

## **Integrity versus Access? The Effect of Federal Financial Aid Availability on Postsecondary Enrollment**

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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 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, particularly at for-profit institutions. The decline in enrollment appears to be driven by fewer new enrollees. I find less conclusive evidence that ineligibility to disburse federal financial aid substantially alters student body composition. This research is particularly relevant considering recent 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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## **I. 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 federal financial aid. Recently, spurred by government and media reports of federal aid fraud and abuse among 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 institutions disbursing financial aid federal postsecondary education, including Pell Grants, Stafford Loans, and other sources of aid.

This type of rulemaking, which endeavors to protect students and the integrity of federal aid programs, is not uncommon. In the past 20 years, the ED has implemented a number of policy changes that determine which institutions are allowed to disburse federal aid. The intent of the rules is to safeguard taxpayers and students 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).

In this study, I present the first causal estimates (to my knowledge) of the effects on enrollment and student body composition when postsecondary institutions lose eligibility to disburse federal financial aid. Federal financial aid program integrity regulations aim to protect students by discouraging enrollment at institutions where former students do not or cannot repay student loans. Institutions that lose aid disbursement eligibility may experience decreases in the matriculation of financially and/or geographically constrained students. A key issue is whether students who forgo education at ineligible schools transfer to other institutions or drop out of higher education altogether. Abstaining from higher education might be considered a positive outcome when compared to attending schools that allegedly provide few skills or result in unmanageable debt. Over time, aid disbursement ineligibility may also affect the management and survival of institutions, leading to closure or a dramatic overhaul of operations. Alternatively, if losses in institutional aid eligibility have no impact on enrollments and student body composition, such findings suggest that institutions can compensate for the loss of federal aid or that this aid was not as critical to operations as presented.

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 loans (the yearly cohort default rate or “CDR”) exceeds 40 percent in a single year or 25 percent continuously over three years.

The identification strategy uses these thresholds to implement a dynamic regression discontinuity (“DRD”) design. In the DRD, I identify differences in outcomes by comparing the enrollment and student composition of institutions that are just over the eligibility cutoffs against the outcomes of institutions that are just under the cutoffs. 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 & 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 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 15 percent and lower annual new student enrollment of approximately 25 percent. I do not find strong evidence of increased transfer or drop-out of existing students, suggesting that 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. Reduced enrollment at certain schools might be considered a favorable outcome if schools that fail student loan debt measures 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 find some weak evidence that the composition of student bodies at ineligible schools changes because of loss of eligibility. Specifically, in the reduced first-year student bodies, results suggest that after losing eligibility, sanctioned schools appear to realize an increase in the proportion of minority students. This may give rise to concerns that minority students are less discouraged from enrollment at schools that fail to provide a high-quality education while non-minority students are able to take advantage of financial aid funds at better functioning institutions. As well, in some sectors after loss of federal aid, I observe an increase in the proportion of students who attend part-time.

The paper is organized as follows. Section II provides context on federal financial aid. Section III 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 IV presents identifying assumptions and empirical research design. Sections V and Section VI provide an overview of data and a discussion of results. Section VII concludes.

## **II. 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. Concerns persist that some of this public investment is wasted by generating revenue for certain schools and not contributing to 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. 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”).<sup>1</sup> Table 1 lists of Title IV grant and loan programs, along with related expenditures for the 2009-2010 school year.

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<sup>1</sup> Prior to July 1, 2010, the Federal Family Education Loan (“FFEL”) program allowed private lenders to offer federally subsidized student loans.

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 other reasons are briefly described in the Appendix.

The main empirical identification strategy in this study will examine loss of Title IV eligibility because of high institution-level student loan cohort default rates. 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.<sup>2</sup> Institutions with CDRs greater than or equal to 25 percent for three consecutive years or exceeding 40 percent 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.<sup>3</sup> 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.<sup>4</sup>

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<sup>2</sup> 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.

<sup>3</sup> Starting in 1992, institutions lose Title IV eligibility if they have cohort default rates exceeding 25 percent for 3 consecutive years or 40 percent for 1 year. The thresholds were 35 and 30 percent in for the multiple year cut-off and 45 and 40 percent 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.

<sup>4</sup> 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.

### **III. Consequences of Institutional Aid Loss and Extant Literature**

#### *III.A. Institutional Federal Financial Aid Loss*

##### *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 on for-profit colleges casts some doubt on these claims (Cellini & 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 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, child care, or convenient class schedules. 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 may be at most risk for closure, given their relatively high tuition and reliance on federal financial aid for revenue (Moore, 1995). This sector, however, may also be better suited to offer other benefits to attract financially marginal students, such as career counseling 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 outweighs potential benefits for many students.

##### *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

he undertakes postsecondary education. 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. Demand may also decline because of the harmed reputation of the school that may result from failing eligibility requirements. 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 postsecondary studies at specific institutions; for example, schools that are geographically proximate, offer convenient 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 higher education altogether. Other sources, such as state financial aid programs, may be able to partially offset loss in federal aid. Students might also substitute working or private student loans to make up for the loss of federal aid.

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. Though not based on empirical evidence, the ED's (2011a) regulatory impact analysis of the proposed gainful employment rule assumes that five to 42 percent of students will drop out of higher education if they attend or plan to attend programs that fail debt measures and lose Title IV eligibility, depending on sector.

Enrollment, transfer, and drop-out behavior in response to institution-level financial aid availability may alter the distribution of students across institutions and student body composition at each school, potentially affecting academic experience and performance (e.g., Gurin, Dey, Hurtado, & Gurin, 2002; Zimmerman, 2003). Therefore, in addition to analyzing enrollment volume effects, I also investigate changes in student composition in this study. Lack of financial aid resources at an institution may also 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 & Moller, 2000). Lack of aid availability may also affect decisions on the intensive margin, such that students at ineligible institutions, after deciding to

attend, may decide to borrow higher levels of private student loans or be more likely to decide to work to help fund their education.

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” or gross 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 (Cellini & Goldin, 2012). Financially adept 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 savvy 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 & 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, Long, Oreopoulos, and Sanbonmatsu (2012) provide compelling experimental evidence of this 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.

### *III.B. Prior 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’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 & Thompson, 2010). This rule was ultimately struck down by a US District

Court for not being based on expert studies or industry standards and provides a clear example of the dearth of available research to advise rulemaking of this type.

There is a rather large literature, however, on the individual student-level responses to financial aid. 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 & Brinkman, 1987; Van der Klaauw, 2002), though the evidence regarding specific federal grant and loan programs is less conclusive. With particular relevance for this study, there is mixed evidence that Pell Grant availability encourages enrollment, especially among low-income students (e.g., Curs, Singell, & Waddell, 2007; Hansen, 1983; Kane, 1995; Seftor & Turner, 2002; Turner, 2007). Evidence on the effect of state-level grants appears to more consistently demonstrate that grant programs increase enrollment and can affect the distribution of students across types and location of institutions, though beneficiaries and magnitudes vary by program structure and student demographics (e.g., Cellini, 2010; Cornwell, Mustard, & Sridhar, 2004; Dynarski, 2000; Farrell & Kienzl, 2009; Linsenmeier, Rosen, & Rouse, 2006). More equivocal is the ability for student loans to increase matriculation at higher education institutions. In a review of research, Haskins, Holzer, and Lerman (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., Campaigne & Hossler, 1998; Carneiro & Heckman, 2002; Heller 2008).

#### IV. 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 percent for three consecutive years or exceeding 40 percent 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:

$$(1) \text{Ineligibility}_{it} = \begin{cases} 1 & \text{if } (CDR_{it} \geq c^* \cup CDR_{i(t-1)} \geq c^* \cup CDR_{i(t-2)} \geq c^*) \cap CDR_{it} > c^{**} \\ 0 & \text{if } (CDR_{it} < c^* \cap CDR_{i(t-1)} < c^* \cap CDR_{i(t-2)} < c^*) \cup CDR_{it} \leq c^{**} \end{cases}$$

where  $c^*$  represents the three year default rate cutoff (i.e., 25 percent) and  $c^{**}$  represents the single year default rate cutoff (i.e., 40 percent). The primary outcome I analyze is the effect of

Title IV loss on subsequent enrollment. I also examine 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:

$$(2) \quad y_{it} = \beta_0 + \beta_1 \text{Ineligibility}_{it} + \kappa_{it} + d_t + d_i + \varepsilon_{it}$$

where  $i$  indexes institutions and  $t$  indexes year;  $d_t$  represents fixed effects for year and  $\kappa_{it}$  is the number of years passed since losing eligibility;  $\beta_0$  and  $\beta_1$  are estimated parameters; and  $\varepsilon_{it}$  is an error term.  $\text{Ineligibility}_{it}$  is an indicator for school-level Title IV ineligibility in year,  $t$ , because of violation of either threshold. I estimate the OLS models with and without an institution fixed effect,  $d_i$ . The fixed effect accounts 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.<sup>5</sup> For the OLS models and all the regressions discussed hereafter, heteroskedasticity robust standard errors are clustered by institution.

The parameter  $\beta_1$  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 enrollment/student body composition. Eligibility might be correlated with other school level characteristics that affect the outcome of interest, such that  $E[\varepsilon_{it}] \neq 0$ , yielding biased estimates of  $\beta_1$ . 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.

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<sup>5</sup> In the models without institution fixed effects, I control for institution sector (4-year/2-year/<2-year and public/private, not-for-profit/for-profit) and average enrollment over the eight-year time-period to account for differences among institutions of different sizes. I also include a fixed effect for states to account for differential financial aid programs that may vary across states. Since the institution fixed effect accounts for all factors that do not change over time, I do not display these time invariant factors in equation (2).

#### IV.A. 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  $CDR_i \leq c^{**}$ , 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. Equation (3) depicts a cross-sectional regression discontinuity design:

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

The term,  $Ineligibility_{it}$  indicates whether institution,  $i$ , is eligible to disburse Title IV funds in year,  $t$ ;  $\overline{CDR}_{it}^* = CDR_{it} - c^*$ , the distance between each institution's CDR and threshold  $c^*$  (the 25 percent threshold);  $\overline{CDR}_{it}^{**} = CDR_{it} - c^{**}$ , the distance between each institution's CDR and threshold  $c^{**}$  (the 40 percent threshold);  $i$  indexes institutions and  $t$  indexes year;  $\alpha_0$  and  $\alpha_1$  are parameters; and  $u_{it}$  is an error term. As in equation (2),  $d_t$  and  $\kappa_{it}$  represent fixed effects for year and a control for the number of years passed since losing eligibility respectively. 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.  $f\{(1 - Over_{it}^*) \times \overline{CDR}_{it}^*, \gamma_0\}$  is a vector of polynomial functions of  $\overline{CDR}_{it}^*$  interacted with an indicator for being under the cutoff  $c^*$ , with coefficients  $\gamma_0$ , and controls for relative distance where CDR is less than 25 percent.  $f\{Over_{it}^* \times (1 - Over_{it}^{**}) \times \overline{CDR}_{it}^*, \gamma_1\}$  is a

vector of polynomial functions of  $\overline{CDR}_{it}^*$  interacted with an indicator for being over the cutoff  $c^*$  but under the cutoff  $c^{**}$ , with coefficients  $\gamma_1$ , and controls for relative distance where CDR is greater than or equal to 25 percent, but less than or equal to 40 percent.  $f\{Over_{it}^{**} \times \overline{CDR}_{it}^{**}, \gamma_2\}$  is a vector of polynomial functions of  $\overline{CDR}_{it}^*$  interacted with an indicator for being over the cutoff  $c^{**}$ , with coefficients  $\gamma_2$ , and controls for relative distance where CDR is greater than 40 percent.

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 (i.e.,  $E[y_{0it} | CDR_{it}]$  can be observed when  $CDR_{it} \geq c^*$  if  $CDR_{i,t-1} < c^*$  or  $CDR_{i,t-2} < c^*$ ).

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

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

where  $i$  indexes institution,  $t$  indexes year, and  $\tau$  indexes the number of years prior to eligibility loss, for  $\tau \in [0, 2]$ . Equation (4) includes, for the current and prior two years, indicators being over the eligibility threshold for the multiple year threshold,  $Over_{i,t-\tau}^*$ , as well as polynomial functions and interacted polynomial functions of relative distance between each institution's CDR and the two CDR thresholds as in equation (3). By adding these lagged factors, I control for satisfaction of the condition of needing cohort default rates equal to or above 25 percent 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.<sup>6</sup>

The coefficient on the *Ineligibility<sub>it</sub>* term,  $\alpha_1$ , represents the local average treatment effect. 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. Given perfect capture of ineligibility status,  $\alpha_1$  would represent treatment-on the-treated (“TOT”) effects. However, because of data considerations that may lead to imprecise assignment into eligible and ineligible groups (discussed in the Section V), results are not TOT effects.

### *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 selection, as long as they cannot *precisely* manipulate assignment, RD designs will produce estimates akin to a randomized experiment (Lee & 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

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<sup>6</sup> Based on Lee and Lemieux (2010), I include in Appendix Tables A1-A3 dynamic regression discontinuity estimates of the effect of Title IV loss for a range of bandwidths ( $\widehat{CDR}_{it}^* \pm 0.04, 0.06, 0.10, 0.25, 0.50, 1.00$ ) and orders of polynomials (0, 1, 2, 3, 4, 5). Narrower bandwidth choices may yield less biased estimates, but at the sacrifice of precision. In estimations with narrower bandwidths, estimated enrollment effects are always directionally similar to the estimations using all institutions, though not always statistically significant at traditional levels.

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 left the institution for at least two years, such that any efforts put forth to influence repayment behavior of students would not be measured until at least two years after they finished the program. For example, suppose a two-year college attempted to influence the default behavior by restricting the enrollment of entering students. At the earliest, these students would be counted in CDR calculations for four years and many students (those who do not finish in two years or defer student loan payments) would not be counted for many years later.

To examine for potential manipulation by institutions, I test for density discontinuities around the two cutoffs. Higher densities just below the 25 or 40 percent cutoffs could signify that institutions are able to select their position into the Title IV eligible group. In Figure 1, 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, and, there is no evidence of endogenous sorting of schools just below the default rate thresholds.<sup>7</sup> 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 slightly higher density of schools just above the 25 ( $\hat{\theta}=0.26$ ) and 40

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<sup>7</sup> 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.

( $\hat{\theta}=0.64$ ) 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 behavior by institutions that have missed the 25 percent threshold for two consecutive years may be determined to get under the threshold in the third year. The ability for institutions to affect their third year CDR may be substantially impaired because of timing issues discussed above. Using McCrary density tests for just the subset of institutions that failed the 25 percent cutoff in two consecutive years, I still find no evidence that institutions are able to select themselves under the 25 ( $\hat{\theta}=0.17$ ) or 40 ( $\hat{\theta}=0.12$ ) percent thresholds.

#### IV.B. *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:

$$\begin{aligned}
 (5) \quad y_{it} = & \alpha_0 + \alpha_1 Ineligibility_{it} \\
 & + \sum_{\tau=0}^{\bar{\tau}} [\theta_{\tau} Over_{i,t-\tau}^* + f\{(1 - Over_{i,t-\tau}^*) \times \overline{CDR}_{i,t-\tau}^*, \gamma_{0\tau}\} \\
 & \quad + f\{Over_{i,t-\tau}^* \times (1 - Over_{i,t-\tau}^{**}) \times \overline{CDR}_{i,t-\tau}^*, \gamma_{1\tau}\} \\
 & \quad + f\{Over_{i,t-\tau}^{**} \times \overline{CDR}_{i,t-\tau}^{**}, \gamma_{2\tau}\}] \\
 & + \phi_1(Over_{i,t}^* \times Over_{i,t-1}^*) + \phi_2(Over_{i,t-1}^* \times Over_{i,t-2}^*) \\
 & + d_t + d_i + \kappa_{it} + u_{it}
 \end{aligned}$$

where I add to equation (4), indicators for the institution having a CDR over the  $c^*$  threshold in the current 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  $\phi_1$  and  $\phi_2$ .

The added terms in equation (5) distinguish institutions that have exceeded 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  $\phi_1$  and  $\phi_2$  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. Therefore, the expectations effect is  $\omega = [\theta_0 + \theta_1 + \theta_2 + \phi_1 + \phi_2]$  and the total effect including expectations is  $\alpha_1 + \omega$ . I test for the joint statistical significance of the total effect (i.e.,  $\alpha_1 = \theta_0 = \theta_1 = \theta_2 = \phi_1 = \phi_2 = 0$ ) and of just the expectations 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 posted publicly on an ED website, however, the extent to which students are aware of CDRs, their implications, or potential sanction at institutions is unclear.

## V. 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. 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, however, even when not compelled to do so, 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 default rates for all Title IV eligible schools and voluntary reporters spanning 1990 to 1999. 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 in that year, based on that cohort's student loan default performance of two years prior. In order to use information on CDRs for the two prior year in the DRD, I focus on outcomes from 1992-1999 for institutions that were potentially subject to sanction during the 1990-1999 time period. 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.<sup>8</sup>

In Table 2, I list summary statistics for institutions in the sample by detailed institution sector over the total time period. For-profit institutions with programs of two years or less comprise the largest group of sanctioned institutions, with more sanctioned schools than the other sectors combined. As such, the results from this study are largely applicable to this group of institutions. The analysis sample also includes a large number of sanctioned public 2-year colleges. Because these schools have relatively large enrollment, consequences at these schools may affect a substantial number of students. Few 4-year or private, not-for-profit institutions are subject to sanction during the analysis period. Minority students make up the largest proportion of student bodies at for-profit and private, not-for-profit less than 2-year institutions, with the student bodies at these schools being comprised of 42 to 41 percent minorities, on average. Public 2-year institutions have the highest proportion of part-time students, with over half of students attending part-time. In Table 3, I list the number of institutions in the analysis sample by sector from 1992 to 1999. I also display the number of institutions subject to Title IV loss in each year for either threshold.<sup>9</sup>

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<sup>8</sup> I test the sensitivity of this assumption and find similar results when excluding growth rates ranging from 500 to 1,500 percent.

<sup>9</sup> Schools that are subject to Title IV loss based on the one-year threshold may also be counted in the count of schools that lose Title IV eligibility based on violating the lower threshold for three consecutive years.

Certain characteristics of the data do not allow perfect assignment into Title IV eligible and ineligible groups. These characteristics will bias observed effects towards zero, such that the results based on this data may underestimate the true treatment effect and therefore represent a lower bound of the effects of losing Title IV eligibility on institution outcomes. 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 (described in more detail in the Appendix) 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 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 towards zero, I consider results derived from these data to be a lower bound.

## **VI. Results**

Before discussing results from estimations, I display in Figure 2 graphical depictions of the relationship between the running variable CDR and outcomes. I fit local linear regression estimates of outcomes on CDR, estimated separately for each range of  $CDR \in [0,25)$ ,  $CDR \in [25,40]$ , and  $CDR \in (40,100]$ . Note that these simple graphs provide an incomplete picture of

the effect of passing the 25 percent threshold since ineligibility is based on exceeding the cutoff for multiple years. There appear to be small discontinuities in enrollment for all students and first year students at the 25 percent cutoff, with variation in outcomes for institutions with CDR greater than 40 percent large making conclusions at this threshold less clear. There appears to be at best minimal graphical evidence for changes in discontinuity for the proportion of students who are minority or attend school part-time.

#### *VI.A. All Students, All Sectors*

Table 4 presents results from the estimates of the effect of Title IV loss on the various outcomes from OLS models with and without institution fixed effects. I find that institutions that are subject to Title IV ineligibility have almost 17 percent lower yearly fall enrollment than Title IV eligible institutions in the model with institution fixed effects (column 1, panel A). To provide a sense of relative enrollment size across sectors, during this time period, 4-year public institutions (the omitted group) have the largest average enrollment, with 2-year public institutions being closest in size and for-profit less than 2-year institutions being the smallest. The average annual enrollment of sanctioned institutions is approximately 410 students, such that a 17 percent decline in enrollment would translate to an average of 70 students not attending per school and year.

Enrollment results from the DRD model (Table 5, column 1, panel A) yield similar, but slightly lower, point estimates of the effect of Title IV loss (around 15 percent lower enrollment). Like the OLS model with fixed effects, this result is statistically significant at the 99 percent confidence level. In the models including expectations in Table 5, column 1, panel B, I find that the total effect of Title IV loss is statistically significant (using a Wald test), but do not find statistical significance when testing the joint significance of just the components of the expectations effect.

Turning to the effects of Title IV ineligibility on student body composition, I next examine results for the proportion of students identified as minority race or ethnicity in the second and third columns of Tables 4 and 5. In the OLS or DRD models with fixed effects, I find a consistent estimate that institutions subject to Title IV loss had slightly higher average proportions of minority students (about two percentage points higher). Neither of these results is statistically significant at conventional levels, but the results from the OLS model with fixed

effects (Table 4, column 2, panel A) is just below this level (t-statistic = 1.43). These findings may reflect the behavior of white students seeking institutions that offer federal financial aid, the ability for Title IV ineligible institutions to offer other incentives that are attractive to minority students, or relative differences in familiarity with financial aid across races and ethnicities.

I find some evidence that Title IV ineligibility affects the proportion of students attending school part-time, as displayed in column 3 of Tables 4 and 5. Results from the OLS model with fixed effects point to a slightly higher proportion of part-time students as a result of Title IV loss of about 3.5 percentage points, with the result statistically significant at the 95 percent confidence level. Public 2-year and less than 2-year institutions have the highest proportion of part-time students in the sample, and interestingly, private, not-for-profit and for-profit institutions have lower proportions of part-time students than public 4-year colleges. I do not find jointly or individually statistically significant expectations effects for proportion of minority or part-time students, though the expectations effects for part-time students are only marginally below traditional statistical significance levels.

#### *VI.B. 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 some evidence that students of different levels respond differently 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 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 certificates 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 dynamic RD model in Table 6. The enrollment effect for first year students is rather different than for continuing students. I find large and statistically significant negative effects of Title IV ineligibility on enrollment for first year students (column 1, Panels A and B), with sanctioned institutions experiencing a decline of approximately 25 percent. With an average annual first year enrollment of 260 at these sanctioned schools, a 25 percent decline in first year enrollment would translate to a per school and year average of 65 students not attending these schools. Conversely, I find no statistically significant effects for continuing students on enrollment growth (column 2, Panels A and B). These results suggest that institutions lose new enrollees because of Title IV loss, but do not experience an increase in transfers or dropouts 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.

The proportion of minority students after losing Title IV is slightly higher for first year students than for the pooled sample of students, with the point estimate statistically significant at the 86 percent confidence level (column 3, Panel A). When coupled with the large overall enrollment declines, this finding does not necessarily indicate that more minority students are attending sanctioned schools. Rather, as enrollment levels contract, white students could be fleeing at a higher rate, such that the remaining reduced student body becomes more heavily minority. Point estimates for the proportion of minority continuing year students are close to zero (column 4, Panels A and B).

With respect to the percent of students who are part-time, I do not find statistically significant results (and with relatively large standard errors), however, the point estimates for first year and more advanced students are both positive (columns 5 and 6, Panels A and B). Expectations also appear to influence first year student part-time enrollment, with joint significance p-values of 0.06 and 0.05 for the total effect and expectations effect of Title IV loss respectively.

### *VI.C. By Sector*

Because of potentially different outcomes across types of institutions, I also examine results separately by public, private, not-for-profit, and for-profit sector. Table 7 presents results of DRD estimates with institution fixed effects. I find that institutions that lose access to federal financial aid funds in all sectors have 10 to 21 percent lower overall enrollment levels than eligible institutions, though results are only statistically significant for the for-profit sector. The large effect for for-profit institutions may be because the higher tuition and fees typically charged by these schools make loss of aid more impactful. When considering just first year students, observed enrollment declines are higher, with both public institutions and for-profit institutions experiencing statistically significant declines after Title IV loss of 34 and 23 percent respectively. Some caution should be taken when interpreting results of Title IV loss by sector, especially for public and private, not-for-profit institutions, as these sectors have relatively fewer sanctioned institutions.

Estimates based on data pooled across all sectors provide little conclusive evidence on the effect of Title IV loss on the percent of part-time students at institutions. However, in the by-sector estimations I find evidence that being subject to Title IV loss results in an eight to 12 percentage point decline of the proportion of part-time students in private institutions. The result is statistically significant at the 90 percent confidence level for all students and at the 95 percent confidence level for first year students. This may suggest that students at private schools, faced with the prospect of reduced resources, may be switching to part-time status in order to work to pay for extra expenses or that part-time students rely less on financial aid than full-time students. I find little evidence that Title IV eligibility loss affects the proportion of the student body that is minority race/ethnicity differentially by sector.

## **VII. Summary and Conclusions**

This study is among the first to assess the causal impact of institution federal aid funding loss on aggregate student outcomes. Taking advantage of 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 percent in one year or 25 percent in three consecutive years, institutions are subject to loss of the ability to disburse federal

financial aid, such as Pell Grants and subsidized student loans. Since institutions local to the cutoff values cannot precisely control their assignment into the eligible (control) and ineligible (treatment) groups, local average treatment effects reasonably approximate randomization (Lee & 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 Title IV expenditures totaling almost \$200 billion during the 2009-2010 school year (Baum, Payea, & Cardenas-Elliott, 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. Ineligibility to provide Title IV funding because of former students' failure to repay student loans appears to effectively discourage enrollment, with lower overall annual enrollment of 15 percent and lower annual first year student enrollment of approximately 25 percent.

The efforts to strengthen program integrity, however, may have important implications for whether students attend higher education, and if so, where. For-profit institutions appear particularly hard hit by Title IV funding loss and this is a sector where popularity is growing rapidly (Baum, Little, & Payea, 2011; Baum & Payea, 2011; Deming, Goldin, & Katz, 2012). Financial aid disbursement ineligibility could result in lower matriculation for the many low-income, minority, and non-traditional students who attend these schools. On the other hand, concerns that some institutions, especially those on the margin of losing Title IV disbursement eligibility because of high CDRs, provide few skills or leave low-income students with overly burdensome debt may cast decreased enrollment at these schools in a favorable light.

Estimated enrollment declines, 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.

The ED's (2011a) regulatory impact analysis of the proposed gainful employment rule assumes that five to 42 percent of students will drop out of programs that fail debt measures and lose Title IV eligibility, depending on sector. Using these assumptions, loss of Title IV funds because of CDR restrictions results in many students forgoing postsecondary education, considering total enrollment for institutions subject to sanction because of Title IV debt requirements over the period of 1992-1999 in the sample was over 700,000.<sup>10</sup> The ED's (2011) assumptions also indicate, however, that a large number of students likely transferred to other institutions.

Evidence regarding claims about the disproportionate burden of Title IV loss on disadvantaged groups is equivocal. Enrollment appears to be most negatively affected at institutions that are likely to have high proportions of minority and non-traditional students. Interestingly, I find some weak evidence that the proportion of students who are minority race or ethnicity is on average larger at schools that lose Title IV eligibility. Because of the large overall decline in enrollment, this doesn't necessarily indicate that more minority students enroll at the institution. Rather, after enrollment contracts, minority students make up a larger proportion of the reduced student body.

The higher proportion of minorities after Title IV loss could be because non-minority students are more likely to seek other institutions with more available resources. 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, these students might be left behind in failing or 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.

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<sup>10</sup> To estimate total forgone education because of Title IV program integrity rules, one would also need to add decreased access at institutions that closed due to Title IV loss, but this information is not presently available.

The sizable enrollment loss and risk that minority students may be less likely to eschew poorly functioning schools indicates that careful evaluation is needed when considering future (and assessing already implemented) policies that experiment with or limit Title IV eligibility at institutions. The results suggest that program integrity rulemaking could be in conflict with other stated federal goals, such as improving higher education access through the expansion of the Pell Grant program. In contrast, the reduced access may be considered acceptable or preferable if schools that fail student loan debt measures truly provide little in return for the human capital investment. Evaluation of the costs and benefits of program integrity rules requires additional research on many key issues for which little conclusive evidence exist. Future 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 evaluate policies addressing student loan defaults at institutions.

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**Table 1: Title IV Federal Student Aid, 2009-2010 Academic Year**

Program	Total Aid (\$MM)	10-year Change in Total Aid
<b>Grants</b>		
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
<b>Loans</b>		
Perkins	\$1,106	-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)	\$5,934	309%
PLUS – Federal Family Education (FFELP)	\$8,231	195%
<b>Federal Work Study</b>	\$1,417	20%

*Source: Baum, Payea, & Cardenas-Elliott (2010). ACG and SMART grants started within the past 10 years and therefore 10 year change is not displayed.*

**Table 2: Sample Summary Statistics**

Detailed Sector	# of Institutions	# of Sanctioned Institutions	Average Total Enrollment	Average % Minority	Average % Part-Time
Public 4-yr	555	2	10,308	26%	29%
Private, Non-Profit, 4-yr	1,463	27	2,028	23%	25%
For-profit 4-yr	87	5	1,074	33%	20%
Public 2-yr	1,082	97	5,130	25%	56%
Private, Non-Profit, 2-yr	305	18	330	22%	18%
For-profit 2-yr	489	75	346	28%	14%
Public <2-yr	255	25	443	18%	28%
Private, Non-Profit, <2-yr	60	8	321	41%	10%
For-profit <2-yr	1,509	500	121	42%	20%

*Source: IPEDS and cohort default rate data.*

**Table 3: Number of Institutions Subject to Sanction in Sample**

Status	1992	1993	1994	1995	1996	1997	1998	1999
<i>Panel A: All Institutions</i>								
All Institutions	3,641	4,856	4,870	4,761	5,016	5,040	5,239	5,280
Subject to Title IV Loss (CDR >40%)	60	163	170	131	104	74	53	10
Subject to Title IV Loss (3X CDR >25%)	18	160	353	290	236	185	118	40
<i>Panel B: Public Institutions</i>								
All Institutions	1,567	1,575	1,572	1,538	1,700	1,729	1,801	1,841
Subject to Title IV Loss (CDR >40%)	25	15	17	13	8	11	7	0
Subject to Title IV Loss (3X CDR >25%)	7	10	33	24	23	18	15	4
<i>Panel C: Private, Non-Profit Institutions</i>								
All Institutions	1,619	1,673	1,673	1,638	1,650	1,626	1,667	1,663
Subject to Title IV Loss (CDR >40%)	12	15	12	12	8	10	9	3
Subject to Title IV Loss (3X CDR >25%)	3	8	16	11	10	9	11	5
<i>Panel D: For-Profit Institutions</i>								
All Institutions	455	1,608	1,625	1,585	1,666	1,685	1,771	1,776
Subject to Title IV Loss (CDR >40%)	23	133	141	106	88	53	37	7
Subject to Title IV Loss (3X CDR >25%)	8	142	304	255	203	158	92	31

*Notes: Schools that are subject to Title IV loss based on the one-year threshold may also be counted in the count of schools that lose Title IV eligibility based on violating the lower threshold for three consecutive years. Source: IPEDS and cohort default rate data.*

**Table 4: Ordinary Least Squares Estimates of the Effect of Title IV Loss**

	ln(Enrollment)	% Minority	% Part-time
	(1)	(2)	(3)
<i>Panel A: With Fixed Effects</i>			
Subject to Title IV Loss	-0.167*** (0.049)	0.020 (0.014)	0.035** (0.016)
<i>Panel B: Without Fixed Effects</i>			
Subject to Title IV Loss	-0.029 (0.057)	0.265*** (0.025)	-0.016 (0.019)
Private 4-yr	-0.712*** (0.052)	-0.006 (0.013)	-0.008 (0.010)
For-profit 4-yr	-1.075*** (0.104)	0.051** (0.021)	-0.057*** (0.021)
Public 2-yr	-0.062 (0.042)	-0.027** (0.012)	0.291*** (0.009)
Private 2-yr	-2.377*** (0.091)	-0.001 (0.016)	-0.070*** (0.016)
For-profit 2-yr	-2.010*** (0.065)	0.054*** (0.014)	-0.100*** (0.012)
Public <2-yr	-2.230*** (0.101)	-0.028* (0.017)	0.041* (0.022)
Private <2-yr	-2.864*** (0.154)	0.145*** (0.040)	-0.146*** (0.026)
For-profit <2-yr	-2.977*** (0.060)	0.100*** (0.014)	-0.042*** (0.011)
Observations	38,680	38,354	38,703
Number of Institutions	5,799	5,792	5,805

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses.*

*\*Significant at 10%, \*\* significant at 5%, \*\*\*significant at 1%. Models include institution fixed effects and controls for year and years since sanction, but coefficients are not displayed. Source: IPEDS and cohort default rate data.*

**Table 5: Dynamic Regression Discontinuity Estimates of the Effect of Title IV Loss**

	ln(Enrollment)	% Minority	% Part-time
	(1)	(2)	(3)
<i>Panel A: Actual Loss</i>			
Subject to Title IV Loss	-0.148*** (0.053)	0.015 (0.015)	0.010 (0.017)
<i>Panel B: Loss including Expectations</i>			
Subject to Title IV Loss	-0.145*** (0.054)	0.014 (0.015)	0.009 (0.017)
CDR <sub>t</sub> > 25%	-0.026 (0.026)	-0.004 (0.007)	0.017* (0.009)
CDR <sub>t-1</sub> > 25%	-0.025 (0.023)	-0.008 (0.006)	-0.004 (0.007)
CDR <sub>t-2</sub> > 25%	0.004 (0.020)	-0.004 (0.005)	-0.011* (0.006)
CDR <sub>t-1</sub> > 25% & CDR <sub>t-2</sub> > 25%	-0.009 (0.023)	0.010 (0.006)	0.003 (0.007)
CDR <sub>t</sub> > 25% & CDR <sub>t-1</sub> > 25%	0.023 (0.021)	-0.001 (0.006)	-0.008 (0.007)
Joint significance test, Total Effect	0.07	0.44	0.17
Joint significance test, Expectations Effect	0.59	0.51	0.13
Observations	36,258	35,970	36,279
Number of Institutions	4,812	4,817	4,817

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses.*

*\*Significant at 10%, \*\* significant at 5%, \*\*\*significant at 1%. Models include third order polynomials, institution fixed effects, and controls for year and years since sanction, but coefficients are not displayed. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.*

**Table 6: Dynamic Regression Discontinuity Estimates of the Effect of Title IV Loss, 1st Year vs. Continuing Students**

	ln(Enrollment)		% Minority		% Part-Time	
	1st Year Students	Continuing Students	1st Year Students	Continuing Students	1st Year Students	Continuing Students
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Actual Loss</i>						
Subject to Title IV Loss	-0.253*** (0.078)	-0.005 (0.128)	0.022 (0.015)	-0.003 (0.028)	0.019 (0.017)	0.010 (0.028)
<i>Panel B: Loss including Expectations</i>						
Subject to Title IV Loss	-0.251*** (0.078)	-0.001 (0.128)	0.021 (0.015)	-0.003 (0.028)	0.018 (0.017)	0.010 (0.027)
CDR <sub>t-1</sub> > 25%	-0.052* (0.030)	0.015 (0.052)	-0.005 (0.007)	0.002 (0.010)	-0.003 (0.008)	0.015 (0.012)
CDR <sub>t-2</sub> > 25%	-0.002 (0.026)	0.031 (0.046)	-0.003 (0.006)	-0.002 (0.009)	-0.011* (0.007)	-0.005 (0.011)
CDR <sub>t-1</sub> > 25% & CDR <sub>t-2</sub> > 25%	0.003 (0.029)	-0.039 (0.056)	0.009 (0.006)	-0.004 (0.012)	0.001 (0.008)	0.001 (0.013)
CDR <sub>t</sub> > 25% & CDR <sub>t-1</sub> > 25%	0.022 (0.028)	-0.011 (0.050)	0.000 (0.006)	-0.007 (0.010)	-0.008 (0.008)	-0.002 (0.013)
Joint significance test, Total Effect	0.03	0.96	0.50	0.98	0.06	0.83
Joint significance test, Expectations Effect	0.56	0.92	0.76	0.96	0.05	0.73
Observations	35,846	28,676	35,562	28,646	35,901	28,766
Number of Institutions	4,763	3,629	4,771	3,637	4,771	3,637

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. \*Significant at 10%, \*\* significant at 5%, \*\*\*significant at 1%. Models include third order polynomials, institution fixed effects, and controls for year and years since sanction, but coefficients are not displayed. Joint significance test displays the two-sided Wald test p-value. Source: IPEDS and cohort default rate data.*

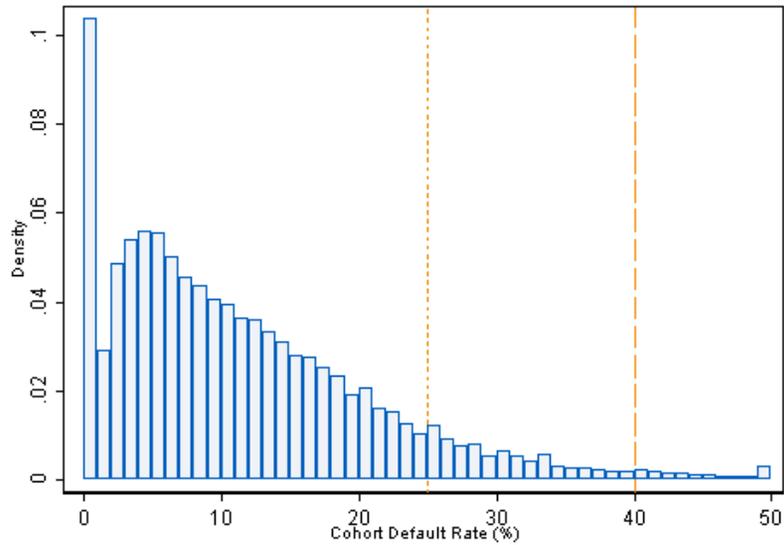
**Table 7: Dynamic Regression Discontinuity Estimates of the Effect of Title IV Loss, By Sector**

	All Students			1st Year Students			Continuing Students		
	<i>Public</i>	<i>Private, Non- Profit</i>	<i>For-Profit</i>	<i>Public</i>	<i>Private, Non- Profit</i>	<i>For-Profit</i>	<i>Public</i>	<i>Private, Non- Profit</i>	<i>For- Profit</i>
<i>Panel A: ln(Enrollment)</i>									
Subject to Title IV Loss	-0.097 (0.093)	-0.153 (0.182)	-0.208*** (0.076)	-0.343** (0.156)	-0.214 (0.282)	-0.227** (0.104)	0.098 (0.139)	-0.200 (0.220)	-0.196 (0.306)
Observations	12,610	12,804	10,844	12,526	12,585	10,735	11,836	12,147	4,693
Number of Institutions	1,578	1,665	1,569	1,565	1,645	1,553	1,469	1,562	598
<i>Panel B: Percent Minority</i>									
Subject to Title IV Loss	0.015 (0.013)	0.000 (0.057)	-0.004 (0.022)	0.018 (0.015)	0.011 (0.060)	0.003 (0.022)	0.019 (0.012)	-0.006 (0.064)	-0.066 (0.077)
Observations	12,600	12,778	10,592	12,529	12,564	10,469	11,854	12,147	4,645
Number of Institutions	1,570	1,647	1,554	1,470	1,564	1,554	1,580	1,667	603
<i>Panel C: Percent Part Time</i>									
Subject to Title IV Loss	0.005 (0.028)	0.084* (0.048)	-0.005 (0.022)	0.006 (0.030)	0.123** (0.055)	-0.001 (0.023)	0.002 (0.033)	0.019 (0.063)	-0.005 (0.062)
Observations	12,614	12,812	10,853	12,546	12,601	10,754	11,856	12,168	4,742
Number of Institutions	1,570	1,647	1,554	1,470	1,564	1,554	1,580	1,667	603

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. \*Significant at 10%, \*\* significant at 5%, \*\*\*significant at 1%. Models include third order polynomials, institution fixed effects, and controls for year and years since sanction, but coefficients are not displayed. Source: IPEDS and cohort default rate data.*

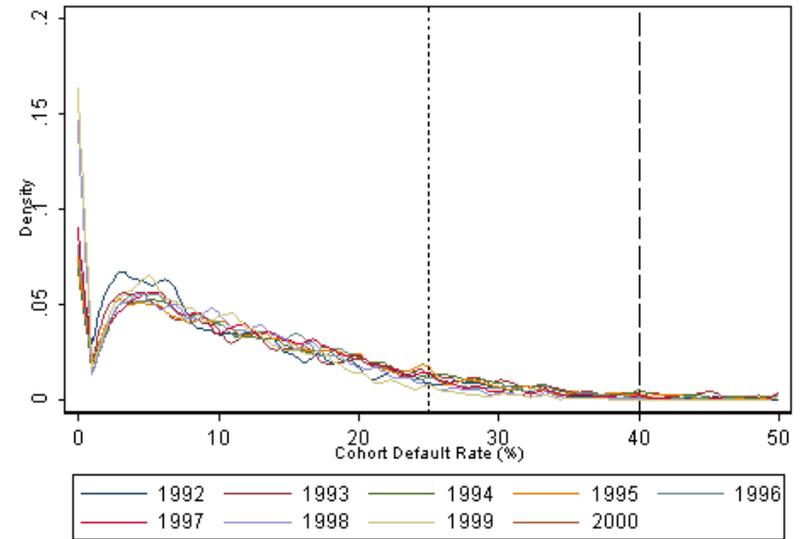
**Figure 1: Cohort Default Rate Distribution**

**(A) All Years (N = 38,703)**



*Notes: Histogram uses one percentage point bins. Default rate data censored at 50 percent. Source: cohort default rate data.*

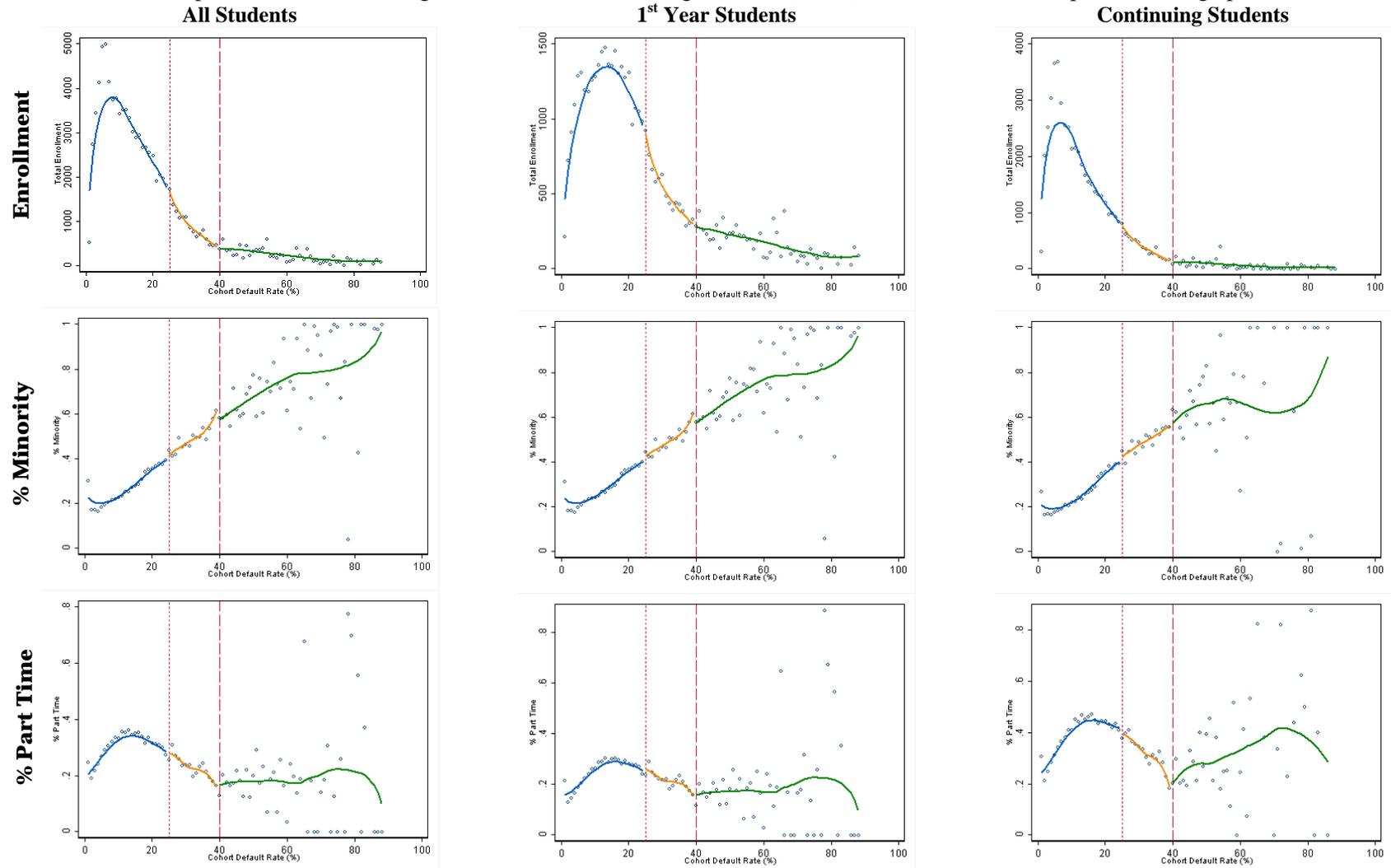
**(B) By Year**



*Notes: Kernel density plots by year using a triangular kernel and a bandwidth of one percentage point. Default rate data censored at 50 percent. Source: cohort default rate data.*

**Figure 2: Graphical Discontinuities: Enrollment, % Minority, % Part Time**

Note: Solid lines represent a local linear regression for each of the ranges [0,25), [25,40], (40,100]. Markers represent average per CDR bin.



**Appendix Table A1: DRD Estimates of the Effect of Title IV Loss on ln(Enrollment)**

Bandwidth:	1.00	0.50	0.25	0.10	0.06	0.04
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All Students</i>						
<i>Polynomial of order:</i>						
Zero	-0.154 (0.053)	-0.156 (0.053)	-0.101 (0.059)	-0.130 (0.092)	-0.051 (0.122)	-0.010 (0.194)
One	-0.152 (0.054)	-0.155 (0.054)	-0.099 (0.059)	-0.127 (0.092)	-0.050 (0.122)	-0.011 (0.194)
Two	-0.148 (0.054)	-0.154 (0.053)	-0.097 (0.059)	-0.126 (0.092)	-0.050 (0.122)	-0.012 (0.193)
Three	-0.148 (0.053)	-0.153 (0.054)	-0.097 (0.059)	-0.127 (0.092)	-0.051 (0.121)	-0.013 (0.193)
Four	-0.146 (0.053)	-0.151 (0.053)	-0.097 (0.059)	-0.126 (0.092)	-0.051 (0.121)	-0.014 (0.194)
Five	-0.146 (0.053)	-0.151 (0.053)	-0.098 (0.059)	-0.126 (0.092)	-0.050 (0.122)	-0.013 (0.194)
Observations	36,258	36,211	33,278	9,590	5,128	3,293
Number of Institutions	4,812	4,808	4,443	1,472	861	568
<i>Panel B: 1st Year Students</i>						
<i>Polynomial of order:</i>						
Zero	-0.255 (0.077)	-0.257 (0.077)	-0.255 (0.098)	-0.232 (0.145)	-0.144 (0.208)	-0.073 (0.234)
One	-0.254 (0.078)	-0.260 (0.079)	-0.251 (0.098)	-0.232 (0.145)	-0.145 (0.209)	-0.077 (0.233)
Two	-0.253 (0.078)	-0.260 (0.079)	-0.247 (0.098)	-0.231 (0.145)	-0.144 (0.209)	-0.082 (0.230)
Three	-0.253 (0.078)	-0.260 (0.079)	-0.248 (0.098)	-0.232 (0.145)	-0.146 (0.207)	-0.084 (0.230)
Four	-0.252 (0.078)	-0.259 (0.079)	-0.248 (0.098)	-0.232 (0.145)	-0.146 (0.207)	-0.086 (0.230)
Five	-0.252 (0.078)	-0.258 (0.079)	-0.249 (0.098)	-0.231 (0.145)	-0.146 (0.207)	-0.083 (0.231)
Observations	35,846	35,799	33,006	9,512	5,086	3,263
Number of Institutions	4,763	4,759	4,407	1,457	854	562

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include institution fixed effects and controls for year and years since sanction, but coefficients are not displayed. Third order polynomials are used for lagged relative distance terms. Source: IPEDS and cohort default rate data.*

**Appendix Table A2: DRD Estimates of the Effect of Title IV Loss on % Minority**

	Bandwidth:	1.00	0.50	0.25	0.10	0.06	0.04
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All Students</i>							
<i>Polynomial of order:</i>							
Zero		0.014 (0.014)	0.014 (0.014)	0.028 (0.015)	-0.004 (0.024)	-0.030 (0.047)	0.007 (0.038)
One		0.015 (0.015)	0.014 (0.015)	0.027 (0.015)	-0.005 (0.024)	-0.029 (0.047)	0.009 (0.039)
Two		0.015 (0.015)	0.014 (0.015)	0.027 (0.015)	-0.005 (0.024)	-0.029 (0.047)	0.008 (0.039)
Three		0.015 (0.015)	0.015 (0.015)	0.027 (0.015)	-0.004 (0.024)	-0.029 (0.047)	0.008 (0.039)
Four		0.015 (0.015)	0.015 (0.015)	0.027 (0.015)	-0.005 (0.024)	-0.029 (0.047)	0.007 (0.039)
Five		0.022 (0.015)	0.015 (0.015)	0.028 (0.015)	-0.005 (0.024)	-0.029 (0.046)	0.007 (0.039)
Observations		35,970	35,923	32,997	9,455	5,048	3,231
Number of Institutions		4,817	4,813	4,448	1,474	863	568
<i>Panel B: 1st Year Students</i>							
<i>Polynomial of order:</i>							
Zero		0.021 (0.015)	0.022 (0.015)	0.040 (0.016)	0.010 (0.026)	-0.017 (0.048)	0.018 (0.041)
One		0.022 (0.015)	0.022 (0.015)	0.039 (0.016)	0.009 (0.026)	-0.016 (0.049)	0.019 (0.041)
Two		0.022 (0.015)	0.022 (0.015)	0.039 (0.016)	0.009 (0.026)	-0.016 (0.049)	0.019 (0.041)
Three		0.022 (0.015)	0.022 (0.015)	0.040 (0.016)	0.009 (0.026)	-0.016 (0.049)	0.018 (0.041)
Four		0.022 (0.015)	0.022 (0.015)	0.040 (0.016)	0.009 (0.026)	-0.016 (0.048)	0.017 (0.041)
Five		0.022 (0.015)	0.022 (0.015)	0.040 (0.016)	0.009 (0.026)	-0.016 (0.048)	0.017 (0.041)
Observations		35,562	35,515	32,734	9,378	5,007	3,201
Number of Institutions		4,771	4,767	4,415	1,461	857	562

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include institution fixed effects and controls for year and years since sanction, but coefficients are not displayed. Third order polynomials are used for lagged relative distance terms. Source: IPEDS and cohort default rate data.*

**Appendix Table A3: DRD Estimates of the Effect of Title IV Loss on % Part-Time**

	Bandwidth:	1.00	0.50	0.25	0.10	0.06	0.04
		(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All Students</i>							
<i>Polynomial of order:</i>							
Zero		0.013 (0.016)	0.011 (0.017)	0.014 (0.015)	-0.013 (0.021)	-0.016 (0.031)	-0.010 (0.048)
One		0.010 (0.017)	0.008 (0.017)	0.010 (0.015)	-0.015 (0.021)	-0.019 (0.031)	-0.012 (0.048)
Two		0.010 (0.017)	0.008 (0.017)	0.009 (0.015)	-0.015 (0.021)	-0.020 (0.031)	-0.012 (0.048)
Three		0.010 (0.017)	0.008 (0.017)	0.009 (0.015)	-0.015 (0.021)	-0.020 (0.031)	-0.012 (0.048)
Four		0.010 (0.017)	0.008 (0.017)	0.009 (0.015)	-0.015 (0.021)	-0.020 (0.031)	-0.012 (0.048)
Five		0.019 (0.017)	0.008 (0.017)	0.009 (0.015)	-0.015 (0.021)	-0.020 (0.031)	-0.012 (0.048)
Observations		36,279	36,232	33,293	9,598	5,132	3,295
Number of Institutions		4,817	4,813	4,448	1,474	863	568
<i>Panel B: 1st Year Students</i>							
<i>Polynomial of order:</i>							
Zero		0.025 (0.017)	0.022 (0.017)	0.014 (0.016)	-0.014 (0.022)	-0.038 (0.034)	-0.038 (0.051)
One		0.021 (0.017)	0.018 (0.017)	0.011 (0.016)	-0.015 (0.022)	-0.041 (0.035)	-0.039 (0.051)
Two		0.020 (0.017)	0.018 (0.017)	0.010 (0.016)	-0.015 (0.022)	-0.042 (0.034)	-0.039 (0.051)
Three		0.019 (0.017)	0.017 (0.017)	0.009 (0.016)	-0.016 (0.022)	-0.041 (0.034)	-0.039 (0.052)
Four		0.019 (0.017)	0.017 (0.017)	0.009 (0.016)	-0.016 (0.022)	-0.041 (0.034)	-0.039 (0.051)
Five		0.019 (0.017)	0.017 (0.017)	0.009 (0.016)	-0.016 (0.022)	-0.042 (0.034)	-0.039 (0.051)
Observations		35,901	35,854	33,054	9,529	5,095	3,267
Number of Institutions		4,771	4,767	4,415	1,461	857	562

*Notes: Heteroskedasticity robust standard errors are clustered by institution and included in parentheses. Models include institution fixed effects and controls for year and years since sanction, but coefficients are not displayed. Third order polynomials are used for lagged relative distance terms. Source: IPEDS and cohort default rate data.*

## **Appendix B: Other Reasons for Title IV Eligibility Loss**

### *(A) Voluntary withdrawal from the program*

Institutions may have various motivations to withdraw from the Title IV program or not pursue eligibility. They may not want to submit to the numerous requirements associated with the program (discussed in more detail below) because of the costs or scrutiny associated with compliance. The requirements, moreover, may not coincide with the institution's strategic and administrative plans. For example, most predominantly online schools were granted eligibility for Title IV programs only in 2006 and schools with alternative schedules may not meet credit hour or instructional time requirements. As well, some institutions may not be concerned with attracting students who need financial aid funds, perhaps because their target student body has independent access to financial resources, they reside in a locale with ample non-federal financial assistance, or because they are able to attract students in absence of federal financial funds.

### *(B) Change in status of the school*

If schools have a significant change in status, such as closure, merging with a different institution, or an ownership change, they lose eligibility for Title IV programs. During the years 2000-2005, the largest reason for loss of Title IV eligibility was change in status (Congressional Research Service, 2007). Effects of Title IV loss are difficult to estimate for these types of institutions, as they will no longer report data to IPEDS under their new organizational structure or their operations will likely be substantially different than from before the status change.

### *(C) Failure to meet program requirements*

In order for their students to be eligible to receive Title IV resources, each institution must satisfy three broad groups of requirements: attain licensing from the state in which it is located, gain accreditation by an agency authorized by the ED, and prove compliance with a number of provisions "protecting the administrative capacity and fiscal integrity of its funding programs" (Congressional Research Service, 2007, pp. 1-2). Among administrative capacity conditions are requirements that institutions have adequate staff to support Title IV aid programs, provide all prospective and enrolled students financial aid counseling, and monitor the academic progress of Title IV recipients. Financial responsibilities currently dictate that institutions cannot derive more than 90 percent of revenues from Title IV funds. They must maintain sufficient cash reserves to repay Title IV funds, and prove solvency. Other requirements cover diverse topics

such as alcohol, drug use, and campus security policies. Additionally, institutions are compelled to report various data to the ED, though these requirements have changed over time.

*(D) Federal policy changes*

Federal policy changes may also affect Title IV eligibility. The ED has implemented various rule changes, as well as tested various programs, that can affect Title IV eligibility. The recently instituted gainful employment rule is one such example. Targeting for-profit institutions, this rule adds two debt measure requirements for eligibility to disburse Title IV funds that go beyond just student loan default rates: repayment rate and debt-to-earnings ratio. The repayment rate is generally calculated as the proportion of original loan balance that remains unpaid over time. The debt-to-earnings ratio is broadly defined as the annual loan payment for each individual divided by annual earnings. Programs with students that fail both of these requirements for three out of four fiscal years lose Title IV eligibility.

Another example of federal policy changes are related to the status of online programs. Congress enacted the “50 percent rule” in 1992 that rendered a school ineligible if it offered more than 50 percent of courses by correspondence or over 50 percent of its students participated in correspondence courses. The 50 percent rule was enacted, in large part, response to concerns about “diploma mills” that appropriate financial aid funds rather than educate students. This somewhat correlates to the current debate concerning for-profit schools.

In response to concerns about the rule limiting access to postsecondary education, the ED initiated the Distance Education Demonstration Program (DEDP) in 1999 that temporarily allowed a few participants to disburse federal Title IV funds to students even if they crossed the 50 percent correspondence course threshold. Due in part to the success of the DEDP, the 50 percent rule was rescinded in 2006 as part of the Higher Education Reconciliation Act of 2005.

Other rulemaking includes a “one day rule” implemented by the ED in 1994 that mandated that Title IV eligible institutions have an academic year consisting of at least 30 weeks with a minimum of one day of organized academic activity. The ED initially allowed alternatively structured institutions to follow a “12-hour rule,” where 12 hours of scheduled educational activities signified an academic week. The ED, however, revised regulations in 2003 such that all schools must comply with the “one day rule” rather than the “12 hour rule.”