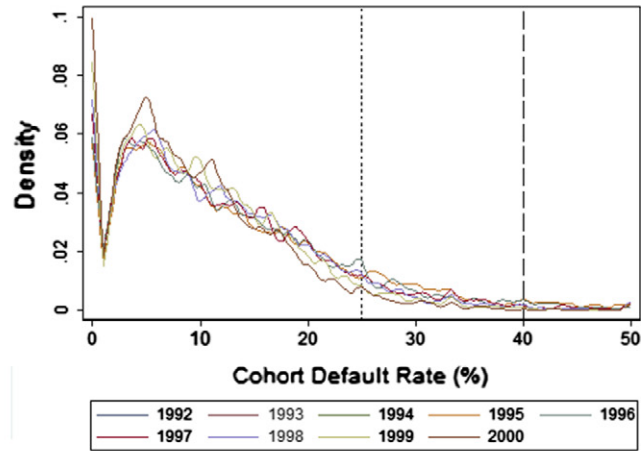
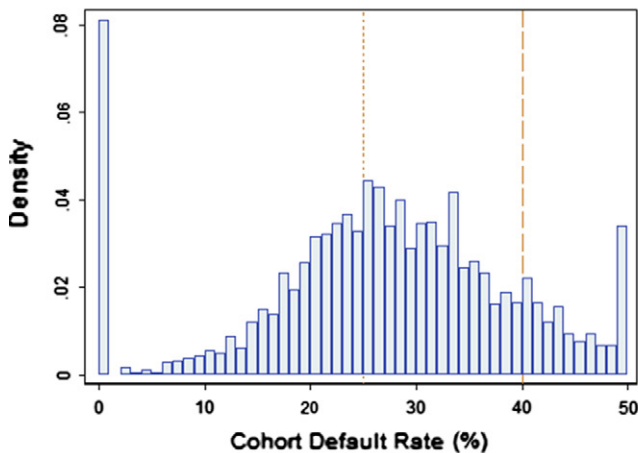
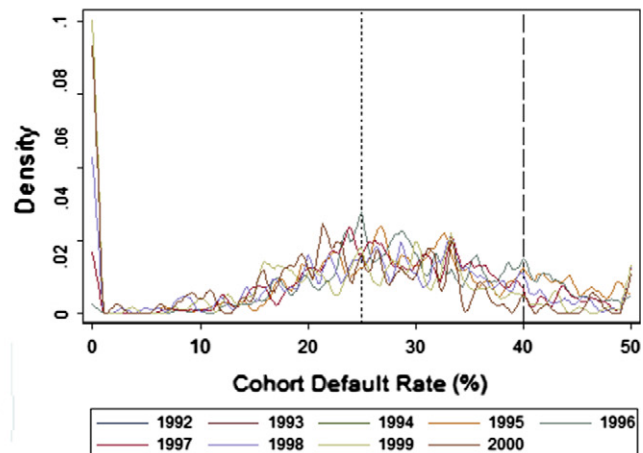
(C) Institutions with $\geq 25\%$ CDR in 2 Prior Years, All Years(D) Institutions with $\geq 25\%$ CDR in 2 Prior Years, By Year

Fig. 1. Cohort default rate distribution. Notes: Histograms use one percentage point bins. Kernel density plots by year using a triangular kernel and a bandwidth of one percentage point. Default rate data censored at 50%. Source: cohort default rate data.

where I add to Eq. (4) indicators for the institution having a CDR over the c^* threshold in the current year and prior year, $(Over_{i,t}^* \times Over_{i,t-1}^*)$, and for the institution having a CDR over the c^* threshold in prior year and two years prior, $(Over_{i,t-1}^* \times Over_{i,t-2}^*)$, with parameters ϕ_1 and ϕ_2 .

The added terms in Eq. (5) distinguish institutions that have exceeded the allowed thresholds for two years in a row and are therefore most at risk for losing Title IV eligibility for violating the multiple year CDR thresholds. The estimated parameters ϕ_1 and ϕ_2 represent expectations effects – the change in enrollment and other outcomes in response to expected or feared ineligibility. Being over the c^* threshold for only one of the prior years may also change behavior, but likely at a lower magnitude than for failing in two consecutive years. The expectations effect is $\omega = [\theta_0 + \theta_1 + \theta_2 + \phi_1 + \phi_2]$ and I test for the joint statistical significance of this effect (i.e., $\theta_0 = \theta_1 = \theta_2 = \phi_1 = \phi_2 = 0$).

These terms encapsulate both supply and demand effects. The risk of sanction could change enrollment behavior by risk-averse students who will avoid, or transfer away from, institutions where there is a danger of no longer being able to obtain federal financial aid. The terms will also capture any actions taken by institutions themselves. The ED directly notifies institutions of their CDRs, so schools should be well aware of potential sanctions. CDRs are publicly available by the ED; however, the extent to which students are aware of CDRs, their implications, or potential sanction at institutions is unclear.

5. Data

The source of institution-level data is the Integrated Postsecondary Education Data System. The ED requires that all Title IV eligible institutions report data to IPEDS, while non-Title IV eligible schools may choose to report voluntarily.⁶ Voluntary reporting of data to IPEDS, even when the institution is not Title IV eligible, may bring forth concerns about selection bias. Schools that choose to report to IPEDS, even when not compelled to do so, however, are likely to be more similar to Title IV eligible schools than schools that choose not to report. As well, the voluntary reporting may reflect intentions to comply with Title IV fiduciary and administrative requirements and therefore may be more likely to apply for eligibility in the future. Any selection bias due to voluntary reporting of aid-ineligible institutions would likely attenuate estimated differences between schools that lose Title IV eligibility and others.

I obtained CDR data from the ED, including official cohort default rates for all Title IV eligible schools and voluntary reporters spanning

⁶ In the analysis data sample, while there is some attrition from the sample after a school loses Title IV eligibility, the rate at which schools leave the sample after losing eligibility does not appear to be much different than the rate of overall attrition of institutions in the sample. For example, in the for-profit ≤ 2 year institution sector, the yearly attrition rate is approximately 2.2%–2.5% for non-sanctioned schools and schools subject to sanction (within three years of being subject to sanction because of CDR threshold violations).

Table 2
Sample summary statistics.

Sector	# of institutions	# of sanctioned institutions	Average total enrollment	Average completions	Average % minority	Average % part-time
All public institutions	1748	118	6582	444	26%	45%
Public 4-yr	553	3	10,264	871	27%	28%
Public \leq 2-yr	1195	115	4712	227	25%	53%
All private, non-profit institutions	1664	48	1893	200	24%	24%
Private, non-profit 4-yr	1372	22	2159	225	24%	25%
Private, non-profit \leq 2-yr	292	26	361	58	28%	18%
All for-profit institutions	1733	507	285	60	39%	18%
For-profit 4-yr	102	6	1128	164	35%	20%
For-profit \leq 2-yr	1631	501	221	52	39%	18%

Source: IPEDS and cohort default rate data.

1990 to 2000. I also received a list of the institutions in each year that are subject to loss of Title IV eligibility at the time CDRs are released, indicating they violated at least one of the CDR requirements. During the period of the study, CDRs were calculated and distributed on a two year lag. For example, CDRs based on the 1992 cohort were calculated and distributed in 1994. Therefore, for each year's CDR, I use the CDR that was calculated and distributed by the ED in that year, based on that cohort's student loan default performance of two years prior. In order to use lagged information on CDRs in the DRD, I focus on outcomes for institutions that were potentially subject to sanction based on 1992–2000 cohort CDR performance. I merge the institution-level IPEDS data with CDR data, and exclude institution-years missing enrollment, student body composition, institutional characteristics, or CDR data. I consider reported enrollment that is over ten times larger than the prior year to be a data error.⁷

In Table 2, I list summary statistics for institutions in the sample by institution sector. For-profit institutions with programs of two years or less comprise the largest group of institutions subject to sanction, with more sanctioned schools than the other sectors combined. As such, the results from this study are largely applicable to this group of institutions. Included in this group are institutions that offer Associates degrees, but also certificates and professional training, such as business colleges, trade/vocational colleges, and fashion and culinary institutes. The analysis sample also includes a large number of sanctioned public institutions with programs of two years or less. Because these schools have relatively large enrollment, consequences at these schools may affect a substantial number of students. Few four-year institutions in any sector and few private, non-profit institutions are subject to sanction during the analysis period. Minority students make up the relatively largest proportion of student bodies at for-profit institutions, with the average student bodies at these schools being comprised of almost 40% minority students. Public \leq 2-year institutions have the highest proportion of part-time students, with over half of students in the sample attending part-time.

Certain characteristics of the data do not allow perfect assignment into Title IV eligible and ineligible groups. As a result of the associated measurement error, the observed ITT effects of losing Title IV eligibility on institution outcomes based on the available data are likely smaller than TOT effects. First, in the years analyzed in this study, the available data do not allow me to identify Title IV loss for any reasons except because of loan cohort default rates. Schools that change status, such as merging with another school, will drop out of the sample, but institutions that lose access to Title IV funds for other reasons may remain. If these institutions exist in the data, they would represent Title IV

ineligible schools included in the eligible control group, and as a consequence would underestimate results. Second, it is important to note that the identifier I have available for sanctioned schools is comprised of institutions pre-appeal. Schools can appeal sanctions on grounds such as erroneous data or demonstration that the school is serving a high number of low-income students who achieved high completion rates.

I do not have information regarding when/if a school successfully re-applies for Title IV eligibility and assume that institutions that are subject to sanction remain without access to these funds through the end of the time period. As such, some schools may have been subject to sanction, but remained Title IV eligible. In addition, if schools appeal their penalty, but are denied, this could also affect the timing of sanction. Since schools remain eligible during the period of appeal, schools that unsuccessfully appeal may actually lose eligibility in years after originally being informed of their sanction status. Schools that are subject to sanction and successfully appeal may still experience some enrollment or student body composition effects, to the extent that the information of being subject to sanction affects applicants' or current enrollees' behavior. Since Title IV eligible schools being included in the ineligible treatment group and ineligible schools being included in the control group will bias observed effects toward zero, I consider results derived from these data to be a lower bound.

6. Results

Based on the volume of sanctioned institutions, I focus the analysis on four non-mutually exclusive sectors: (1) for-profit institutions that offer programs of two years or less; (2) all for-profit institutions; (3) public institutions that offer programs of two years or less; and (4) all institutions that offer programs of two years or less (including public, private non-profit, and for-profit institutions). I provide graphical depictions of the relationship between the running variable CDR and student enrollment (in Fig. 2) and completions (in Fig. 3) for the three groups for which I find effects of Title IV loss in estimations (for-profit \leq 2-years, all for-profits, and all \leq 2-years). In panel A of both figures, for institutions that exceeded the 25% CDR threshold in the prior two years, I fit local linear regression estimates of the natural log of enrollment and completions on CDR, estimated separately for the ranges of [0,25) and [25,40]. In panel B, I fit local linear regression estimates of the natural log of enrollment and completions on CDR, estimated separately for the ranges of (20, 40] and (40, 60). The graphs consistently reveal discontinuities at the 25% cutoff for those institutions that have exceeded the threshold in the prior years, and at the 40% threshold for for-profit \leq 2-year institutions and all for-profits, where enrollment and completions drop once crossing over the threshold. When considering both outcomes for all \leq 2-year institutions at the 40% threshold, however, the graphs reveal the opposite relationship. There is little graphical evidence for changes in discontinuity for the proportion of students who are minority or attend school part-time; therefore the graphs are not included. Note that the preferred estimations include an institution fixed effect, such that some differences may be observed between the graphs and DRD results.

⁷ I test the sensitivity of this assumption and find similar results when excluding growth rates ranging from 500 to 1500%. The analysis data sample is conditional on reporting data in IPEDS and having a CDR. Missing data for the primary outcome of interest (enrollment) from the sample of institutions in IPEDS with CDR data for the relevant time period results in drops of the following number of institutions for each sector analyzed: 48 from the for-profit \leq 2-year institution group, 61 from the all for-profit group, and 107 from the all \leq 2-year institution group. These counts include institutions that overlap among the different sectors.

A: 25% Cutoff, Institutions with $\geq 25\%$ CDR in 2 Prior Years

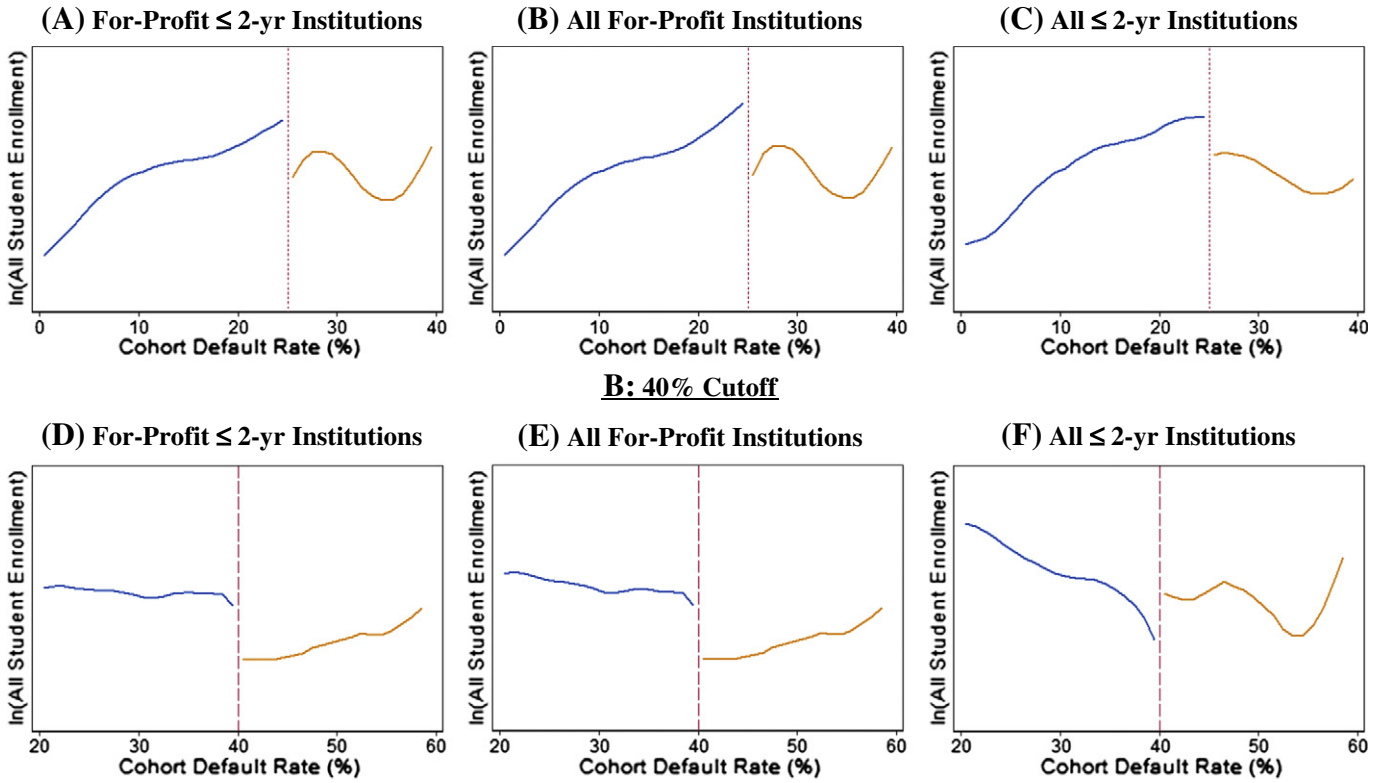


Fig. 2. Graphical discontinuities: ln(Enrollment). Note: Solid lines represent a local linear regression based on average per one percentage point CDR bin.

A: 25% Cutoff, Institutions with $\geq 25\%$ CDR in 2 Prior Years

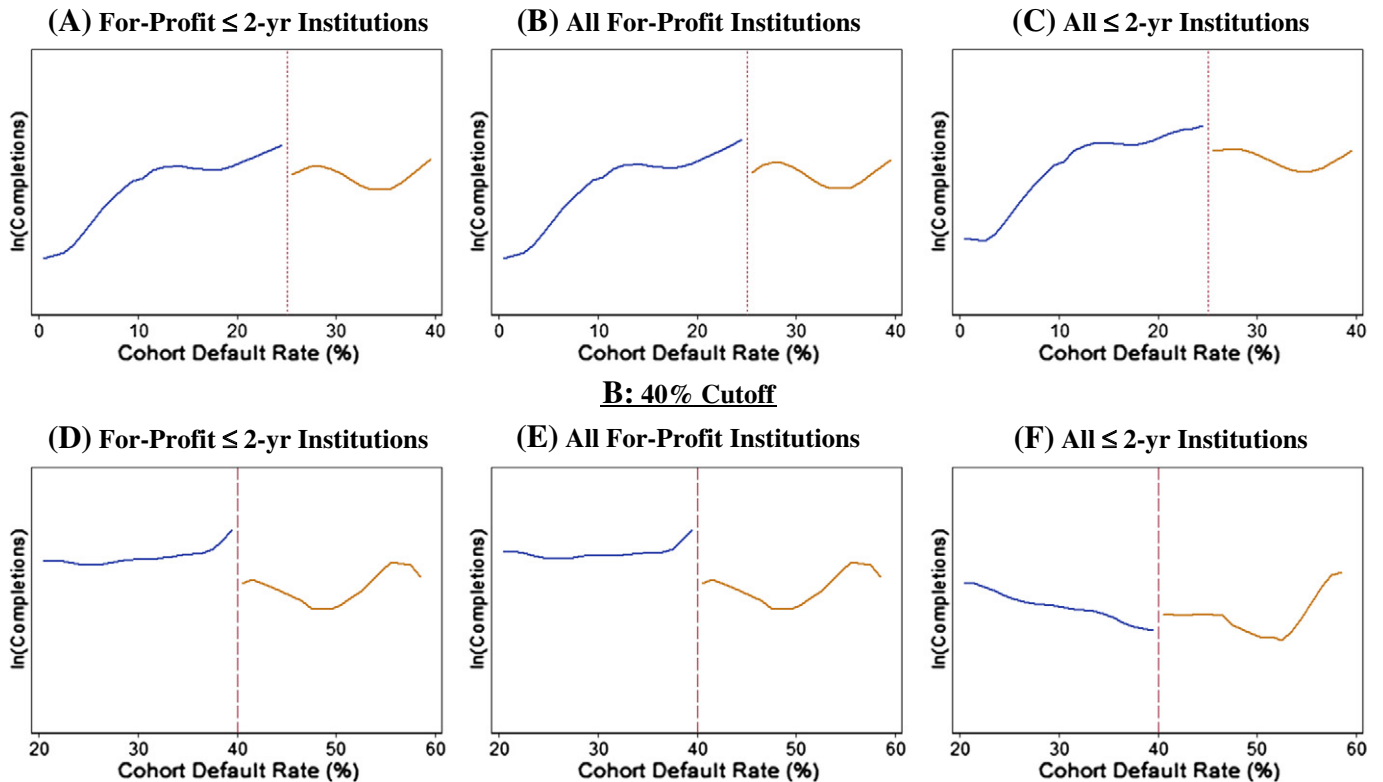


Fig. 3. Graphical Discontinuities: ln(Completions). Note: Solid lines represent a local linear regression based on average per one percentage point CDR bin.

Table 3
Ordinary least squares estimates of the effect of Title IV loss.

	ln(Enrollment) (1)	ln(Completions) (2)	% Minority (3)	% Part-time (4)
<i>Panel A: For-profit ≤2-yr institutions</i>				
Subject to loss – joint thresholds	–0.170* (0.090)	–0.152* (0.090)	–0.024 (0.017)	0.026 (0.018)
Subject to loss – 3 × 25% threshold	–0.136* (0.077)	–0.111 (0.095)	–0.009 (0.015)	0.020 (0.021)
Subject to loss – 40% threshold	–0.223 (0.188)	–0.099 (0.128)	–0.059 (0.040)	–0.002 (0.038)
Observations	6911	6911	6911	6911
Number of institutions	1631	1631	1631	1631
<i>Panel B: All for-profit institutions</i>				
Subject to loss – joint thresholds	–0.177** (0.087)	–0.154* (0.086)	–0.021 (0.017)	0.030* (0.018)
Subject to loss – 3 × 25% threshold	–0.146* (0.075)	–0.115 (0.091)	–0.006 (0.014)	0.025 (0.020)
Subject to loss – 40% threshold	–0.233 (0.188)	–0.103 (0.128)	–0.060 (0.040)	–0.004 (0.038)
Observations	7430	7430	7430	7430
Number of institutions	1733	1733	1733	1733
<i>Panel C: Public ≤2-yr institutions</i>				
Subject to loss – joint thresholds	–0.025 (0.094)	–0.016 (0.171)	0.000 (0.005)	–0.003 (0.022)
Subject to loss – 3 × 25% threshold	–0.034 (0.101)	0.115 (0.137)	–0.000 (0.006)	–0.014 (0.018)
Subject to loss – 40% threshold	–0.072 (0.232)	0.153 (0.402)	–0.005 (0.006)	0.038 (0.043)
Observations	6289	6289	6289	6289
Number of institutions	1195	1195	1195	1195
<i>Panel D: All ≤2-yr institutions</i>				
Subject to loss – joint thresholds	–0.123* (0.067)	–0.094 (0.076)	–0.016 (0.012)	0.016 (0.014)
Subject to loss – 3 × 25% threshold	–0.102* (0.060)	–0.045 (0.076)	0.001 (0.012)	0.013 (0.016)
Subject to loss – 40% threshold	–0.132 (0.132)	0.004 (0.133)	–0.040 (0.025)	0.010 (0.027)
Observations	14,540	14,540	14,540	14,540
Number of institutions	3118	3118	3118	3118

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include institution fixed effects and controls for year. Source: IPEDS and cohort default rate data.

* Significant at 10%.
** Significant at 5%.

6.1. Estimations

Table 3 presents results from the estimates of the effect of Title IV loss on the various outcomes from OLS models with institution fixed effects. Estimates for public ≤2-year institutions in panel C are not statistically significant for any of the outcomes. I find that both sectors of for-profit institutions that are subject to Title IV ineligibility have approximately 17–18% lower yearly fall enrollment than Title IV eligible institutions (column 1, panels A & B). Decline for all ≤2-year institutions is lower, with estimates of just over 12% (panel D). I also observe lower average for-profit completions for both sectors after Title IV loss of approximately 15%, but do not find a statistically significant result for all ≤2-year institutions. Point estimates for the proportion of minorities are consistently negative, while the inverse is true for the proportion of part-time students. Coefficients, however, are generally not statistically significant with the exception of a three percentage point increase in the proportion of part-time students for all-for profit institutions (column 4, panel B). While empirical issues, as discussed in Section 4, prevent causal inference from OLS estimates, the consistency of OLS and DRD results discussed hereafter may provide some support for the relevance of these findings beyond the typically local nature of RD inference.

In Table 4, I display results from the DRD model for for-profit ≤2-year institutions. The effects of Title IV loss on enrollment are displayed in column 1. With joint thresholds (panel A) and separate

thresholds (panel B), I find large negative, but imprecise, point estimates ranging from approximately 11 to 25%.⁸ When including expectations (panel C), the estimated decline in enrollment due to Title IV loss is almost 17% and significant at the 10% level. In panels D and E, I estimate time-varying effects, with the immediate effect showing the consequences of Title IV loss in the first two years immediately after becoming ineligible, and the continued effect being outcomes thereafter. These estimates indicate that the medium-to-longer term impact of Title IV loss is more drastic than the immediate effect, with a statistically significant 22% decline for the former as opposed to a 15% statistically insignificant point estimate for the latter. This could be due to the delay in timing for students to find out that a school has been sanctioned and respond to associated penalties, or schools that appeal sanctions may be able to delay eligibility loss (discussed previously in the data section). Consequences might be delayed because of differences between the timing when students make decisions about where they will attend and when they learn about federal financial aid awards. The timing of effects could reflect the behavior of students who are already on campus or have already made the decision to go to a certain school being less likely to adjust these choices. Whatever the reasons

⁸ I note that when estimates are weighted by enrollment, some estimates are smaller than observed in unweighted estimates. This suggests that institutions with lower enrollment may be experiencing the most sizable effects of Title IV loss.

Table 4
For-profit institutions, dynamic RD estimates of the effect of Title IV loss.

	ln(Enrollment)	ln(Completions)	% Minority	% Part-time
	(1)	(2)	(3)	(4)
<i>Panel A: Total time period</i>				
Subject to loss – joint thresholds	–0.153 (0.098)	–0.196** (0.095)	–0.029 (0.018)	0.017 (0.020)
<i>Panel B: Total time period, separate thresholds</i>				
Subject to loss – 3 × 25% threshold	–0.109 (0.082)	–0.146 (0.099)	–0.013 (0.015)	0.011 (0.021)
Subject to loss – 40% threshold	–0.252 (0.202)	–0.095 (0.142)	–0.074* (0.042)	–0.005 (0.042)
<i>Panel C: Total time period, with expectations effect</i>				
Subject to loss – joint thresholds	–0.166* (0.100)	–0.216** (0.096)	–0.027 (0.019)	0.017 (0.021)
CDR _t > 25%	0.022 (0.054)	–0.000 (0.082)	–0.006 (0.013)	0.019 (0.017)
CDR _{t-1} > 25%	–0.034 (0.048)	–0.030 (0.076)	0.002 (0.013)	0.005 (0.015)
CDR _{t-2} > 25%	–0.020 (0.039)	0.063 (0.066)	–0.002 (0.010)	0.031** (0.013)
CDR _{t-1} > 25% & CDR _{t-2} > 25%	0.048 (0.041)	0.073 (0.060)	–0.012 (0.011)	–0.001 (0.013)
CDR _t > 25% & CDR _{t-1} > 25%	–0.006 (0.036)	–0.018 (0.063)	0.020* (0.011)	0.010 (0.013)
Expectations effect joint significance	0.82	0.53	0.12	0.14
<i>Panel D: Immediate effect</i>				
Subject to loss – joint thresholds	–0.147 (0.097)	–0.197** (0.094)	–0.028 (0.018)	0.017 (0.020)
<i>Panel E: Continued effect</i>				
Subject to loss – joint thresholds	–0.217** (0.106)	–0.213* (0.118)	–0.041* (0.021)	0.028 (0.023)
Observations	6911	6911	6911	6911
Number of institutions	1631	1631	1631	1631

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.

* Significant at 10%.
** Significant at 5%.

for the delayed response, the estimate in panel E indicates that the impact, when it occurs, is sizable.

Consider next the effects of Title IV loss on completions, as displayed in column 2. The estimate with combined thresholds (panel A) indicates that Title IV loss results in a statistically significant 20% decline in for-profit ≤2-year institution completions. Estimates of the effects of the separate thresholds (panel B) are imprecise. The immediate and continued effects are quite similar (20 and 21% respectively) to the overall effect in panel A.

Turning to the effects of Title IV ineligibility on student body composition in columns 3 and 4, I find little conclusive evidence that Title IV loss affects the proportion of students that are minority or part-time. While point estimates for percentage minority are all negative, I find only two statistically significant results. First, losing Title IV eligibility has a continued effect of a lower proportion of minority students of approximately four percentage points (column 3, panel E) and that violation of the 40% threshold leads to a seven percentage point decline in minority student body composition (column 3, panel B). Point estimates for the proportion of part-time students are all positive, but are not statistically significant. Across all outcomes in the three sectors in Tables 4–7, the joint tests of significance for the expectations measures are statistically insignificant, such that I find no evidence that students and institutions substantially altered behavior based on the expectation or risk of sanction.

Table 5 includes estimates for all for-profit institutions. Because of the large proportion of for-profit institutions that are two years or less in the sample, results are similar to Table 4. Here, however, I observe a

Table 5
All for-profit institutions, dynamic RD estimates of the effect of Title IV loss.

	ln(Enrollment)	ln(Completions)	% Minority	% Part-time
	(1)	(2)	(3)	(4)
<i>Panel A: Total time period</i>				
Subject to loss – joint thresholds	–0.163* (0.095)	–0.196** (0.090)	–0.026 (0.017)	0.024 (0.020)
<i>Panel B: Total time period, separate thresholds</i>				
Subject to loss – 3 × 25% threshold	–0.120 (0.079)	–0.149 (0.094)	–0.010 (0.015)	0.017 (0.021)
Subject to loss – 40% threshold	–0.262 (0.202)	–0.104 (0.141)	–0.075* (0.042)	–0.005 (0.042)
<i>Panel C: Total time period, with expectations effect</i>				
Subject to loss – joint thresholds	–0.179* (0.097)	–0.216** (0.091)	–0.023 (0.018)	0.025 (0.021)
CDR _t > 25%	0.024 (0.052)	–0.005 (0.079)	–0.008 (0.013)	0.022 (0.017)
CDR _{t-1} > 25%	–0.041 (0.046)	–0.037 (0.073)	0.001 (0.012)	0.008 (0.015)
CDR _{t-2} > 25%	–0.020 (0.038)	0.062 (0.062)	–0.001 (0.009)	0.029** (0.013)
CDR _{t-1} > 25% & CDR _{t-2} > 25%	0.057 (0.040)	0.071 (0.058)	–0.012 (0.011)	–0.003 (0.013)
CDR _t > 25% & CDR _{t-1} > 25%	0.001 (0.036)	–0.007 (0.061)	0.019* (0.011)	0.007 (0.013)
Expectations effect joint significance	0.71	0.53	0.16	0.16
<i>Panel D: Immediate effect</i>				
Subject to loss – joint thresholds	–0.156* (0.094)	–0.197** (0.090)	–0.024 (0.017)	0.023 (0.020)
<i>Panel E: Continued effect</i>				
Subject to loss – joint thresholds	–0.234** (0.104)	–0.222** (0.113)	–0.037* (0.021)	0.037 (0.023)
Observations	7430	7430	7430	7430
Number of institutions	1733	1733	1733	1733

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.

* Significant at 10%.
** Significant at 5%.

statistically significant 16% decline in enrollment after Title IV loss for the total time period (column 1, panel A). There is a larger continued effect than for just ≤2-year for-profit institutions, with over 23% enrollment decline (column 1, panel E). As well, I find that Title IV loss results in an approximately 20% decline in completion for all for-profits, with a continued effect of over 22% (column 2). Student body composition outcomes for all for-profits are largely similar to those discussed in Table 4 for just for-profit ≤2-year institutions.

I observe no statistically significant results for any of the public ≤2-year institutions outcomes, displayed in Table 6. Enrollment effects in column 1 are negative, but of a smaller magnitude than effects from the for-profit sectors and imprecisely estimated. These results suggest that enrollment and completions at public institutions are less affected by Title IV loss than at for-profit schools. One explanation is that public institution prices are likely lower on average than for-profit prices, such that federal aid loss may be less harmful at these less costly institutions.

Effects of Title IV loss are attenuated when considering all ≤2-year institutions, as listed in Table 7, compared to for-profit institutions. This reflects the inclusion of public institutions in the analysis sample, where I find no statistically significant effect of being subject to Title IV loss. Results suggest that Title IV loss leads to a 12% decline in enrollment, but I no longer find a statistically significant decline in completions in the preferred model (panel A in columns 1 and 2). The continued effect for enrollment suggests a 16% decline, but I do not find a statistically significant immediate decline. When adding consideration of expectations to the model, I find that Title IV loss

Table 6
Public ≤2-yr institutions, dynamic RD estimates of the effect of Title IV loss.

	ln(Enrollment)	ln(Completions)	% Minority	% Part-time
	(1)	(2)	(3)	(4)
<i>Panel A: Total time period</i>				
Subject to loss – joint thresholds	-0.090 (0.102)	-0.013 (0.165)	-0.000 (0.007)	-0.006 (0.024)
<i>Panel B: Total time period, separate thresholds</i>				
Subject to loss – 3 × 25% threshold	-0.052 (0.114)	0.149 (0.144)	0.001 (0.007)	-0.013 (0.021)
Subject to loss – 40% threshold	-0.279 (0.251)	0.176 (0.433)	-0.012 (0.010)	0.027 (0.050)
<i>Panel C: Total time period, with expectations effect</i>				
Subject to loss – joint thresholds	-0.098 (0.105)	-0.022 (0.167)	0.001 (0.007)	-0.009 (0.025)
CDR _t > 25%	-0.090 (0.055)	-0.060 (0.071)	-0.001 (0.009)	-0.000 (0.016)
CDR _t – 1 > 25%	-0.067 (0.050)	-0.059 (0.070)	0.000 (0.007)	-0.014 (0.015)
CDR _t – 2 > 25%	-0.012 (0.038)	-0.044 (0.049)	-0.000 (0.005)	-0.005 (0.012)
CDR _t – 1 > 25% & CDR _t – 2 > 25%	0.026 (0.048)	0.029 (0.062)	-0.006 (0.006)	0.012 (0.015)
CDR _t > 25% & CDR _t – 1 > 25%	0.028 (0.054)	0.077 (0.068)	-0.010 (0.007)	-0.020 (0.015)
Expectations effect joint significance	0.60	0.87	0.60	0.39
<i>Panel D: Immediate effect</i>				
Subject to loss – joint thresholds	-0.087 (0.103)	-0.021 (0.165)	0.001 (0.006)	-0.008 (0.024)
<i>Panel E: Continued effect</i>				
Subject to loss – joint thresholds	-0.109 (0.119)	0.008 (0.176)	0.003 (0.008)	-0.002 (0.031)
Observations	6289	6289	6289	6289
Number of institutions	1195	1195	1195	1195

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.

is followed by an annual 13% decline in completions, but do not find a statistically significant coefficient when examining the immediate or continued timeframes. I observe in column 3 a two percentage point decline in minority student body composition due to Title IV loss, which appears to be driven by the medium-to-long term decline in minority student attendance (panels A and E).

6.2. First year vs. continuing students

Losing Title IV funds could have differential effects for new students as compared to continuing students. Already enrolled students, especially those who have completed a large amount of coursework or are close to gaining their degree, even if they have substantial need for financial aid, may figure out ways to stay at the institution rather than start over elsewhere or abandon studies altogether. Alternatively, students who have not yet enrolled may have no loyalty to institutions they are evaluating for entry or may not have a connection to the campus community that allows the institution to elucidate offsetting benefits. As such, faced with a lack of financial aid funds at the school they otherwise would have attended, new students may be more likely to enroll at another institution or forgo postsecondary education altogether.

In an effort to understand these effects, I separately examine the enrollment and student body composition effects of Title IV loss for first year students as compared to continuing students. Taken together, the differing results for first year and continuing students provide evidence that students of varying levels respond uniquely to Title IV loss. I note that first year students in these data may also include continuing students who have not yet advanced to higher levels, though I expect this

Table 7
All ≤2-yr institutions, dynamic RD estimates of the effect of Title IV loss.

	ln(Enrollment)	ln(Completions)	% Minority	% Part-time
	(1)	(2)	(3)	(4)
<i>Panel A: Total time period</i>				
Subject to loss – joint thresholds	-0.120* (0.072)	-0.114 (0.077)	-0.021* (0.013)	0.010 (0.015)
<i>Panel B: Total time period, separate thresholds</i>				
Subject to loss – 3 × 25% threshold	-0.087 (0.064)	-0.062 (0.079)	-0.002 (0.013)	0.007 (0.016)
Subject to loss – 40% threshold	-0.180 (0.144)	0.016 (0.144)	-0.057** (0.027)	0.002 (0.030)
<i>Panel C: Total time period, with expectations effect</i>				
Subject to loss – joint thresholds	-0.126* (0.073)	-0.128* (0.078)	-0.019 (0.013)	0.007 (0.016)
CDR _t > 25%	-0.010 (0.038)	-0.008 (0.056)	-0.003 (0.009)	0.012 (0.012)
CDR _t – 1 > 25%	-0.022 (0.033)	-0.021 (0.051)	0.004 (0.008)	-0.002 (0.010)
CDR _t – 2 > 25%	-0.010 (0.026)	0.034 (0.043)	-0.002 (0.006)	0.015* (0.009)
CDR _t – 1 > 25% & CDR _t – 2 > 25%	0.022 (0.030)	0.051 (0.044)	-0.008 (0.008)	0.008 (0.010)
CDR _t > 25% & CDR _t – 1 > 25%	-0.003 (0.029)	0.009 (0.046)	0.010 (0.008)	0.001 (0.010)
Expectations effect joint significance	0.97	0.73	0.43	0.31
<i>Panel D: Immediate effect</i>				
Subject to loss – joint thresholds	-0.115 (0.072)	-0.118 (0.077)	-0.019 (0.013)	0.009 (0.015)
<i>Panel E: Continued effect</i>				
Subject to loss – joint thresholds	-0.162** (0.079)	-0.099 (0.092)	-0.025* (0.015)	0.021 (0.017)
Observations	14,540	14,540	14,540	14,540
Number of institutions	3118	3118	3118	3118

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.

* Significant at 10%.
** Significant at 5%.

number to be small relative to new students. As well, first year student counts include students who enroll in academic programs that only last for one year, such as many certificate programs. In this study, I term all students with standing beyond the first year as continuing students, and note that these counts can include advanced standing students who transfer in from other institutions.

I display results from the DRD model in Table 8. The enrollment effect for first year students is rather different than for continuing students.⁹ I find large and statistically significant negative effects of Title IV ineligibility on enrollment for first year students at for-profit institutions, with sanctioned institutions experiencing a decline of approximately 17–19% overall driven by a continued effect of 23–25% (column 1, panels A and B). These coefficients are larger than those of the advanced standing students, as well as the pooled group of all students. With an average annual first year enrollment of 150 at these sanctioned for-profit schools, this decline in first year enrollment translates to a per school average of 25–38 students annually. Conversely, I find no statistically significant effects for continuing students (column 2, panels A and B). In panels C and D, I find statistically insignificant results for both first year and advanced standing students. These results suggest that for-profit institutions lose new enrollees because of Title IV loss, but do not experience an increase in transfers out or dropouts

⁹ There are some differences in sample size among advanced standing and first-year students due to reporting by institutions.

of existing students relative to Title IV eligible schools. The lack of conclusive negative effects for advanced standing students may be because already enrolled students are unlikely to abandon an already started program, or because institutions are able to offer more attractive offsetting benefits to students with more advanced standing.

While point estimates are always positive, the only significant results for the proportion of part-time students after losing Title IV are the continued effect on first year students at for-profits (five percentage points in column 5, panel B) and all ≤ 2 -year institutions (four percentage points in column 5, panel C). An interpretation of this result is that entering students in the medium-to-long term could be increasing their paid work activity because of the lack of available federal financial aid.

6.3. Falsification tests and bandwidth sensitivity

I include in Table 9 a falsification check of the identification strategy for the multiple year 25% threshold. Using the DRD model, I estimate the effect of an institution passing the 25% threshold, without having passed it in the two prior years. A significant coefficient for the “false treatment” indicator would be cause for question as to whether the observed relationships in the primary analysis are spurious. For all sectors and outcomes examined in this study, I find no evidence of discontinuous outcomes based on this falsification test. As such, the lack of statistically significant coefficients in this table provides some support for the veracity of the effects observed in prior sections.

Additionally, to test the sensitivity of results to choice of bandwidth and order of polynomials, I include in the online Appendix DRD estimates

of the effect of Title IV loss for for-profit ≤ 2 -year institutions based on Lee and Lemieux (2010). Similar tables for the other two groups of institutions are available in an online appendix. Narrower bandwidth choices may yield less biased estimates, but at the sacrifice of precision. In this table, I display results for what I consider to be the primary findings of enrollment and completion estimates based on a range of bandwidths ($\widehat{CDR}_t \pm 0.05, 0.10, 0.25, 0.50, 1.00$) and orders of polynomials (0, 1, 2, 3, 4, 5). In estimations of enrollment with narrower bandwidths (panel A), estimated enrollment effects, though not always statistically significant at traditional levels, are always directionally similar to the estimations using all institutions and are almost always larger than my preferred estimates including all institutions for all three sectors analyzed. The results for completions, however, suggest that observed results are not robust for the smallest bandwidth for the for-profit ≤ 2 -year and all for-profit sectors. Therefore, the results indicating declines in completions because of Title IV loss should be regarded with some caution.

7. Summary and conclusions

This study is among the first to assess the causal impact of institution federal aid funding loss on aggregate student outcomes. Using Title IV regulations, I implement a quasi-experimental dynamic regression discontinuity design based on student loan repayment rates. If former students default on education loans at a rate above 40% in one year or 25% in three consecutive years, institutions are subject to loss of the ability to disburse federal financial aid, such as Pell Grants and subsidized

Table 8
DRD estimates of the continued effect of Title IV Loss, 1st year vs. advanced students.

	ln(Enrollment)		% Minority		% Part-time	
	1st year	Advanced standing	1st year	Advanced standing	1st year	Advanced standing
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: For-profit ≤ 2-yr institutions</i>						
Total time period effect	-0.175*	0.093	-0.027	-0.014	0.021	0.028
	(0.106)	(0.246)	(0.019)	(0.061)	(0.020)	(0.078)
Immediate effect	-0.170	0.071	-0.025	-0.011	0.020	0.029
	(0.106)	(0.250)	(0.019)	(0.061)	(0.020)	(0.078)
Continued effect	-0.229**	0.152	-0.036	-0.032	0.032	0.040
	(0.113)	(0.278)	(0.022)	(0.064)	(0.023)	(0.083)
Number of institutions	1631	1398	1631	1411	1631	1412
<i>Panel B: All for-profit institutions</i>						
Total time period effect	-0.186*	0.034	-0.023	-0.006	0.032	0.043
	(0.103)	(0.213)	(0.018)	(0.051)	(0.020)	(0.068)
Immediate effect	-0.181*	0.017	-0.022	-0.004	0.032	0.042
	(0.102)	(0.214)	(0.018)	(0.051)	(0.020)	(0.068)
Continued effect	-0.249**	0.103	-0.031	-0.023	0.047**	0.056
	(0.110)	(0.244)	(0.021)	(0.055)	(0.024)	(0.076)
Number of institutions	1733	1496	1733	1509	1733	1510
<i>Panel C: Public ≤ 2-yr institutions</i>						
Total time period effect	-0.090	0.142	0.012	-0.019	-0.020	0.018
	(0.113)	(0.180)	(0.008)	(0.018)	(0.027)	(0.022)
Immediate effect	-0.095	0.127	0.013	-0.017	-0.022	0.014
	(0.114)	(0.178)	(0.008)	(0.017)	(0.027)	(0.023)
Continued effect	-0.137	0.294	0.016	-0.022	-0.023	0.037
	(0.140)	(0.217)	(0.011)	(0.025)	(0.033)	(0.026)
Number of institutions	1195	1178	1195	1179	1195	1179
<i>Panel D: All ≤ 2-yr institutions</i>						
Total time period effect	-0.061	0.106	-0.015	-0.018	0.020	0.021
	(0.080)	(0.138)	(0.013)	(0.026)	(0.016)	(0.034)
Immediate effect	-0.068	0.079	-0.014	-0.015	0.017	0.019
	(0.080)	(0.138)	(0.013)	(0.026)	(0.016)	(0.034)
Continued effect	-0.077	0.215	-0.016	-0.027	0.035*	0.031
	(0.088)	(0.161)	(0.016)	(0.029)	(0.018)	(0.037)
Number of institutions	3118	2852	3118	2866	3118	2867

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Source: IPEDS and cohort default rate data.

* Significant at 10%.

** Significant at 5%.

Table 9
Falsification test.

	<u>ln(Enrollment)</u>	<u>ln(Completions)</u>	<u>% Minority</u>	<u>% Part-time</u>
	(1)	(2)	(3)	(4)
<i>Panel A: For-profit ≤2-yr institutions</i>				
>25% CDR (not in 3 consecutive years)	0.056 (0.037)	−0.030 (0.057)	0.011 (0.009)	−0.003 (0.010)
Observations	6911	6911	6911	6911
Number of institutions	1631	1631	1631	1631
<i>Panel B: All for-profit institutions</i>				
>25% CDR (not in 3 consecutive years)	0.048 (0.036)	−0.025 (0.054)	0.010 (0.009)	−0.001 (0.010)
Observations	7430	7430	7430	7430
Number of institutions	1733	1733	1733	1733
<i>Panel C: Public ≤2-yr institutions</i>				
>25% CDR (not in 3 consecutive years)	−0.016 (0.033)	0.058 (0.042)	0.003 (0.006)	−0.004 (0.011)
Observations	6289	6289	6289	6289
Number of institutions	1195	1195	1195	1195
<i>Panel D: All ≤2-yr institutions</i>				
>25% CDR (not in 3 consecutive years)	0.040 (0.025)	0.018 (0.037)	0.010 (0.006)	−0.004 (0.007)
Observations	14,540	14,540	14,540	14,540
Number of institutions	3118	3118	3118	3118

Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include third order polynomials, institution fixed effects, and controls for year. Source: IPEDS and cohort default rate data.

student loans. Institutions local to the cutoff values cannot precisely control their assignment into the eligible (control) and ineligible (treatment) groups, reasonably approximating randomization such that local average treatment effects can be obtained (Lee and Lemieux, 2010). The use of the relatively new DRD design allows me to take into account the time dependent nature of Title IV funding ineligibility, where sanctions are conditional on the cumulative performance of former students' student loan repayment behavior over three years.

The federal government invests heavily to encourage higher education attendance through financial aid programs, with almost \$200 billion in aid and funds used to finance postsecondary students during the 2009–2010 school year (Baum et al., 2010). Controversial program integrity rules limit institutional eligibility to disburse federal aid in an effort to protect both taxpayers and students. Proponents of such rules believe they will limit the number of students with unaffordable debt burdens and improve the value proposition of program offerings (ED, 2011a). As well, the restrictions defend against potential financial aid fraud by “diploma mill” schools that provide little value in return for relatively high costs, especially since a large portion of these costs are often paid by publicly funded financial aid.

Regulations restricting Title IV funding because of former students' failure to repay student loans appear to be an effective policy lever. I find that ineligibility due to violation of CDR thresholds effectively discourages enrollment, with lower overall annual enrollment of approximately 12–16% at ≤2-year institutions. This outcome appears to be largely driven by declines at for-profit institutions, and in particular, declines in lower annual first year student enrollment of approximately 18%. These results suggest that sanctioned schools are generally able to retain students with whom they already have a connection, but have difficulty compensating for the loss of aid among students newly considering their programs. When examining timing, observed medium-to-long term effects are larger than immediate ones. This implies that it takes students a couple of years to respond to information about aid ineligibility at sanctioned institutions, and that these schools may have difficulty providing benefits sufficient enough to offset the costs of federal aid loss. I also find evidence that completions decline almost 20% annually after Title IV loss, but the instability of the results dictates caution when drawing conclusions from these results.

Results from the study indicate that the efforts to strengthen program integrity have important implications for whether students attend higher education, and if so, where. As such, program integrity rulemaking could conflict with broad goals to improve higher education access. Increased regulation that leads to financial aid disbursement ineligibility appears to lead to lower matriculation activity at schools where many low-income, minority, and non-traditional students attend. Estimated enrollment and completion declines at certain institutions, however, do not provide direct evidence on the broader question of whether Title IV loss decreases access to postsecondary education. Enrollment loss at an institution could reflect an overall decrease in postsecondary education participation or the transfer of students to other institutions. There is little empirical evidence of this distribution. However, even if one assumes that only a small percentage of students dropped out of postsecondary education due to Title IV aid disbursement eligibility loss (instead of transferring to another institution), this would still suggest that a large number of students forwent postsecondary education.

Support for increased restrictions based on this type of rulemaking, therefore, requires more nuanced objectives, namely that simply encouraging higher education participation may not be sufficient without consideration of where and how a student attends. Reduced access to institutions with poor student loan repayment histories may be considered acceptable or even preferable if these schools truly provide little in return for the human capital investment. Moreover, if the government deems certain institutions as underserving, reducing enrollment and therefore federal subsidies serves to protect taxpayers by limiting public expenditures at these schools. Decreases in the number of completions are potentially more troubling than enrollment declines, but only if students are being deterred from programs that provide value and these students are not able to complete their studies at other institutions. The definition and identification of institutional quality and performance is critical to answer these questions, and it is worth considering whether student loan default rates are an appropriate measure.

Evidence regarding claims about the disproportionate burden of Title IV loss on disadvantaged groups is more equivocal than evidence on overall enrollment or completion declines. Enrollment appears to be most negatively affected at institutions that are likely to have high proportions of minority and non-traditional students.

However, I do not find conclusive evidence that the student body compositions change substantially at schools that lose Title IV eligibility. Title IV ineligible institutions may respond to Title IV loss by lowering tuition in an attempt to attract students. If some students are less familiar with or less enthusiastic about financial aid options, they could be enticed by lower tuition sticker prices, rather than costs net of financial aid. As a result, some students might choose to enroll or stay in poorly performing schools without access to federal financial aid. This phenomenon could have negative consequences for equity if these institutions are simply commandeering funds and not providing a high-quality education or enhanced labor market prospects. Further research, however, is needed to clearly answer this question.

The sizable enrollment loss, completion declines, and risk that minority and low-income students may be particularly affected by program integrity rules indicate that careful evaluation is needed when considering future, and assessing already implemented, policies that modify Title IV eligibility requirements at postsecondary institutions. For-profit institutions appear to be increasingly targeted by program integrity rules, and this is a sector where student demand is large and growing rapidly. In 2009, this sector comprised nearly 2 million students per year, accounting for over nine percent of total enrollment at degree-granting institutions in the country and the majority of non-degree postsecondary students (Deming et al., 2012). As well, for-profits in recent years have awarded over 15% of Associates degrees in the country (Baum and Payea, 2011). Industry representatives assert that these types of regulations threaten the survival of large numbers of for-profit programs and the institutions themselves (Kirkham, 2011).

Initiatives such as the proposed gainful employment rulemaking suggest that policymakers will likely continue to focus on student loan debt repayment as factor on which to judge which institutions merit disbursing federal financial aid. Future policy action could include revisions of the rules related to CDRs or other related yet to be defined measures of debt. That portions of the gainful employment rule were struck down because certain details and requirements were not based on expert studies reflects the need for evidence of the consequences of rulemaking of this type. By definition, the identification strategy used in this study provides results that may not be relevant to the full population of postsecondary institutions. Findings from the study, however, indicate that program integrity rules are effective federal policy levers and provide evidence about the consequences of losing Title IV eligibility at schools where students have difficulty repaying loan obligations. Research on the labor market returns to education and education quality at institutions that lose Title IV eligibility, the transfer and drop-out behavior of students who attend such schools, and the costs and determinants of default will be particularly important to further evaluate policies addressing student loan defaults at institutions.

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